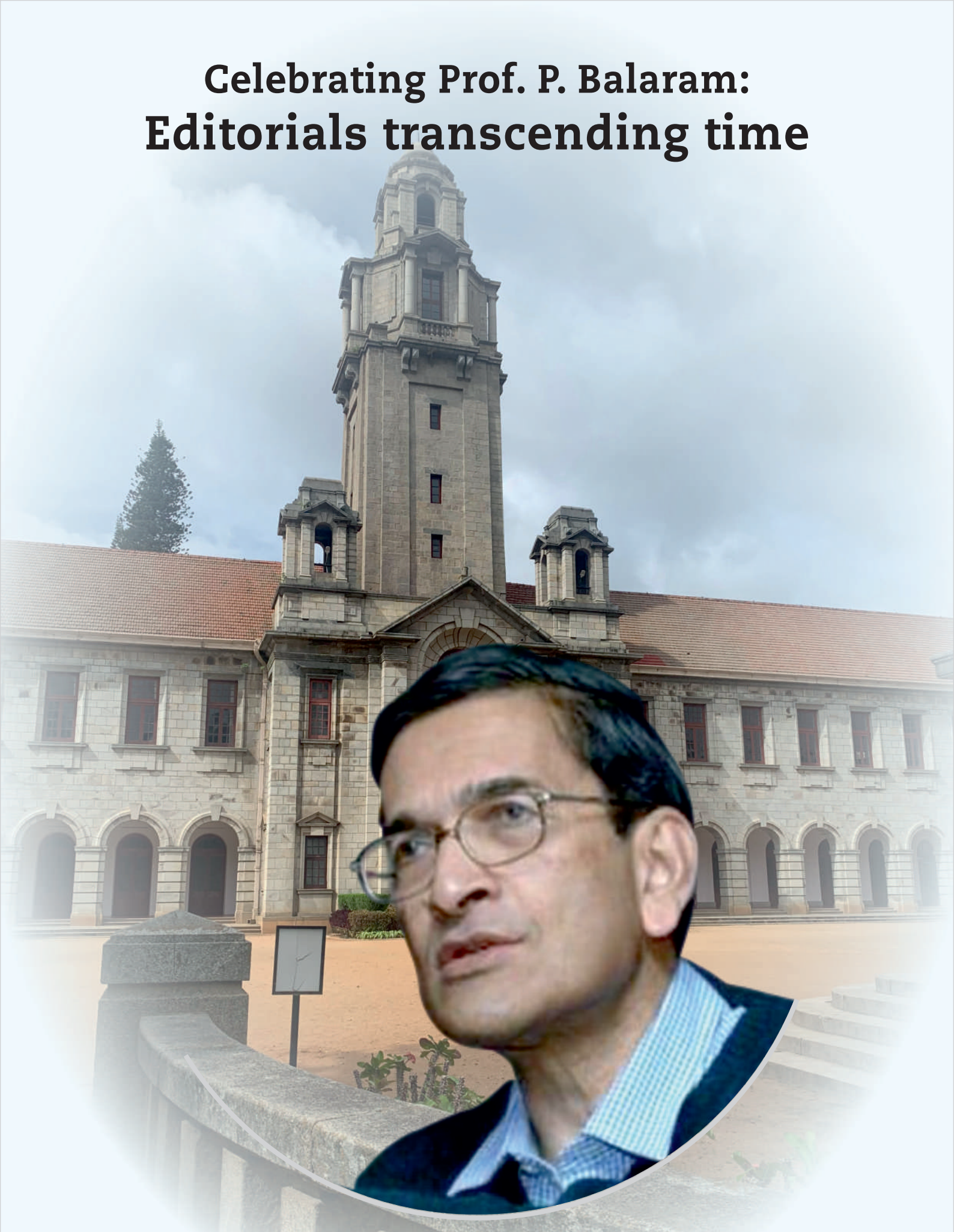
Celebrating Prof. P. Balaram:
Editorials transcending time



(A Collection of Prof. P. Balaram's Editorials published in Current Science)

Celebrating Prof. P. Balaram: Editorials transcending time

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Compiled by Prof. Avinash Bajaj
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"The editorials of Current Science that Professor Balaram wrote epitomise his rare ability to be able to say the right thing that needs to be said, yet without giving offence, month after month, year after year, across the entire gamut of science and society."

Satyajit Rath
Professor Emeritus, IISER Pune

"The thought-provoking style, the incisive perspectives, the wonderful balance, the scholarship, the high literary value, and the tongue-in-cheek wit all combined to make it a treat to read."

Anand K. Bachhawat
IISER Mohali
Curr. Sci. 2013, *105* (4), 429.

"Prof. Balaram's editorials were written more as dialogues with the readers. While a subtle sense of humour was evident in all his writings, they made readers to take serious note of the content. Often elicited discussions across the world. A notable example is his editorials on the journal impact factors and their fallacies."

L. S. Shashidhara
NCBS, Bengaluru

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Compiled by Prof. Avinash Bajaj

Published by: Regional Centre for Biotechnology, Faridabad

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Foreword

I am delighted that a collection of Professor P. Balaram's insightful editorials in *Current Science* is being published. Balaram is one of the Indians I most admire, for his scholarship, his commitment to institution-building, and his upright character. Though not a scientist myself I have had a long-standing connection to the world of Indian science. My father and grandfather both studied at the Indian Institute of Science; an uncle was one of India's foremost chemists, and a female cousin was one of India's foremost nutrition scientists. Therefore, when I moved to Bangalore in the late 1990s, I took out a subscription to *Current Science*, where I was enchanted and educated by the editorials written by Professor Balaram.

In time, I got to know Balaram in person, meeting him on my frequent visits to the IISc campus, where I did collaborative research with its ecologists while also serving several stints as a Visiting Professor. I was thus able to observe at first-hand how Balaram did to the IISc what he had already done to *Current Science*; namely, nurtured and developed the institution further while never departing from the core values of the leaders who had preceded him. Apart from encouraging research in new areas of science, Balaram made two fundamental innovations that greatly enriched the Institute. One is well-known; the undergraduate programme that Balaram initiated and many of whose products have gone on to become outstanding research scientists themselves. The other, of particular interest to a historian like myself, is Balaram's founding of an IISc archive, so that the key documents pertaining to the Institute's history could be preserved and used for research.

Apart from one-to-one conversations over coffee and lunch, I have also served on several selection committees with Balaram, through which my admiration for him has been even further enhanced. His range of intellectual interests is formidable; among Indian scientists, perhaps only Satish Dhawan and Obaid Siddiqi, both now deceased, matched him in this regard. His polymathic mind is complemented by his immaculate judgement, his instinctive fairness, and his utter human decency. To know Balaram, to consider him a friend, and above all to have had the opportunity to be inspired and illuminated by his intellect and his institutional vision, has been one of the great privileges of my life.

Ramachandra Guha
Distinguished University Professor, Krea University
https://ramachandraguha.in

Preamble

Let me start with the history of Current Science, an Indian Flagship journal published by the Indian Academy of Sciences. It commenced its journey in 1932, shortly after Dr. C. V. Raman won the Nobel Prize for physics and was endorsed by esteemed scientific personalities like Dr. Birbal Sahni and Dr. S.S. Bhatnagar. The initial editors of this journal were Dr. C. R. Narayana Rao, a Professor at Central College, Bangalore, and Dr. V. Subrahmanyam, a Professor at the Indian Institute of Science. In its first two decades, the journal earned a distinguished reputation, but in the following years, it slowly started declining until the 1980s. Its revival started when Dr. S. Ramaseshan, an eminent Indian scientist and crystallographer, assumed the role of an editor. Owing to his zeal, he encouraged younger scientists from all over India to contribute to Current Science, which provided new life to the journal.

Prof. Padmanabhan Balaram assumed the role of editor of Current Science in 1995. Building upon the foundation laid by Dr. S. Ramaseshan, he ensured the growth of the scientific stature of the journal as well as its impact nationally and globally. Undoubtedly, the journal soared to new heights, leaving a lasting impression on the scientific landscape. It would not be an exaggeration to state that Current Science garnered subscriptions owing to Prof. Balaram's editorials. These articles were not only thought-provoking and informative in scientific matters but also delved into non-scientific yet relevant topics, including awards and associated politics, biases against women scientists, critical discoveries, etc.

His editorials exhibited wisdom, precision, and a diverse range of subjects. They were truly a feast to read. Needless to say, this command was reflected in each article. His stimulating style, insightful viewpoints, sense of balance and depth, and sarcastic humour made his editorials a joy to read. Proficient in linguistics, he presented his opinions with finesse. It is essential to add here that during his tenure with the journal, he generously accommodated diverse criticisms on policies and concerns affecting the progress of Indian Science. Addressing Prof. Balram as an unparalleled and vital personality would not be an overstatement, given his valued contributions.

Join us in delving into the pages of Prof. Balaram’s editorials. This compilation not only serves as a testament to his enduring legacy but also as a valuable resource for the future generation of scientists and researchers. The wisdom embedded in these editorials is genuinely enlightening.

Arvind Sahu
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Prologue

It was during the lockdown of COVID pandemic that I came across one of the Editorials written by Prof. P. Balaram. By the time I finished reading it I was so intrigued that I started collecting and reading his other editorials. While reading these editorials, I realized that the issues raised by Prof. Balaram more than a decade back are still very relevant and yet to be addressed; be it advocating for funding basic science, supporting women in science, the true value of awards, plagiarism, issues on climate science, antimicrobial resistance, viruses and pandemic, and inadequacies of the impact of journal impact factors to name some. I was impressed and motivated by the fact that for every relevant topic that he chose fortnightly, he did an in-depth research and came up with a scholarly view with proper citations! I also learnt about his love for sports including cricket from his writings, and he beautifully blended examples and anecdotes from sports events to justify his opinion on a particular editorial piece. "Cancer Research: Collaboration and Collegiality" is one of my favourite from this collection, where he stressed that "only a broad interdisciplinary assault on the many problems of biomedical research can lead to results that can be effective in improving health care."

As I read and even re-read these editorials, I felt the need to bring them together as a collection so that they may be easily accessible to the younger generations across disciplines. Therefore, I would like to thank Prof. Balaram and Current Science journal for permitting me to compile them as a collection. I am grateful to Dr. Ramachandra Guha for writing the foreword. I would also thank Prof. Arvind Sahu, Executive Director, RCB for the penning the preamble to this collection. Finally, this collection would have been incomplete without Prof. Samrat Mukhopadhyay's introduction and Prof. L. S. Shashidhara and Prof. Anand Bachhawat's tribute. Last but not the least, I thank Devashish Mehta, Nishant Pandey, and Kajal Rana for their help.

I hope you will enjoy reading this compilation

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Introducing Prof. P. Balaram

It is a formidable task to write a short piece about Prof. P. Balaram. He is an iconic and towering figure in Indian science. When Dr. Avinash Bajaj asked me to write this piece on Prof. Balaram and his Current Science editorial pieces, I was truly delighted and honored. I have been inspired and influenced by Prof. Balaram from my student days at the Indian Institute of Science (IISc) Bangalore (I started my research career as a summer intern in 1998 under his tutelage). His vast and versatile scholarship, mastery of peptide and protein chemistry, insights into science history and philosophy, and unmatchable clarity in thinking, writing, and speaking have always fascinated me. Balaram received his B.Sc. in chemistry honors from Fergusson College, University of Pune (1967) followed by his M.Sc. from the Indian Institute of Technology (IIT) Kanpur (1969).

He went on to do his Ph.D. in the United States and worked with Aksel Bothner-By at Carnegie Mellon University, Pittsburgh, USA. During his Ph.D. he was involved in developing the negative nuclear Overhuaser effect (NOE) as a unique probe to study macromolecular structure. His Ph.D. publications that appeared in the *Journal of the American Chemical Society* in 1972 introduced the concept of the negative NOE in proteins. Then he had a short postdoctoral stint with Nobel Laureate Robert Woodward at Harvard University. During his postdoc, Balaram was a part of the epic total synthesis of vitamin B12 and was also involved in the asymmetric total synthesis of antibiotic erythromycin. He returned to India in 1973 and joined the faculty of the Molecular Biophysics Unit (MBU) at IISc – a center at IISc that was just created by G.N. Ramachandran, a pioneer in the study of peptide conformations.

Balaram remained on the faculty of IISc until his retirement in 2014. He rose through the academic ranks and went on to become the director of IISc in 2005. Balaram's early independent research involved lipid membranes, bile acids, and alamethicin, a transmembrane channel-forming fungal peptide antibiotic. His interest in alamethicin gave rise to beautiful work on the conformation of an unusual nonstandard amino acid called α-methylalanine or α-aminoisobutyric acid (Aib) and its role in the formation of 3_{10}-helices in Aib-containing peptides. His work also demonstrated the role of D-proline in the β-hairpin formation. His research interest then moved into proteins, especially utilizing mass spectrometric characterizations. During his long and illustrious scientific career, he trained a large number of students who went on to become very successful independent faculty members across the globe.

His prodigious scientific contribution has been widely recognized in India and abroad. The most recent one was the R. Bruce Merrifield Award from the American Peptide Society. He is currently a Chair Professor at the National Centre for Biological Sciences (NCBS), Bangalore. During his tenure as the Chief Editor of *Current Science* (the flagship journal of the Indian Academy of Sciences, Bangalore) for over a decade and a half, he wrote influential, incisive, thought-provoking, unputdownable editorial pieces that addressed some of the most important contemporary issues that are still relevant. I remember, during my Ph.D. days in the early 2000s, I (and many of my friends) used to voraciously read his editorial pieces in the IISc library. Often, I had to wait for others to finish reading. On many occasions, I went to the IISc library only to read his latest editorial.

After he stepped down from the stewardship of *Current Science* with a signing-off note in 2013, I thought the response from the Indian scientific community was almost equivalent to the response Sir Arthur Conan Doyle received for *The Final Problem* in 1893, except that Conan Doyle was persuaded to revive Sherlock Holmes after 10 years in *The Return of Sherlock Holmes* in 1903. Balaram's editorial articles beautifully captured the academic issues of yesteryear and are still equally relevant and important in the Indian academic landscape. The vast scholarship, critical assessments, historical perspectives, and high literary value embodied in these editorial articles make them timeless treatises. I am extremely delighted to learn that Avinash has made this effort to assemble all the editorial articles written by Balaram. This collection will be of great importance to the students and early-career scientists of the current and future academic generations.

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Celebrating Prof. P. Balaram: Editorials transcending time

(A Collection of Prof. P. Balaram's Editorials published in Current Science)

Compiled by Prof. Avinash Bajaj
Published by: Regional Centre for Biotechnology, Faridabad

GUEST EDITORIAL

We publish below articles dealing with a proposal for a National Science University. We invite readers to send in their views on this important issue.

—Editor

Indian science

P. Balaram

It has never been easy to practise good science in India and no change appears to be in sight. Four and a half decades after Independence, we are now in the process of reexamining our economic policies and are counting the toll of years of profligate existence, by a succession of governments. With tightening budgets, eroding political support and mounting costs of research, the Indian scientific community is heading towards a state of siege. Indeed, the alarm signals have already sounded in the upper echelons of the country's science managers, who have for years been finely tuned to the varying nuances of political and bureaucratic patronage. For too long, Indian science has basked in the warm glow of Prime Ministerial approbation, most often based on personal equations with individual members of the scientific establishment, rather than on a critical and pragmatic assessment of the achievements and capabilities of our scientists and their institutions. All good things must come to an end so it appears that the honeymoon between the government and the scientific community is over. The immediate future may be characterized by 'benign indifference' (to quote C. N R. Rao, *Curr. Sci.*, 1992, **505**) although there is every possibility that this could degenerate into 'benign neglect' (to borrow a memorable phrase from Daniel Moynihan).

Ironically, even as Indian scientific institutions struggle hard to stay afloat, the most serious consideration is being given to a non-resident Indian proposal for a new National Science University, with a special status and an outright government grant of Rs 200 crores. *Current Science* reproduces in this issue sections of this proposal, a concept paper on the new University and commentaries on this idea. The fact that this extraordinarily naive proposal, completely divorced from ground realities in India has merited favourable attention is a telling commentary on our scientific and political judgement. Nevertheless, the Mahajan proposal is welcome in many ways. It focuses attention on the state of science in our own country. The raison d'etre for the entry of the NRI white knights is the continuing 'low quality of research' in India.

The balance sheet of organized post-independence science makes mixed reading. The apparently conspicuous successes are oft cited – the atomic energy and space programs, the 'green revolution' in agriculture, areas which have been characterized by the adaptation and successful implementation of technologies already tested elsewhere. The failures have been emphasized less, at least publicly. The poor Indian presence in all areas of basic research and the limited, almost negligible impact of our basic science on a world wide level, is a matter of serious concern. Our reluctance to purposefully address pressing problems of biomedical research, which have a bearing on diseases specific to our country and to bring the full power of modern biology to bear on these problems are matters which merit concern. Our inability to clearly identify goals and pursue them single mindedly, and indeed, ruthlessly, has led to the widespread perception that the ship of Indian science is floundering and rudderless. Scientific research and even the teaching of science in our Universities is at a low ebb, for want of both financial and moral support. Indeed, nothing short of a 'Marshall Plan' will resurrect our higher education system from the years of havoc caused by unrestrained political interference. Our string of national laboratories appears to be strapped not only of financial resources but also of original ideas. In most institutions a holding action seems to be in progress to stem the pace of deterioration. Mindless construction of 'infrastructure' (most often, bricks and mortar) has now left us with more science mausoleums, than viable and active institutions. The time has come to put an end to the 'Shah Jehan syndrome' amongst our institution builders. Our collective reluctance to exercise rational judgement has proved as, if not more, costly in science as in other spheres of human activity. All of us would balk at the thought of a passenger being included in the Indian cricket team, although this has been known to happen. But how often has mediocrity been elevated and celebrated in our scientific establishment, a feature also seen in politics. Indian sports comes under public scrutiny at Olympics time. After a little wailing and breast beating we wait for the process to repeat itself on a quadrennial basis. We are more fortunate in science.

The present scenario is depressing not only because of governmental indifference to science, but as a consequence of the scientific community's inability to work collectively and purposefully towards common

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goals. The challenges of free markets, the growing cries for accountability and the breathtaking pace of advance in modern science, can hardly be met by trumpeting well-worn and often, bankrupt ideas or by philosophical resignation. It is clear that the basic structure of Indian science, built over several decades by many dedicated individuals must be preserved and protected against ill informed and short sighted attack by politicians and bureaucrats. However, the fat must be trimmed and the scientific enterprise in India must gear itself to meet an increasingly difficult future. While diminishing governmental support and the absence of any enthusiasm for research amongst the captains of industry are grim realities that must be addressed, the impending squeeze might afford a good opportunity for introspection. Has Indian science lived up to our collective expectations in the last thirty years or so? Is there a sound case for major financial inputs into specific areas of science and do we have a realistic hope of achieving hard results of practical value, in a reasonable time frame? Will present management practices in Indian science prove productive in the future? Has the quality of our basic research shown a dramatic upturn during the boom days of the 1980s? In the weeks and months ahead it is hoped that answers to these questions will be forthcoming from a wide cross-section of the scientific community. An informed debate, ironically catalysed by the National Science University proposal, can prove valuable in setting an agenda for the future. The time is ripe for a close look at the conduct of science in India.

CURRENT SCIENCE

Volume 68 Number 3 10 February 1995

EDITORIAL

Awakening at the Academy

Indian science academies have generally steered clear of any major involvement in influencing science policy in the country. Viewed from the outside, the Academies appear to be cosy clubs whose primary purpose is to perpetuate their Fellowship, with elections of new Fellows dominating the activities of each passing year. The organization of annual (and biannual) meetings and the publication of journals allow the academies a limited interaction with the rest of the scientific community and others interested in science. This limited portfolio of activities has often bothered many and the academies have been internally urged to take a more activist role in shaping the development of science in India.

Now in the sixtieth year of its existence the Indian Academy of Sciences has turned its attention to the problem of declining standards of science education in the country. The Academy's major concern is 'to promote progress in science to attain excellence'; the absence of a proper educational base would of course, be a major barrier in nurturing high quality scientific research. The Academy's interest in higher education in science has found expression in the first of the 'Academy Papers' reproduced in this issue of *Current Science*, with the intention of providing a wide readership for a document which touches upon many important issues.

There have been earlier reports that have had the sanction of Government, but the Academy exercise is a voluntary effort which will hopefully merit attention both in colleges and universities and in the bodies charged with countrywide responsibility for higher education. The issues confronted in the report are complex and wide ranging. The Academy panel has apparently addressed in its deliberation the thorny issue of 'equity and excellence', coming to the unsurprising conclusion that a debate on this issue 'should not be left merely to the politicians and judiciary'. The recommendations are clear and indeed the Academy proposes its own initiatives. However, many specific recommendations are unlikely to be easily implemented or even agreed upon. At times the report appears to have a touch of the ivory tower, but this is inescapable given the fact that many of our research institutions, which contribute heavily to the Fellowship of the Academy, have only a peripheral connection with the real-life problems of colleges and universities. The danger in such reports is that they may appear patronising to those directly charged with the responsibility of communicating science and its excitement to young minds. The Academy has nevertheless taken the plunge into the arena of public policy. Coming closely on the heels of the many public pronouncements by many of its more prominent Fellows on governmental indifference to science, the report signals the Academy's desire to be heard. Advice, both solicited and unsolicited, can often be ignored. The Academy will surely take this hazard in its stride.

P. BALARAM

CURRENT SCIENCE

Volume 70 Number 7 10 April 1996

EDITORIAL

Popularizing science

Every year, National Science Day is celebrated on February 28, marking the anniversary of the announcement of the discovery of the Raman effect in 1928. Customarily, public lectures are held all over the country on scientific topics, ostensibly with the aim of promoting a greater awareness of science amongst the general public. Indeed, in recent times, popularization of science has burgeoned into a flourishing (although heavily subsidized) industry. The science departments of the central government and the Science Academies vie with one another in promoting new schemes for the wide dissemination of science; some of which have proved reasonably popular. The state governments are not far behind. Fledgling state academies and state awards for popularization abound everywhere. The ever ubiquitous committees have sprouted all over; although generally moribund, these groups surface every year in February. Towards whom is this popularization fervour directed? The answers vary. To one school of thought, the targets of these popularization drives are the lay public, young and old in a continuing effort to spread 'scientific temper' (a phrase immortalized by Jawaharlal Nehru), amongst a populace beset by ancient traditions and superstitions. These drives, largely restricted to urban centres, have had a limited general impact. To a second school, the targets of popularization are entirely different. This group believes that diminishing interest in science as a career is resulting in a drying up of talent entering institutions of higher education. Postgraduate courses in the universities and Ph D programmes at our most prestigious institutions appear to have difficulty in maintaining even minimum standards for entry, with serious consequences for our research enterprise. The appropriate targets of science promotion drives according to this alternative school of thought would be high school and college students, in an attempt to stem the flow away from science courses.

Should there not be a third target – politicians, bureaucrats and industrialists who need to be enlisted in the cause of developing science in India into a flourishing and useful activity? The last few years have seen frozen budgets for scientific institutions, diminishing support for research projects in what appears to be a concerted move to divest government of its responsibility towards higher education in science and basic research. Ironically, this year Science Day coincided with the presentation of the budget (albeit interim) in Parliament. Who will lobby for the cause of science in India, wherever it matters? Should not the Academies and scientific societies focus on the deteriorating infrastructure for the practice of science and its dissemination? The infrequency of notable Indian contributions to world science, the failure of many large projects to deliver tangible results and the absence of credible peer review within the scientific community have undoubtedly influenced public perception. Reducing support for science will hardly solve the problems that we face. There is no doubt that science needs popularization. Unfortunately there seem to be a multiplicity of targets, while both gunners and ammunition appear limited.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

An uncertain future

Support for science, both financial and otherwise, is diminishing in India as government's perceptions about the role of the science and technology enterprise in future development, appear to have changed rather rapidly. Higher education in science is threatened by the prospect of institutions having to raise funds from private sources to meet essential running expenses, even as government slowly abdicates its responsibilities in this area. Research departments and institutions can hardly compete for private resources, with management and marketing institutes and colleges and universities distributing engineering and medical degrees. The heady days of enthusiastic government support for science appear to be over. Unfortunately, the rationale for changing policies is often obscure, although budgetary compulsions are always blamed for across the board, non-discriminatory cuts in all areas of governmental expenditure. The uncertainties of the new year, which coincides with the start of a new Five-Year Plan, put together by a disparate coalition of interests, cast a long shadow on the future of scientific institutions in this country. While the defence science, space and atomic energy establishments will undoubtedly come through unscathed because of their perceived strategic importance, the rest of India's science institutions face difficult times ahead.

In most places, frozen budgets over the last few years have seen an ever increasing proportion of the allocated funds (in some cases as high as 90%) going towards payment of staff salaries and allowances, leaving little or no institutional support for research activities. Individual researchers survive largely on sponsored projects, which are of course easier to obtain and operate in a relatively small number of high profile institutions. Even here the decline is clearly visible. Projects submitted to government agencies lie for increasing lengths of time, unattended. Even more depressingly, release of funds for sanctioned projects proceeds at a glacial pace. The ever present battle for turf, an enchanting pastime for bureaucrats in Delhi, appears to have tilted against the managers of the science departments. Procedural delays are now the rule rather than the exception. Ironically, liberalization is a word that seems absent in the lexicon of bureaucracy, both in Delhi and inside our institutions. The pity is that by slowing the process of procurement of equipment, materials and staff appointments, most projects eventually end up costing more. In recent times, there has been much comment about the gradual aging of the scientific work force. Limited recruitments over several years have resulted in a graying of our laboratories. What appears to have passed unnoticed is that aging is a phenomenon that has also caught up with the middle level management of our science ministries.

Funding and encouragement for science may be at a low ebb, but it is also true that many of our agencies and laboratories are largely devoid of good ideas. Collective enthusiasm, which facilitates problem solving, is conspicuous by its absence. Innovation is rare and hardly encouraged by an unresponsive and conservative environment. The scientific establishment today suffers from poor public relations; an inability to enhance the public (and political) perception of what science in India has achieved and even more importantly the crucial contributions that it will make in the future. The herbal petrol fiasco highlighted dramatically the public craving for quick, magical fixes to mounting problems. The faith in conventional science obviously, does not run too deep.

Most non-performing wings of government get a far larger share of the public cake than the science departments. Scientific institutions most often struggle with limited budgets and unrealistic expectations of what can be achieved. Science, technology and higher education need a broad based lobby, which will prevent skewed priorities and ill thought out development strategies from cutting at the roots of our scientific and technological development. It is also clear that scientific institutions must devise mechanisms to shed some fat. Leaner and fitter organizations are more likely to cope successfully with an uncertain future.

P. BALARAM

CURRENT SCIENCE

Volume 75 Number 2 25 July 1998

Editorial

Funding basic science

The new budget has been presented, the waves created by the nuclear explosions and counter explosions are subsiding into ripples (worries about sanctions having given way to concerns about visa denials), making the moment right for reflections on the state of funding for science in India. Increased budgets for 'strategic research', the absence of any new, major initiative for academic science and the complete identification, in the media and in political circles, of science with the activities of the defence, atomic energy and space establishments do not augur well for the future of basic science in universities and research institutions.

The concerns of individual scientists are often apparently very mundane, with worries about finances being primary. The task of keeping laboratories running in universities and research institutions, in an era of diminishing budgets and increasing costs, is daunting. Ironically, this process is being made even more difficult by the very government agencies, which are charged with the responsibility of promoting scientific research, on a broad platform across the country.

Individual investigator generated projects move through a labyrinthine process in Delhi, at a glacial pace. The decision-making process ('peer review') in most committees is not rate limiting, but every 'approved project' must negotiate a minefield of financial bureaucracy before investigators and their institutions finally receive the funds. Unfortunately, in most institutions, investigators face many internal obstacles in utilizing funds obtained with difficulty. The tyranny of capricious finance departments is something against which all but the politically powerful scientists are largely helpless.

There are, however, many issues at the front end of the funding process which may be addressable. Following Parkinson, committees spend disproportionate amounts of time debating the virtues (or otherwise) of minor projects, with the time spent often being inversely related to the money doled out. The large programmes to build new institutions, curiously, face the least stringent review. Even more disturbing is the appearance of an increasing number of 'politically correct' projects, which cannot but be funded, despite a woefully limited base of science. Can anyone say that they will not support projects that aim to develop vaccines against every known disease or that they balk at spending money on the development of new strains of crops that will resist every known pathogen and survive both floods and drought? 'Infrastructure', a once widely used word, has become unpopular with our science planners and financial managers, who control the purse strings of government. It is therefore difficult for institutions to obtain funds for upgradation and replacement of major research facilities; compelling many to resort to the subterfuge of camouflaging their essential requirements into 'mission mode' projects, which promise much but often deliver little. Sometimes research projects, multi-institutional and multi-investigator, are initiated at the instance of funding agencies, which organize 'brain storming' sessions in an effort to direct researchers to enter areas of specific relevance to local needs. Frequently, these areas, particularly in the field of biomedical research, have been investigated unsuccessfully abroad. The fond hope here is that we will succeed where others have failed. To borrow a pithy phrase, this approach often leads to 'indigenization of failure'. The funding mechanisms that support basic science in India urgently need some visible new initiatives which will restore and transform decaying and 'dying' laboratories in academic institutions. 'Strategic science' cannot be practised in isolation by a few laboratories sheltered within the cocoon of the defence and atomic energy networks.

P. Balaram

CURRENT SCIENCE

Volume 75 Number 3 10 August 1998

EDITORIAL

Citation counting and impact factors

Citation counts and journal impact factors appear to be gaining importance in discussions on science and scientists, in committee rooms across the country. The statistics that spew out from the Institute of Scientific Information (ironically abbreviated as ISI) in Philadelphia are mesmerizing; which scientist after all can resist being influenced by rankings of scientists, papers, journals and institutions. Despite the many warnings that have appeared in the literature, citation counting is becoming a critical factor in evaluating science. When time, money and sometimes, desire, are not available to obtain the citation counts on individual papers, the more insidious journal impact factors suffice. The reasoning here is simple and compelling; if a paper is published in a 'high impact' journal then there is a greater probability that it is significantly more important than a similar article in a 'low impact' journal. This argument, of course, ignores the many uncontrolled parameters that eventually determine where a paper is published. To many from the 'less developed' countries the growing influence of the Science Citation Index (SCI) in evaluating science and scientists is disturbing. After all the SCI covers relatively few journals from the poorer countries (only a dozen from India), leading to the labelling of all work published in non-SCI journals as 'lost science'.

Indian journals, including those indexed in the SCI, have great difficulty in attracting the best of papers emanating from this country. This is unsurprising, in view of their relatively low visibility ('low impact'). We face a piquant situation here. Unless our journals publish the best of our research, their impact factors are unlikely to improve. Unfortunately, unless their impact (in SCI terms) improves, they are incapable of netting the most desirable fish. There is also the constant complaint that even the best work from India, published in highly visible journals, attracts less attention than similar work done in the West. The hard fact is that science is a harshly competitive international game, where the rules are written elsewhere. Many extremist solutions have been proposed to deal with the declining quality of our journals. Closing them down (and sentencing their editors to hard labour) is one. At the other extreme, suggestions have sometimes been made that punitive measures must be introduced, compelling our researchers to publish in Indian journals. (This would be akin to travelling by Air-India, when the bills are paid by the government.) Somewhere, in between, probably lies a reasonable course of action. The tendency of committees to applaud publications in international journals (whatever be their repute) is often pointed out as a major factor inhibiting the best of our researchers from patronizing our own journals, at least on occasion. Unfortunately, even initiation of discussions on the state of our journals is likely to attract sharp comments. In our exuberant post-nuclear phase, criticism in scientific circles appears more likely to be decried as 'negativism' and sometimes viewed as an exercise in 'self flagellation'. While no dramatic measures appear visible on the horizon to alter the trend of sinking impact factors, citation counts and even total numbers of papers published (SCI journals, of course), the vast majority of uncited and poorly cited scientists may take heart from the molecular biologist, Sydney Brenner's characterization of citation counting as a 'pseudoscience'. Indeed, finding out how many people have cited a paper is a poor substitute for reading it oneself. In evaluation of colleagues, journals and institutions in our country, personal knowledge (and prejudices) may still turn out to be a better indicator than the raw statistics of the ISI. In assessing scientists there is little doubt that very highly cited researchers have had a major influence on their disciplines. Unfortunately, most of the time we compare citation counts that are so low that it becomes difficult to distinguish the signal from the noise. The ISI churns out an unending stream of facts and figures. It appears important to have access to these facts, so that we may, each in our own way, distort them. We would also do well to remember Brenner's mild admonition 'that citation often tells us more about the sociology of science than about the science itself.' The SCI is undoubtedly an invaluable tool in scholarly attempts at mapping the development of science. It may be a less reliable weapon in the hard task of assessing scientific performance in diverse environments.

P. Balaram

CURRENT SCIENCE

Volume 75 Number 4 25 August 1998

EDITORIAL

Animals in research

The dramatic progress of biomedical science over the past century has relied heavily on the use of experimental animals. While mice, rats, rabbits, hamsters and guinea pigs have been the most widely-used animals in biological research, primates have most often been a key element in clinical and behavioural studies. The excesses of scientific and technological development have resulted in a backlash, that has spawned many activist groups. In recent times, the cries of 'animal rights groups' have been heard clearly across the world, leading to increasing concern on the issue of the use of animals in scientific research. A great deal of biological research that needlessly uses animals is pedestrian and dispensable. However, there is a considerable area of medical research, where the judicious use of animals is the key to scientific and clinical success. Many of these areas hold great promise for the alleviation of human disease. It is, therefore, critical that we weigh the merits of the case, for the use of animals in research, carefully and dispassionately.

In the last few weeks, a set of rules, drafted by a governmental group, which goes by the Orwellian name of 'Committee for the Purpose of Controlling and Supervising Experiments on Animals' (CPCSEA), has disturbed an important section of the biological research community. This Committee, headed by Ms Maneka Gandhi, presently Minister of State for Welfare, has now decided to impose drastic curbs on the use of animals in scientific research and has assumed the power to decide all details on the course of animal-based experiments in future. If the guidelines are accepted by the Government, much of biological research in India will come to a standstill, as scientists grapple with a mountain of red tape, which will quickly be erected by an ever-eager bureaucracy. The Committee has chosen to ignore the fact that most major biomedical institutions already have in place 'ethics committees' and mechanisms which regulate research with experimental animals. Indeed, the Indian National Science Academy has produced and circulated guidelines for the use of animals in experimental research. Interestingly, the report (excerpts of which are reproduced on **page 346**) also avoids defining the biological term 'animal', while going on to precisely define the term 'committee'. While monkeys, dogs, cats, goats and rabbits may have been uppermost in the Committee's concerns, the rules might just as well apply to drosophila, the fruit flies so beloved amongst molecular biologists. The proposed rules inhibit the transfer of animals and possibly even the import of special strains of laboratory animals. Coming at a time when the use of transgenic animals and specifically inbred strains is assuming special importance in biological research, such regulations are likely to become a major impediment in the future.

Curiously, the heads of major scientific agencies and institutions, charged with the conduct of biomedical research, were indeed members of the committee, which drafted the new rules. Did they not realize that the biomedical research community, which is their major constituency, will be completely hamstrung by the new diktat? Maybe the only lesson that might be learned is well known; that committees can often be railroaded by chairpersons, who have 'not so hidden agendas' to pursue. On reflection, institutions and researchers, engaged in biomedical science, might do well to reexamine their own procedures for regulating animal-based research. Strict self-policing might be the best way to avoid governmental initiatives, like the CPCSEA. Maintaining animal houses, which follow strict guidelines is a difficult undertaking and sitting in judgement on a colleague's request for experimental animals may be embarassing. However, in order to avoid imposition of the rules formulated by CPCSEA, biomedical institutions and agencies would do well to reformulate their policy towards animal experimentation and provide a credible alternative.

P. Balaram

CURRENT SCIENCE

Volume 75 Number 5 10 September 1998

EDITORIAL

Staying home

Much discussion has been generated by the denial of visas to prominent members of the atomic energy establishment and the decision of the US Department of Energy (which runs the American nuclear programme) to send home a few Indian scientists working at their institutions. A section of our scientists and politicians seem to have been particularly incensed by this knee jerk reaction to the nuclear bomb tests. The fact that Germany and the UK may have followed suit, in blacklisting Indian nuclear scientists, appears to have sent a shiver down the spines of many, for whom the practice of science necessarily involves extensive travelling abroad. The strident criticism that has followed the visa denials invariably points to the importance of unrestricted travel by scientists to conferences (a principle repeatedly, but selectively, stressed in the past by the International Council of Scientific Unions). But in the cacophony of opinions that have been expressed, some questions do not seem to have been asked loudly enough. How important is foreign travel to the practice of science in India? Is it really necessary or useful for some scientists to always be abroad, attending one conference after another? Must important scientists, engaged in critical research in establishment like defence and atomic energy, have to travel abroad to attend conferences and meetings, to which researchers in academic institutions have little chance of going? Do important government scientists necessarily have to wear two hats – one covering their primary responsibility, while the other provides academic respectability?

Young researchers who attend international scientific congresses are usually inspired and enthused by the quality of work presented. In smaller meetings and workshops they are often exposed to new concepts and techniques and often, more importantly, a different style of doing science. This exposure frequently reveals, with alarming clarity, the vast difference in the nature of research done at home and in the advanced countries. Nevertheless, the realization that a gulf exists is very important in shaping attitudes towards research. Serious scientists with established research programmes also benefit from 'sabbaticals', which often bring them back home in an excited state, from which most relax rather rapidly. 'Senior' researchers also use international conferences to enhance their 'academic contacts' and to bring themselves closer to recent developments. Ironically, many of the 'regulars' on the conference circuit do little to keep themselves abreast, when at home. Continual visits abroad often hid an inability to formulate and develop research problems at home. Unfortunately, government agencies cater to this affliction by providing travel support. The plethora of agreements with foreign governments now provides a range of binational programmes, which are another source of easy travel money. Surprisingly, this urge for travel to conferences abroad persists despite the recent candid confession by a prominent scientist: 'Even if I am invited to deliver a lecture at an international research conference, I am made to feel like an outsider or am aware that I have been invited to satisfy a condition that someone from a developing country be included for the conference...' (Padmanaban, G., *Science*, 1998, **281**, 75).

The visa denials do not seem to have affected the majority of Indian students, whose career goal is often to settle into suitable positions in the West. The great Indian middle class dream of emigrating to America, Europe or even Australia does not seem to be threatened by the nuclear aftermath. Only a relatively small section of Indian scientists seems to be affected. However, their positions and prominence have ensured a public impression that travel restrictions will have an important impact, suggesting that the inability of a few to travel to countries of their choice is a major issue. The scientific community in India is large and a substantial (but silent) majority does not travel frequently abroad. In many instances visa restrictions may indeed be a blessing in disguise; forcing some researchers to stay where they are, compelling them to focus their energies on their own problems, instead of perennially moving around the globe. Collaborations and transfer of materials and ideas between academic laboratories around the world still remain largely unrestricted by governments; personal chemistry is often the only impediment. Modern communications ensure that face to face meetings are often unnecessary. It is time to make a virtue of staying at home.

P. Balaram

CURRENT SCIENCE

Volume 75 Number 6 25 September 1998

EDITORIAL

Research and teaching

Research and teaching are two sides of the scientific coin. In Universities and Centres which award doctoral degrees, it is generally assumed that research is an important and probably major component of the activities of science departments. In colleges and institutions, whose primary focus is science education, bachelors and masters degrees are the prize for successfully completing academic requirements. Here, of course, teaching is the dominant, and sometimes only, activity of the faculty. The importance of combining research with teaching (and presumably teaching with research) has been widely appreciated. The American university system has admirably integrated research and teaching. Many institutions have vigorous undergraduate teaching programmes, within the framework of highly visible and accomplished research departments. A considerable section of the faculties in US universities manage to combine a reasonable teaching load, with the running of productive research laboratories. Students thus, get exposed to a taste of scientific research very early in their development, attracting many highly talented and original students to scientific careers. The interaction of undergraduate students with research laboratories sometimes brings into over-specialized research groups a much-needed infusion of enthusiasm, untainted by too much formal knowledge. Of late, the growth of the research enterprise and the operation of an elaborate system of grants and rewards, has resulted in some American universities slowly accumulating an increasing number of highly acclaimed researchers, who bring in considerable sums of money by way of grants and overheads to their institutions, but who contribute relatively little to the teaching programmes. This, inevitably, results in the slow polarization of some departments into distinct faculty groups; those who shoulder the burden of teaching and others who occupy the high ground of research. Even as the best of US universities come to grips with problems created by the shifting balance in science departments, we might do well to reflect on the Indian scene.

The steady decline of research in University science departments in India and the transfer of undergraduate and sometimes even postgraduate teaching to autonomous colleges, has left many departments with minimal research and teaching commitments. Many, once famous departments, are mere shells of their former selves; a sad commentary on our times. In the colleges, teaching is a central activity, leaving little time for even modest attempts to develop research programmes. Motivated teachers can hardly be expected to sustain their interest in research, in a generally unresponsive environment, created by conservative managements and colleagues who often actively discourage any attempts to introduce the culture of research into colleges. The faculty improvement programme (FIP) devised by the University Grants Commission, to permit college teachers to acquire doctoral degrees by working at research institutions was not a great success; often the Ph D degree was really a passport to break free of the college environment. The growing gulf between research and teaching must be a matter of serious concern to most of our advanced research institutes, which depend exclusively on the University system for their new recruits. At some institutions like the Indian Institutes of Technology and the Indian Institute of Science, attempts have been made to start integrated programmes, targetting students who wish to acquire M Sc and Ph D degrees, by permitting entry after the high school and B Sc levels. The 'catch them young' strategy is based on the premise that an early exposure to a research environment would eventually provide a much higher calibre of students entering our laboratories. Sometimes, these programmes have required a high level of teaching commitment from a predominantly research-oriented faculty, often reluctant to forsake the unstructured freedom of their laboratories for the more regimented environment of the classroom. A great deal of research in India is carried out in national laboratories, where formal teaching duties are almost nonexistent. A large number of Ph D students at these places emerge with a remarkably limited view of science. There is little doubt that teaching (and teaching conscientiously) can bring great academic benefit to a teacher, interested in research. In turn, exposure to research may indeed introduce a more useful perspecive and instil greater confidence in the undergraduate teacher. Caltech of the middle years of this century may serve as a shining example of the highpoint of the fusion between research and teaching, with generations of undergraduates learning physics from Richard Feynman and chemistry from Linus Pauling. While such inspirational standards are not easy to set, we might wonder what happened to the once-important centres of both research and teaching in India – the Universities at Calcutta, Madras, Delhi and Benares. Many recent proposals to restore the importance of fostering undergraduate education in a research environment, toy with the idea of creating new institutions. Few will deny that a seamless fusion of teaching and research, is a consummation devoutly to be wished. The harsh realities are otherwise.

P. Balaram

CURRENT SCIENCE

Volume 75 Number 7 10 October 1998

EDITORIAL

Scientific collaboration

Most research papers in the scientific literature today are multi-authored, with authors being drawn from different laboratories, institutions, countries and continents. As research becomes an increasingly complex exercise, purposeful attack on many scientific problems requires collaborative efforts, with the participants bringing diverse skills and techniques to bear on the issue being addressed. In some fields, large collaborative research efforts are the norm, with experimental particle physics providing an extreme example. In modern biology too, large teams have become common; areas like genome sequencing rival the mega projects in physics for the level of coordinated effort (and funding) involved. Even when we move away from the realm of 'big science', collaborations are common. Indeed, the number of papers featuring one or two authors is rapidly diminishing in most fields.

Collaboration, of course, brings in its wake a host of problems that did not exist at a time when science was simpler. How is the credit for a scientific contribution to be shared? Whose name should appear first on the author's list in a published paper? These are mundane questions; but many collaborations end on a bitter note when the work is finished and the authors begin to write their papers for eventual publication. Human nature being what it is, heartburn is almost always inevitable. It is therefore not surprising to find footnotes in the best of journals which say '... these authors contributed equally to this work', sometimes leaving the reader to wonder what share could be attributed to the remaining authors. The importance of credit sharing increases dramatically with the perceived importance of the work. After all the Nobel Prize cannot be shared by more than three persons. In many areas distinct groups compete for the prizes and each group has but one or two visible leaders.

The problem of credit sharing inexorably percolates down, even to the bottom of the scientific pyramid. How does one compare a fresh Ph D applicant for a postdoctoral fellowship, who has a dozen publications each sporting half a dozen authors, with one who has a couple of publications authored only with his or her Ph D supervisor. Unfortunately, assessments of scientific capabilities must often be made with little data to judge the candidates, other than their lists of publications. In the extraordinarily competitive environment that we find ourselves in, the number of authors on a paper becomes yet another parameter in a complex 'evaluation equation', which includes journal impact factors and citation counts.

In the context of science in India, the problems of scientific collaborations assume new dimensions. Here, collaborations between different research laboratories are relatively uncommon, although the necessity of using diverse techniques is slowly forcing interactions between normally insular groups. Interactions with laboratories abroad are also rare, with very few long term arrangements, where both sides make nearly equal intellectual and material contributions. Many collaborative programmes are holdovers from postdoctoral periods; major interactions occur only when the Indian partner visits the foreign laboratory. The local perceptions of such collaborations are varied. There is a strong 'swadeshi' school which decries all ties with overseas scientists. At the other end of the spectrum are the 'internationalists', who forcefully argue that science transcends geographical and political boundaries. Normally, this polarization would only be a matter of mild academic interest. Instead, it has become a contentious issue in most committees which decide awards and rewards across the country. Should there be a special 'handicap' (in golf parlance) for those who forswear collaboration – both foreign and Indian? Even when collaborative efforts are between Indian laboratories, many committees seem uncomfortable in decisively giving credit where it is due. Matters are not helped by the relatively low credibility of most decision-making groups involved in the rewards process.

Scientific collaboration has come to stay. In order to move purposefully towards the solution of important problems, we must create a climate where collaborations are encouraged. Large areas of science require broad interdisciplinary efforts, which can hardly be fostered in an academic setting, where the most raucous debates involve issues of credit sharing. As a first measure, committees charged with the responsibility of evaluating science and scientists must apply their mind to the problem of ensuring fair assessments. The results of scientific collaborations must not be summarily discarded, because evaluation committees find this approach expedient. Building bridges between laboratories in India will promote the sharing of scarce resources of equipment and materials. Cooperation may indeed be a painless and pleasant way of enhancing the quality of our science.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Classifying life

Observation and classification are at the heart of science. Biology is a pre-eminent example of a discipline where classification is a central theme, conjuring up visions of Charles Darwin meticulously documenting his observations in the Galapagos islands. Darwin's grand synthesis, of course, stands as one of the most remarkable intellectual achievements of the millennium. Chemistry too is science where cataloguing and classification have been key elements for progress. Mendeleev and his periodic table, the cornerstone of chemistry, are a constant reminder that gathering and sifting through scattered and disparate facts can often provide intuitive insights that herald a revolution. Physicists have generally looked down upon the 'classifiers'. Gathering facts without a clearly defined predetermined purpose, by observation and painful experiment, can be an intellectually sterile exercise. Ernest Rutherford was at his dismissive best when he said that there are two kinds of science – physics and stamp collecting. But, the physicists too have been furtive classifiers, accumulating evidence for an increasing number of subatomic particles whose final purpose will be revealed when we have a 'grand theory of everything'. However, it is in biology, even more than philately that collection and cataloguing assume critical importance. Biodiversity, a common buzzword nowadays, clearly underscores the view that variability of the species is biology's dominant theme.

The remarkable progress of molecular biology over the last few decades, has pushed classical biology to a corner, where taxonomists and microbiologists (who can identify and classify organisms) are rapidly becoming extinct. Gene cloning and manipulation have become so widespread that classicism may be as rare in biology today, as in music or literature. Ironically, the growth of modern techniques of nucleic acid analysis provide a new opportunity for the revival of taxonomy, now clothed in the garb of comparitive genomics. Evolutionary connections between organisms have, in recent years, been traced by molecular relationships, which appear to provide firmer clues to common ancestral origins, than the apparently gross morphological approach of classical biology.

At the heart of biological classification is the imperative to group organisms into classes (families). After all, are we not interested in the relationships between mice, monkeys and men? Biology becomes a great leveller when we consider common origins; imagined evolutionary superiority quickly evaporates when confronted with molecular genealogies. The sharp distinctions between plants and animals are exemplified by the sometimes unbridgeable gap between botany and zoology departments. Microorganisms, generally unseen, occupy a more shadowy position. Fungi, originally studied in botany departments are now thought to be closer to animals than plants. A more universally appealing classification of organisms is based on whether their cells are ennucleated or not, leading to the two kingdoms of eukaryotes and prokaryotes. The latter of course contain the entire world of bacteria. The discovery of bacteria which live in hot sulfur springs at high temperatures in inhospitable environments about twenty years ago – named archaebacteria by Carl Woese – marked a turning point in classification. Woese proposed that the 'tree of life' possessed a third branch, which he christened Archaea. Ever since biological classification has been the centre of major controversy. Is life best described by two or three kingdoms?

Ernst Mayr, arguably one of the most influential figures in comparative biology, has fired the most recent salvo in the battle to decide whether there are 'two empires or three'. In a provocatively written article (*Proc. Natl. Acad. Sci. USA*, 1998, 95, 9720-9723), Mayr examines the systems of ordering in biology and asks – '*What is the objective of classification?*' In Mayr's view, '*a classification is an information storage and retrieval system*', which permits an item to be located with a minimum of effort and loss of time. Librarians who preside over chaotic libraries would do well to note Mayr's dictum that what '*is true for a classification of books in a library*' is true '*for taxa of organisms*'. He

argues that '*one of the basic principles of good classification is the principle of balance, which states that retrieval of information is greatly facilitated if the taxa at a given categorical rank are, as far as possible, of equal size and degree of diversity*'. It is this principle of balance that appears violated in Woese's proposal of a three kingdom classification of life. To Mayr, 175 different archaebacterial groups and approximately 10,000 named eubacteria appear dwarfed by the number of species of eukaryotes; 30 million in his estimation. Insect lovers (and haters) would of course testify that biological diversity is at its best in the insect world – reminding us of Haldane's remark that God loves beetles. Woese's classification of the living world into three major kingdoms was to a great extent based on the small though important part of the genome that archaebacteria share with eukaryotes. But to Mayr, this argument is less than compelling. He points to the fact that this similarity extends to only a tiny fraction of the eukaryotic genome. In Mayr's view, the most wonderful characteristics of the eukaryotic genome include '*not only the genetic program for the nucleus and mitosis, but the capacity for sexual reproduction and meiosis, and ability to produce the wonderful organic diversity represented by jellyfish, butterflies, dinosaurs, hummingbirds, yeasts, giant kelp and giant sequoias*'. How then can we claim that the difference between the two kinds of bacteria (eubacteria and archaebacteria) '*is of the same weight as the difference between prokaryotes and the extraordinary world of eukaryotes*'. Mayr concludes by emphatically asserting that only a '*two empire classification correctly reflects the structure of the living world*'. The intense debate on classification and the growing interest in biodiversity suggests that taxonomy may indeed return to centre stage, even in this molecular age.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Reviewing a review

'*Science in India today is characterized by a pervasive mediocrity; lack of originality and of the courage to challenge absurdity and defend truth; widespread plagiarism and the denial of credit where it is due; fear of excellence; complacency; self glorification; cronyism of every description; careerism without commitment; lack of professional, social or financial accountability; ignorance of scientific methods . . .; fraud, dishonesty and lack of integrity; sycophancy and crisis of leadership . . . .*' This endless list of charges forms the basis of an extraordinarily harsh assessment of science in this country, that appeared in the 17 September 1998 issue of the British journal, *Nature* (395, 233–234). The author, Pushpa M. Bhargava, former Director of the Centre for Cellular and Molecular Biology, Hyderabad and a prominent biochemist used the occasion of a review of a book entitled *Nonsense in Indian Science* by Dilip Salwi, to launch a wide ranging indictment of the Indian science establishment. To an unfamiliar reader (and I suspect the *Nature* review targets a particularly susceptible section of readers abroad) it would appear that science in India is really in a parlous state, ruled by a 'mafia' completely lacking in intellectual and personal integrity. If one takes Bhargava literally, then plagiarism is the order of the day, science managers border on the scientifically illiterate ('*they hardly ever read serious science*') and the Academies elect to their fellowship scientists '*who do not have even an independent paper representing their work in a journal indexed in Current Contents*' (Alas, Bhargava too is in the grip of ISI, Philadelphia). Bhargava's ire is also directed at all three of our national science academies (Indian National Science Academy, Delhi, Indian Academy of Sciences, Bangalore and the National Academy of Sciences, Allahabad), from all of which he resigned some years ago.

How much truth is there in Bhargava's scathing attack? Is it merely the case of difficulties in adjusting to an uncomfortable situation, where an once influential scientist finds himself on the sidelines (a not uncommon phenomenon in other spheres of activity). Indeed there are other scientists in retirement, who suddenly discover that after their departure from centre stage, all is crumbling. They fall prey to the all too common, 'after me the deluge' syndrome. Bhargava did indeed build one of our most modern laboratories in molecular biology at Hyderabad and successfully translated a vision he had held for years. That would hardly have been possible if there was not a broad consensus on the need for excellence in modern biology and if the venture had not been fully supported by the science managers of that time. The journal *Nature*, in the characteristically mischievous manner of a successful magazine (which, of course, makes for good reading) has provided ample and visible space for Bhargava to publicly display his sense of frustration. I did not however, quite understand the title – 'The cowboys in Indian science'. It did take me back to a misspent childhood and visions of the Wild West as seen through comics, where the cowboys were always the good guys while the Indians (red, of course) were the screaming villains.

The first reactions of those who now constitute the scientific 'establishment' have been angry, but some reflection might, in fact, bring temperatures back to near normal. Bhargava is not the first person to be sharply critical of the rising mediocrity and lack of accountability. Previous laments have, of course, appeared in less noticeable journals. Bhargava has admitted that we do have centres of '*excellence*' (whatever the word signifies) and gently chides Salwi for not recognizing this fact. He proceeds to name the Tata Institute of Fundamental Research in Mumbai, the Centre for Cellular and Molecular Biology in Hyderabad, the Centre for Development of Telecommunications in Delhi and the Centre for Development of Advanced Computing in Pune. Somewhat, parochially, I did notice that the Indian Institute of Science was not mentioned, but this could, of course, have been a Freudian slip. Bhargava attributes much of what is wrong to the absence of '*democratization*' of our science management. It seems almost inevitable that the virtues of democratic processes are most evident to the once powerful when they no longer command authority. But Bhargava is hopeful. He says, '*those who uphold excellence must act in concert to*

throw out mediocrity and corruption—intellectual, moral and financial. This is beginning to happen and, therefore, there is hope.'

Interestingly, plagiarism, cronyism, fraud and dishonesty exist everywhere. Bhargava admits this but argues that '*nowhere is it on the same scale and so endemic as in India*'. Here he caters to a Western gallery and overstates his case. The publicized investigations of scientific misconduct in the United States and Europe are fresh in everyone's memory. In science, as in all other professions, there are those who transgress acceptable norms of behaviour. Bhargava ignores the fact that there have been debates and discussions on the state of science in India and that many serious scientists have been concerned about some of the issues that he raises. In a peculiar throwback to a colonial age, Bhargava, a once prominent member of the establishment, seeks to present his case to a predominantly Western audience. It is precisely this weakness that has bedevilled our science for so long. A mindless desire for approbation from overseas has dominated the attitudes of many leaders of the scientific establishment. Self confidence and self esteem have been conspicuously missing and Bhargava's tirade publicly displays an attitude that is undoubtedly shared by others. The path of science in India over the last century has been rocky, but it has been brightly illuminated by many shining stars. There have been men and women of brilliant achievement, institution builders to whom we owe a debt of gratitude (ironically, Bhargava is among them) and of course legions of teachers and researchers, who in their modest ways have ensured that science occupies an important place in modern India; in marked contrast to many countries of the Third World. Reading and rereading Bhargava's essay I could not but help being saddened, but then I was reminded of the end of Robert Browning's lament about William Wordsworth in his poem 'The Lost Leader':

'Best fight on well, for we taught him – strike gallantly
Menace our heart ere we master his own
Then let him receive the new knowledge and wait us
Pardoned in Heaven, the first by the throne'

P. Balaram

CURRENT SCIENCE

Volume 75 Number 10 25 November 1998

EDITORIAL

The Raman legacy

C. V. Raman is one of the great icons of Indian science. Best known for his remarkable work on the scattering of light and his discovery in 1928 of the effect which bears his name, Raman's career spanned the most critical period of the growth of modern science in India, from the 1920s to the 1960s. The high point of his career came in 1930, when he received the Nobel prize for physics. In the public perception, the Nobel award immortalized Raman. His life and times have been chronicled in great detail in G. Venkataraman's scholarly biography, *Journey into Light*. Raman was an unusual scientist obsessed with his physics and passionately concerned with natural phenomena; attributes that often left him furroughing a lonely path. He was unsuited for the rough and tumble of science administration, which often requires finely-honed political skills. Indeed, he was the only Director of the Indian Institute of Science, who was compelled to resign. This distinction, like his Nobel prize, has remained unmatched. In an era of institution builders, which saw the spectacular growth of government-supported science, Raman remained a firm critic of the manner in which Jawaharlal Nehru and his advisers approached the issue of promoting scientific development of India. Raman launched, from the sidelines, in the 1950s, many trenchant attacks on governmental policies, some of which will undoubtedly find an echo even today. Upon his retirement from his position as professor of physics at the Indian Institute of Science in 1948, he moved across the road to build his own private laboratory, which today is the Raman Research Institute (RRI). On the 50th anniversary of its founding, it is indeed appropriate to consider the totality of the Raman legacy.

In Raman's heyday, romanticism was still an important feature of science. His extraordinary achievements and personality served as a magnet for generations of students attracted to physics. Raman's students, spread over the breadth of the country, included many of the most accomplished scientists of post-independence India. But Raman in his own inimitable way was also a creator of institutions; not always were these mere constructs of bricks and mortar. He recognized the need for scientific journals in India and helped in the starting of *Current Science* in 1932. Soon thereafter in a powerful editorial he recognized the importance of an Academy for science. A man of action, driven by impulse, often impatient with his surroundings, Raman founded the Indian Academy of Sciences in 1934, over which he imperiously presided until his death in 1970. Today, the Academy publishes many of India's scientific journals and retains a strong tradition of promoting scientific activity. The Academy's recent forays into the area of science popularization would have surely gladdened the heart of its founder. Raman's institute is today the Raman Research Institute, focusing on frontier areas of research, ranging from astrophysics and astronomy to liquid crystals. In the post-Raman era the institute has embraced government funding and is one of the many centres of advanced research directly funded by the Department of Science and Technology. Following Raman's passing in 1970 all these institutions were suddenly orphaned, an inevitable consequence of his intensely personalized approach to their functioning. Fortunately, in an environment where institutional decay is all too common, the Raman legacy was nurtured through the difficult days of the post-Raman period by Sivaraj Ramaseshan, for whom this act was indeed a labour of love. It is largely a consequence of his efforts and those of many other dedicated individuals that all of Raman's creations are both viable and vigorous today. The Current Science Association, the Academy and the Raman Research Institute coexist in harmony on a sylvan campus, much of whose greenery can be traced back to Raman. *Current Science* and the Academy are institutions, which have evolved characteristic styles of functioning that have withstood the vagaries of time. Raman was always concerned about the building of large national laboratories, which he feared would eventually become 'mausoleums of science'. His own creations have acquired a life of their own and are a fitting monument to the man. Indeed, they may survive long after the lustre of the Nobel prize has begun to fade.

P. Balaram

CURRENT SCIENCE

Volume 75 Number 11 10 December 1998

EDITORIAL

Prizes

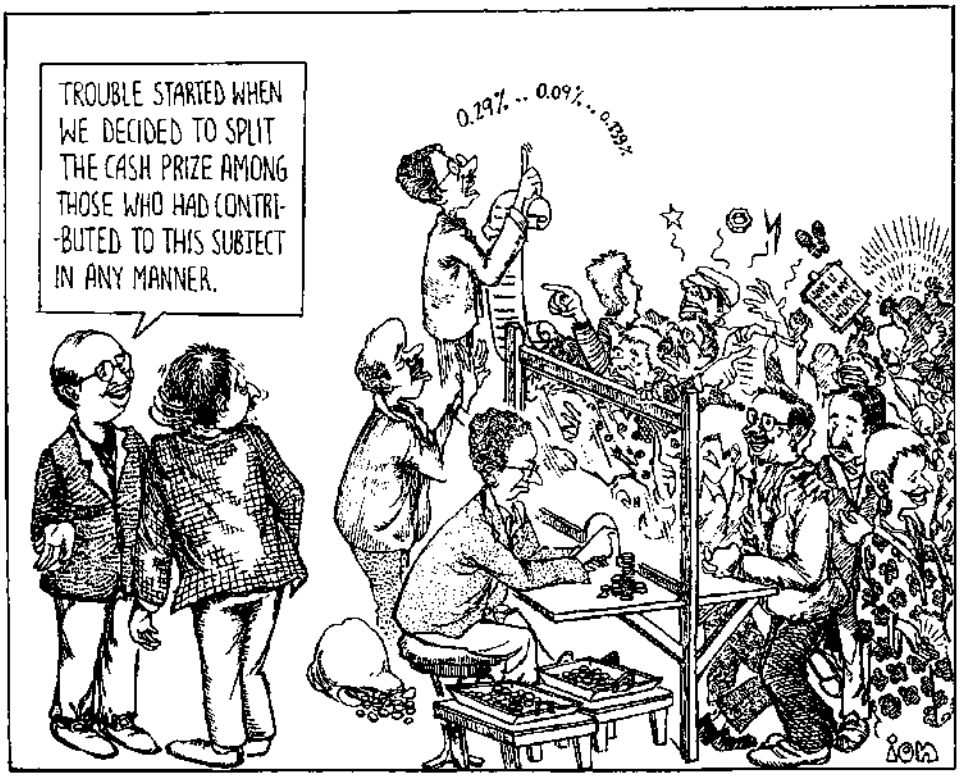
At this time of the year the Nobel prizes bring into sharp focus the process of recognition of individual achievement in literature, the sciences and economics. The prizes for 'peace' are best left undiscussed. This year's awards brought some cheer to India in the shape of Amartya Sen, whose decision to retain his original passport ensured that his fellow citizens can bask at least for a while in reflected glory. In the sciences (although limited to physics, chemistry and medicine) the Nobel awards represent the summit of recognition, despite an increasing number of handsome prizes emanating in different continents, which have been growing in monetary value and prestige. In recent times, prizes in many disciplines are shared, often by as many as three individuals as happened this year in physics, and medicine. Inevitably, as the number of scientists who make important contributions grows, major omissions in the Nobel awards are the subject of intense discussion in the weeks following the announcement. This year's prize for medicine and physiology, which was shared by three Americans, Robert Furchgott (State University of New York), Louis Ignarro (University of California, Los Angeles) and Ferid Murad (University of Texas Medical School, Houston), for their work on the role of nitric oxide in physiology and their recognition of its importance as a signalling molecule in biology, has stirred up a minor tempest. Omitted from the honours list was Salvador Moncada (University College, London) who was indeed the leader of the group that first concluded that Furchgott's 'endothelium-derived relaxing factor' (EDRF) and nitric oxide were indeed identical. Moncada's paper published in *Nature* (1987, **327**, 524–526) led to an accompanying 'News and Views' write-up that called the finding – 'the climax of one of the most exciting sagas in vascular physiology and pharmacology'. Nevertheless, Moncada has been left out in the cold. Surprisingly, the 1996 Lasker award for medical research, often considered a forerunner of the Nobel prize went only to Furchgott and Murad. Moncada's exclusion has prompted Cesar Milstein (of monoclonal antibody fame and a 1986 Nobel laureate) to strongly criticize the Nobel Committee and to state that he is 'uneasy about the way that recognition is being given at the level of major prizes, and in terms of careers and publications'. There have also been instances in the past (and even this year) where members of research groups, whose leaders have been recognized, have felt that they have not been given a fair share of the credit. The frequency, with which controversies accompany the announcement of such major awards, appears to be increasing; an inevitable consequence of the growing complexity of the scientific enterprise. Fertile research fields are often born inconspicuously but later flower, producing a flood of results. A high level of retrospective scholarly analysis is necessary in order to trace the intellectual roots of an area of science. With larger numbers of scientists making important contributions, future prizes may be as much noted for their omissions as for their citations.

In deciding awards there is always the conflict of choosing between 'discoveries' and substantial bodies of work that slowly transform a field. The polymerase chain reaction (PCR) is an example of an instantly applicable discovery that swept the field of molecular biology like wildfire, garnering for its discoverer, Kary Mullis, the Nobel prize in chemistry. Here scholarship and years of intellectual toil were conspicuously absent; what was important was the quick germination of an idea, that led to a technique whose time had come. In awarding the prize to Mullis the Nobel Committee strictly followed the terms of their benefactor's will. Many times however, awards are not made for 'instant finds', but are indeed given to those who have cultivated an important area of science for years; transforming an initially barren field into a lush and thriving area of scientific activity. Here, patience, dedication and a firm conviction of the importance of a relatively uncharted area ('tall oaks from little acorns grow') are necessary to carry pioneers through years of non-recognition. Several Nobel awards do indeed pay a tribute to lifetime achievement, although the bare citations sometimes hid this fact. This year's chemistry prizes to John Pople and Walter Kohn may indeed fall into this category. The recent Lasker awards also included a special lifetime honour for Daniel Koshland, the Berkeley biochemist.

Even the mathematicians at a recent Congress feted Andrew Wyle (the conqueror of Fermat's theorem) with a special achievement award. He did not receive the Fields medal, because of limitations on the age of the awardee. But then as an onlooker is reported to have said – 'Wyle does not require the Fields medal as much as the medal requires Wyle'.

Are awards important? The desire for recognition by peers and the public at large is an almost universal feature, in all spheres of human activity. For filmstars, sportspersons, artists and musicians, public appreciation can sometimes act as the catalyst for greater achievement. Scientists (though less visible in a public sense) are often no different. The problem with awards, of course, is that for every deserving scientist who wins one, there are several equally accomplished researchers, who do not. In India too we have built up elaborate rewards system, whose credibility is often questioned. Reflections on the awards process must, however, wait for another time.

P. Balaram



CURRENT SCIENCE

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EDITORIAL

History in science

Is the history of science important enough to be taught or even occasionally discussed by teachers and students? Or is history something that ageing scientists are comfortable with (living in the past can be pleasantly sedative), while the young are clearly more concerned with the future. In our surroundings, wrestling with the problems of the present can sometimes be an overwhelming task, leaving little time for contemplating the past or the future. In many subjects in the sciences, the volume of information that needs to be transmitted to new students has increased enormously in the past fifty years. Teachers struggle to be coherent, comprehensive and correct even in post-graduate courses. Their task is not lightened by rigid syllabi, archaic examination systems and the diminishing quality of students entering science courses. Students now enroll for research degrees more poorly prepared than ever before, only to realize that they have entered areas of research, which are moving forward at a breathtaking pace worldwide. In areas like biology and medicine the progress is over such a broad front that new entrants to the field find that the battle to stay abreast is lost even before it has begun. There is so much that is new that has to be assimilated in time spans that become shorter with each passing day. Looking back to logically trace the development of an area, thus appears to be a poor investment of time and effort. History, therefore does not seem to have an important place in the teaching of science today. Few teachers seem to echo Goethe's' famous statement that 'the history of a science is the science itself'.

Scientific papers are invariably written in a sanitized style, in which failures and misinterpretations of data that are common in the course of research are not recorded. The false starts and false trails, that are explored before an even relatively minor research problem is satisfactorily addressed, are never described. Journals have a limited space, editors and referees have a charter to ensure economy of presentation and, of course, most readers are impatient. All of these factors ensure that most scientific papers are incomprehensible to the non-specialist (specialization today is rapidly becoming a case of 'knowing more and more about less and less'). Most papers are certainly unhelpful to students of the subject seeking to explore the origin and development of concepts and techniques. In their haste to provide encyclopaedic coverage of their fields, authors of books too treat historical development in cavalier fashion. Students thus, have only a very hazy view of the history of the fields in which they intend to do research. Balanced views of emerging areas of science are provided by reviews, which have today acquired enormous influence and importance, as witnessed by the phenomenal growth of review journals, focusing on current 'hot' topics. Unfortunately, most often, the average review provides only a round-up of the current literature with almost no reference to historical background of an area.

In reflecting on the relevance of an historical approach to teaching science we might do well to recall Maxwell's appeal to Ampere: 'If you have built up a perfect demonstration do not remove all traces of the scaffolding by which you have raised it'. Looking back can teach us the importance of being wrong. History can sometimes be difficult to write and historical accounts can be even more difficult to judge. In recent days the debate on the reconstitution of the Indian Council of Historical Research emphasizes that 'rewriting' history can be politically expedient. In science, of course, 'history' has been approached in a more detached and scholarly way, but the manner of chronicling events depends very much on the observer. There can be 'biased' and 'personalized' accounts (often very readable) in which the observers are also participants. James Watson's 'The Double Helix' belongs to this genre. At the other end of the spectrum are the more elaborate and well-documented treatments of a field; Robert Olby's 'Path to the Double Helix' is an example. In tracing the growth of a subject, historians of science tread a dangerous path, reminding us of the dictum that 'time blots out small merit while fattening big glory'. An historical approach to teaching a subject can often be inspira-

tional; which budding scientist can remain unstirred by tales of Galileo and the inquisition, a paranoid Newton dabbling in alchemy, the boy Faraday transforming himself into one of the great experimentalists of all time, Pasteur's remarkable career which set the stage for microbiology, vaccinology and stereochemistry, the unmatched story of Ramanujan and his magic with numbers, Madam Curie's legendary commitment which transformed a field and Raman's move from the accountant general's office to resurrect the Indian Association for the Cultivation of Science. The glamorous and much-told stories of the early days of quantum mechanics have never ceased to inspire young (and even ageing) physicists. Stories of people and discoveries convey a sense of excitement and challenge, providing an element of romanticism in science. In today's highly competitive and 'professional' environment there seems little time and place for history and the retelling of famous stories in the classroom or on the pages of journals and textbooks. Divorced from the past, modern science may seem too sterile and purposeful to attract the dreamers amongst students. Unfortunately, it is the dreamers who will create the new science of tomorrow.

P. Balaram

CURRENT SCIENCE

Volume 76 Number 1 10 January 1999

EDITORIAL

Blue sky research

'Blue sky research', a term popular over a decade ago is hardly ever heard in the corridors of funding agencies or research institutions today. The economists and bureaucrats who control the purse strings of government are fond of cost-benefit analysis. While money thrown away on various subsidies (sometimes even in the form of concessions to already fattened industrial houses) yields political dividends, there seems little reason to invest in basic science and higher education, whose returns are not clearly apparent. For a considerable period after independence the personal equations of leading scientists with political powers, insulated scientific agencies from the budgetary imperatives imposed by growing fiscal deficits. Today, except for the departments which preside over strategic science, other agencies face a bleak future. The universities were, of course, cut adrift long ago.

Frozen budgets, rising inflation a quantum jump in salaries and the declining value of the rupee have hit our research institutions hard. Government agencies which disburse research grants and fellowships have also felt the budget crunch, although it is hard to get anyone of importance in the funding bodies to admit that money for research is in short supply. A smaller purse is directly correlated with a diminished position of influence. Consequently, many in the upper echelons of the agencies adopt a somewhat ostrich-like approach and refuse to concede that money for academic research is scarce. Instead they adopt an overly optimistic stance—'show me a good project that has not been funded' or 'no good laboratory will suffer for lack of funds'. Brave words indeed, which unfortunately reflect a lack of appreciation of ground realities.

Laboratories in academic institutions receive little or no support for research from their parent organizations. Most institutions are hard pressed to meet salary bills and the inevitable cost of essential services like water and power. There is invariably no money (or will) for maintenance of physical facilities in academic institutions. Little is left to support specific research activities or even to fund basic equipment infrastructure. The conduct of research in academia, thus, relies entirely on the support provided by external agencies of government; private funding is too insignificant to be a matter of serious discussion. In areas like physics, chemistry, biology and the earth sciences (mathematics fortunately requires little money), the Department of Science and Technology (DST), the Department of Biotechnology (DBT) and the extramural wing of the Council of Scientific and Industrial Research (CSIR) are the agencies which provide the main support for 'blue sky' research. Agriculture and medicine have specialized councils which run networks of institutions. Even at agencies which apparently look favourably at basic research, the fraction of funds set aside for academic science is diminishing, with increasing pressure to support projects which promise (but often do not deliver) practical utility. While the CSIR has always had as its primary mandate the task of running a large network of national laboratories, over the years 'pure funding agencies' like the DST and DBT have acquired a portfolio of their own research institutions. This process has slowly begun to create two divisions—'intramural research' and 'extramural research'. This is not entirely undesirable. The National Institutes of Health in the United States successfully manages an enormous internal program, together with a very large external commitment to academic institutions.

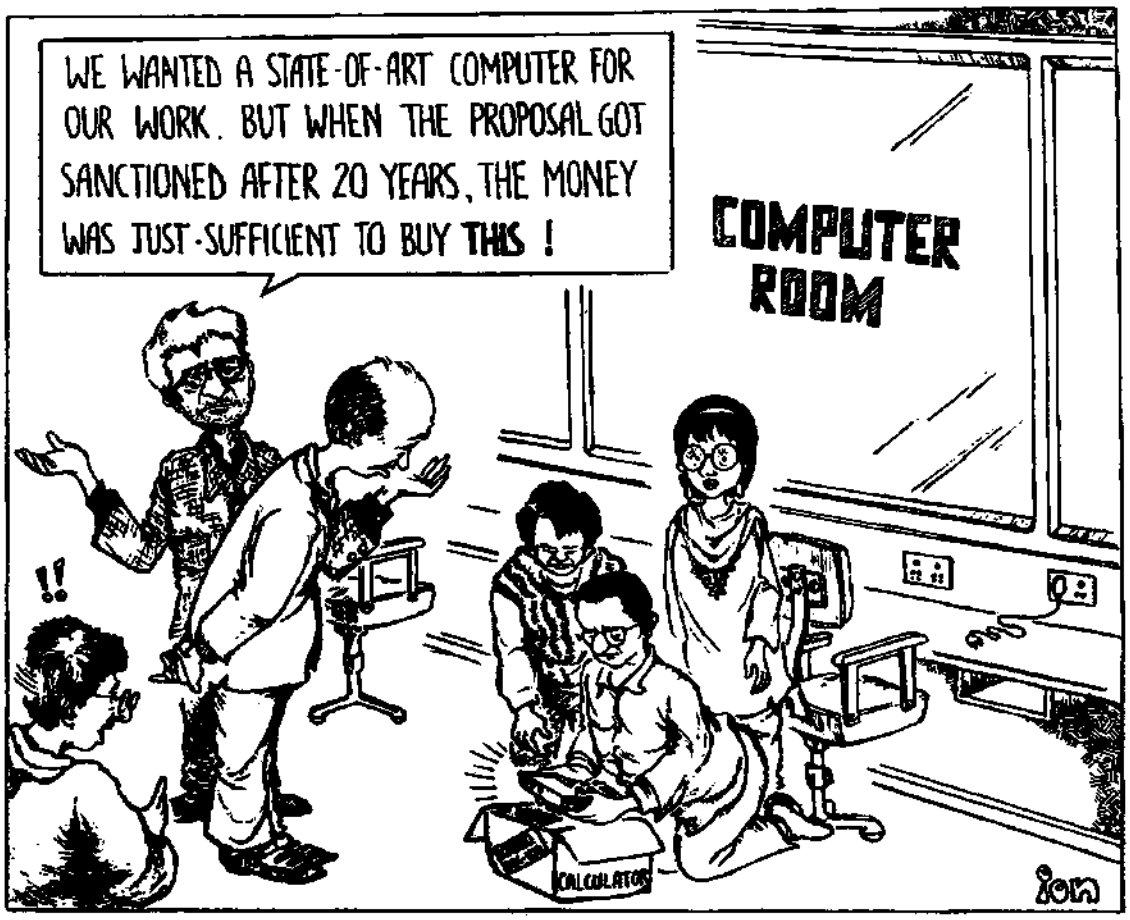
Unfortunately in India, limited budgets are compounded by a growing breakdown of the administrative structure at the agencies. Today, every academic investigator has to learn to live with increasing periods of delay in the release of approved grants. The hardest hit are the research fellows, students and post-doctoral associates who are the true footsoldiers of the research enterprise. Delays in release of grants and fellowships means that the actual workers in the laboratory go without pay. Uncertainty in the funding process often requires that staff employed in research projects must leave; a tremendous waste of the training period which they undergo in most laboratories. Unfortunately our institutions do not have sufficient bank balances (or administrative flexibility) to step into the breach in times of crisis involving project staff. Since almost all funding emanates centrally from Delhi, a lack of sensitivity to these issues in the finance departments of the agencies ensures that no appeals are ever heeded. What is not

appreciated is that the key elements in the workforce of academic science are 'temporary' – research fellows and post-doctoral associates; their salaries are met from project funds. Happily, professors and scientists are 'permanent' – their salaries are paid by government even if they have no funds (or will) to do research. Ironically, in science we often have the situation where the generals are paid but the troops are forgotten.

Scientists have unfortunately been turned into servile supplicants who must run from pillar to post to obtain financial sanctions. If budgets are tight there must be a clear signal that this is so. Only then can institutions and laboratories plan their programs accordingly, even limited budgets must be administered and disbursed efficiently. At the scientific agencies there is a clear need to define an administrative chain of command that ensures that the totality of finances are considered before project approvals are finalized. At present, scientists go through an elaborate rigmarole of peer review of projects with little knowledge of the depths of the agencies purse. There is also an urgent need for the funding agencies to examine their role as facilitators of research. Some of them appear to have evolved into bodies which have a life of their own, quite independent of the research community that they are intended to promote.

It may indeed be the right time to consider creation of a new mechanism for funding science. An autonomous structure like a National Foundation for Science, which operates out of a government corpus supplemented by private funds may be a possible way of breaking out of the shackles imposed by a ministerial structure. If basic science is not nurtured in the academic institutions there is little hope that good applied or 'applicable science' will emerge in the future.

P. Balaram



CURRENT SCIENCE

Volume 76 Number 2 25 January 1999

EDITORIAL

Scientific publishing in the Third World

An interesting paper recently produced by the Committee on Science and Technology in Developing Countries (COSTED) was entitled 'Scientific journal publishing in the developing world?'. Authored by Ana María Cetto of the Universidad Nacional Autónoma de Mexico, the article provides a perceptive analysis of the problems of Third World (a term Cetto largely avoids) journals with particular reference to Latin America. The question mark in the title is particularly provocative. Indeed, one might ask several pointed questions about scientific journals produced in the developing countries. Firstly, should they be produced at all? Secondly, are at least some of these journals relevant in a local context? Thirdly, can anything be done to improve the quality, visibility and international impact of journals from the developing countries? Cetto considers these issues and concludes on a positive note 'that there are strong reasons to promote and strengthen these journals by every means possible to enhance their quality, relevance, diffusion and visibility'. Many of the concerns voiced by Cetto will be strongly echoed in India. Indeed, her characterization of 'the Latin American landscape' as 'that of First World islands embedded in a Third World environment', would be an apt description of our own situation; very few institutions can access the vast body of scientific literature published in the world, both in printed form and through 'on line' electronic services, while for the majority local libraries provide a pitifully poor supply of journals, burdened as they are with increasing costs and diminishing budgets. Despite all the publicity, 'information superhighways' may not reach most of our scientific institutions and Universities for some time to come. In such a scenario the traditional printed journal, particularly if it is produced locally and economically, may still have a major role in the dissemination of scientific information in the poorer countries.

India is particularly fortunate in having a tradition of scientific publishing. Many of our journals have long, if not distinguished, histories. This journal is a rare example of an inter-disciplinary publication in the Third World; indeed it is even more unusual in that its pages provide an opportunity for expression of opinions. Dissent and underdevelopment rarely coexist comfortably. In this context Cetto's comments on Latin American journals are relevant: 'Traditionally, in contrast to journals from developed countries, scientific journals in Latin America do not publish book reviews and very seldom do they publish letters to the editor; also the proportion of conference papers and proceedings is low. There does not seem to be any recognition on the part of the editors, of the need to offer a wider range of documents, that would perhaps make their journals more appealing to their readership?' Here, Cetto raises an important issue. Are many scientific journals which struggle to survive, more tuned to author's needs, with little regard for readers? This is particularly true in specialist journals where 'editors and authors belong to the same reduced circle'. Third World journals also suffer from limited circulation, ironically even in the countries of publication. This is partly because of deficiencies in promoting and distributing publications. Cetto quotes from the recommendations of a workshop on publications held at Guadaljara and charges editors with the responsibility of searching 'for new ways of financing, distribution, publicity and marketing, so as to ensure the sustainability and the necessary efficacy, both practical and in terms of knowledge of scientific journals'.

Reading Cetto's thoughtful essay, I could not but help reflecting on *Current Science*. There is pressing need to raise resources for this journal, increase circulation and to enhance both national and international visibility. The quality of the manuscripts published in many ways determines the future of the journal. The present contents of the journal provide a reflection of the nature of the manuscripts received. Some disciplines like the earth sciences and classical biology are over-represented. Others like physics, chemistry and molecular biology

are under-represented. A substantial part of the journal is indeed devoted to subjects that have a deeply rooted local origin – geology, ecology, agriculture, botany, zoology. This is inevitable. A more balanced coverage of disciplines is possible only if the community of scientists in India view the journal as a suitable medium of communication of at least some of their best results. The impact factor (however much one would like to ignore it) will rise only when authors cite their own work published in this journal, when they publish in the prestigious journals of the First World. Reference lists are a clear way of drawing attention to our own journals (and indeed our own papers).

Current Science also serves an important local function. It provides a forum for discussion of issues that concern the community of scientists in India. This medium is not always effective. Indeed, in recent times, the journal's silence on issues like the atomic explosions have evoked criticism. Nevertheless, there is every reason to hope that the pages of this journal will foster healthy debate and that editorial policy will remain unaffected by external pressures. Enhancing the readership of this journal in India is indeed our primary objective. One can hardly expect that a journal that is unread in the country of its origin will be found interesting and useful elsewhere. We also intend to make every effort to enhance the international visibility of the journal. In this endeavour, the cooperation and help of almost the entire community of scientists in India will be necessary. Only then can *Current Science* provide a window through which the world can view science in India. In the years to come, scientific publishing (like science itself) is going to become a more competitive and difficult activity. Third World journals will undoubtedly face a very uncertain future. Cetto's scholarly analysis reminds us that we must all constantly evaluate our strategies for survival.

P. Balaram

CURRENT SCIENCE

Volume 76 Number 3 10 February 1999

EDITORIAL

Research guides and students

The recent suicide of a fifth year graduate student in Chemistry at Harvard University, apparently the third case in the last few years, has sparked off widespread concern about the pressures to which Ph D students are subjected, during their research towards a doctoral degree. The extensive comments on the case in many journals (cf *Nature*, 1998, vol. 396, p. 823, 826) suggest that a complete reexamination of the mentor–student relationship, in the context of modern day science, may be overdue. In India, almost all basic research involves students who work towards Ph D degrees. This is in contrast to the West, where teams of postdoctoral researchers (who are usually battle hardened) often tackle major problems, under the general guidance of an established scientist. In the best of Indian institutions Ph D students not only work towards their own theses; they also carry out the projects of prime concern to their laboratories. This is not a pressure-free exercise nowadays, since many investigators promise the moon in return for large grants from funding agencies. Once the Faustian bargain has been struck, the die is cast. Unfortunately, it is the students who must finally deliver. It is thus not uncommon to find overstressed and misunderstood research students on campuses across the country (and indeed the world over).

Students enter research in our institutions, poorly prepared in their disciplines – a consequence of the many strains to which the teaching system has been subject to in the Universities. Some enter research with a sadly romantic view of science, only to be rapidly disillusioned. Dreamers can be misfits in the best of institutions nowadays. Others drift into a Ph D programme, because this is a comfortable option, in areas where jobs are scarce and Masters degrees largely worthless. There are, of course, many pragmatic and level-headed individuals who see a Ph D degree as a passport to research positions at laboratories across the world. Even before globalization, science provided opportunities for worldwide employment.

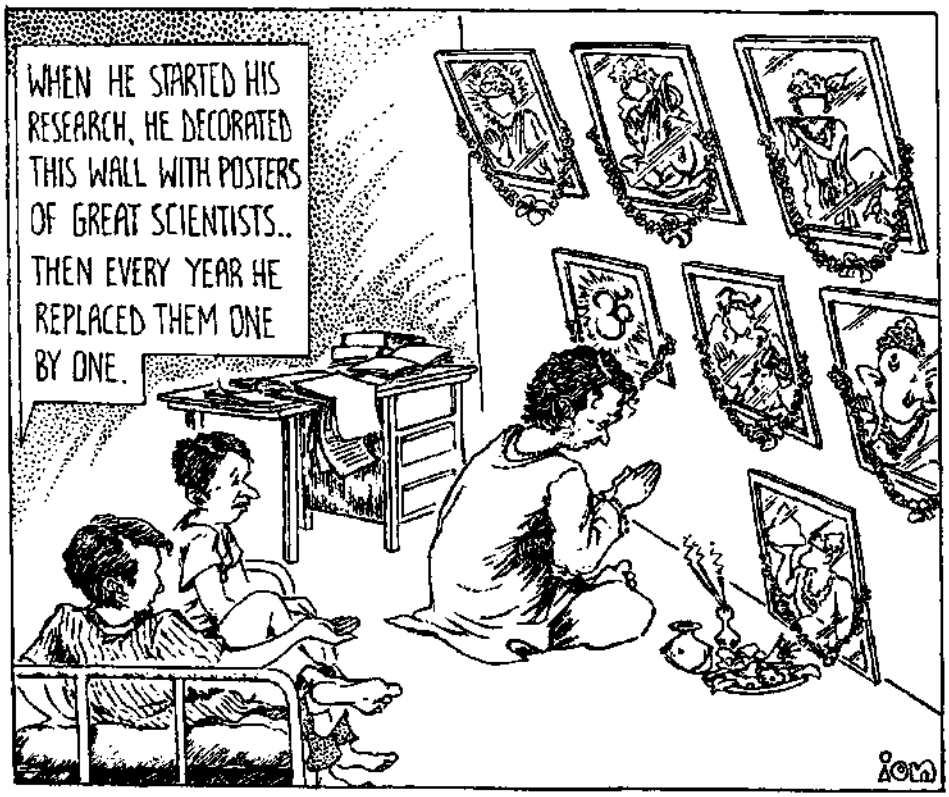
The process of entry into a career in scientific research can be painfully slow. Ph D degrees often require periods as long as 5–7 years to acquire; long stints of postdoctoral research, in temporary positions, are almost obligatory, before consideration for an entry level appointment at an academic institution or R&D laboratory. Despite salary hikes, students and research associates are poorly paid compared to their compatriots in other professions, who quickly enter permanent career positions. The period of uncertainty can sometimes last well into the mid-thirties (or even beyond), by which time 'the young scientist' is neither really young nor in a position to take significant professional risks, which are often a key to success in research. By this stage in life, most others opting for careers in the bureaucracy or in commercial professions are already well set on their career paths. Even the lucky few who find faculty positions in academia, struggle with a situation where research funds are drying up and the environment is tilting decidedly away from academic pursuits. It is against this background, which is now almost a global phenomenon, that we must view the frustrations of those who seek to enter research careers.

Mentors are critically important for most students who wish to enter research. Geniuses who suddenly bloom without any tending are extremely rare. The relationship between Ph D students and their research guides can make or break the careers of aspiring researchers. Like in marriage, divorce can be difficult and unpleasant. Academic departments also tend to be places where the individual autonomy of the faculty is strongly protected. The result is that guide–student conflicts are allowed to simmer endlessly; most often the student is the loser. Departments usually adopt a hands-off attitude towards guide–student problems, until it is too late. Surprisingly, the same departments (and institutions) adopt a collective stance when students are admitted. This attitude is a worldwide phenomenon, with the best of Universities being no exceptions. The strategy of leaving students exclusively at the mercy of their supervisors has been largely successful; many research guides turn out to be good mentors. It is only when problems surface, do institutions wonder whether there are any formal alternatives to this system. The possibility of involving multiple faculty members (graduate committees in Ameri-

can parlance) have been tried in some places, but invariably the system lapses into the classical mode, where the equations with a guide are the dominant determinant of a student's future. Institutions are usually reluctant to accept the fact that despite their apparently rigorous selection procedures they do admit students, clearly unsuited for a research career. There is, of course, even greater reluctance to admit that there can be members of the permanent faculty, who are unsuitable to be mentors for fresh Ph D students; great scientific accomplishment, while desirable in a mentor, is not an essential prerequisite for a research guide. In the modern day context, there is indeed a danger that the overweaning scientific ambition of established scientists may become an altar on which many beginning students can be sacrificed. Institutions must recognize the importance of developing robust internal mechanisms, which can permit the graceful exit of students, who do not possess the motivation (and stamina) to pursue research as a career. At the same time an effort must be made to ensure that academic review procedures protect serious students from systemic faults associated with poor mentoring.

Incidents, like the one at Harvard, remind us that academic frustration amongst students is not uncommon and that problems can be compounded in hostile or impersonal environments. Supportive surroundings can mitigate even the most unpleasant circumstances. Academic departments and research institutions would do well to reflect on their ambience; unhappy researchers are hardly the right ingredient for scientific success.

P. Balaram



CURRENT SCIENCE

Volume 76 Number 4 25 February 1999

EDITORIAL

Ageing scientists

The 'graying' of our scientific institutions has been the subject of some discussion following publication of a survey, which revealed that the average age of scientists in national laboratories was dangerously close to fifty. Youth is invariably associated with vigour (and enthusiasm) and therefore it is not surprising that thoughtful science administrators view an ageing staff profile with concern. Advancing age is rarely a desirable qualification, except in politics. Recruitment to our institutions occurs only as a small trickle nowadays; burdened as most organizations are with an already bloated staff structure. Retirements have also been stalled, thanks to the increase in the age of retirement of scientists and academics from 60 to 62 years. This, of course, is hardly a measure that is calculated to reduce the average age of staff in our scientific laboratories.

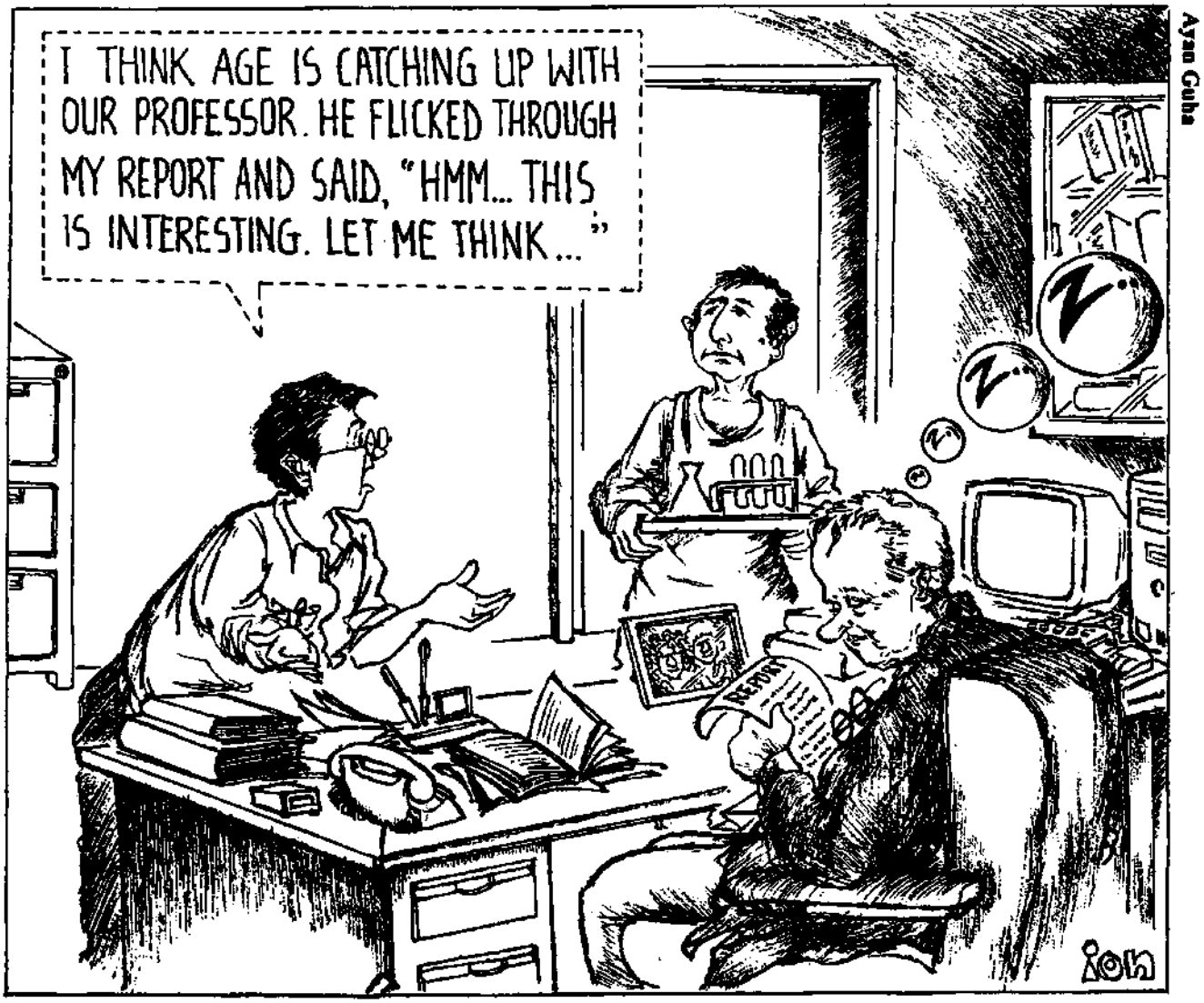
How would one go about reducing the 'average age' of scientists, if, that were indeed a desirable goal. One obvious strategy would be to recruit large numbers of young (truly 'young') researchers to the workforce. Unfortunately, in most places recruitments are frozen, thanks to the financial burden of an already alarming salary budget and the enormous constraints placed by restrictions on hiring. Alternatively, the average age could be lowered by providing incentives (truly 'golden handshakes') that would induce many senior scientists, who do little else but contribute to the 'average age', to make way for younger recruits. Unfortunately such schemes in the public sector (where the motivation was to downsize the staff) have only facilitated the departure of competent and skilled personnel, who find better employment elsewhere, thereby further weakening already moribund organizations.

If 'graying' is indeed a matter of concern, then special attention must be paid to the operation of various schemes by which scientists are retained after retirement. While every effort must be made to ensure that active and productive scientists continue to have full access to laboratories and funds, irrespective of age, the criteria for exercising judgement in emeritus scientist schemes must be strict; high standards must be ensured. Our inability to exercise collective judgement in a fair and credible manner, has inexorably extended to all levels of selection and decision making. The 'lowest common denominator' principle now operates; mistakes of the past are being cited as examples justifying the errors of judgement of the future. It is indeed surprising that the across-the-board increase in retirement age was accepted with such alacrity by the government, when job opportunities for new entrants are so scarce. The inescapable conclusion is that the lobbying power of senior administrators is so great, that there is little time to reflect on the consequences of a decision before it is made. Unfortunately, a substantial number of senior academics who approach retirement contribute little to teaching or research activities. Seniority brings with it many perks; avoidance of routine duties being one of them. Even more disturbingly, administrative positions in most scientific institutions are frequently filled using seniority as a criterion.

The issue at hand is critically important. How can there be a resurgence of our scientific research institutions by infusing a substantial number of new investigators into the system? Should we not be considering innovative schemes which will provide new positions in our laboratories? China, for example, has embarked on an ambitious plan to increase the number of research positions. The irony of course, in Universities, is the large number of unfilled positions which have become a casualty of the 'reservation system'. Should we also not be considering schemes which will ensure that new entrants to the scientific enterprise are challenged vigourously to prove themselves, rather than to settle into a gentle and somnolent existence like their forbears, where premature 'ageing' is inevitable. Ageing, most often is a state of mind. It is quite common to find 'young'

researchers, who have mentally retired while in service. It is less common, of course, to find the vigourous 'retired scientist' who still contributes vibrantly to the scientific process. Many new initiatives are necessary to foster science in India at a time when most of our institutions are struggling to be competitive. At least one of these should be directed towards increasing the prospects for employment of the many trained researchers whom we are producing.

P. Balaram



Ayan Guha

CURRENT SCIENCE

Volume 76 Number 5 10 March 1999

EDITORIAL

Current Science

Readers may have noticed that some recent issues of *Current Science* have been substantially thicker than usual. This sudden spurt in the size of the journal is an unanticipated result of an increase in the number of submissions to special sections, together with the steady increase in the number of papers accepted through our normal procedures. In order to maintain an acceptable size, we intend to tighten editorial procedures over the next few months. In particular, strict limits will be imposed on submissions to the Correspondence, Commentary, Opinion, Book Reviews and Historical sections of the journal. The authors of Reviews, Research Accounts and General Articles will also be encouraged to be economical in their use of journal space. Thus far, this journal has been very liberal in permitting illustrations in colour at no cost to the authors. Indeed, colour figures do add to the overall attractiveness of a publication. We would urge authors in future, to use this facility economically. In cases where the excessive use of colour is requested, the costs of production will at least partly, have to be borne by the authors.

Current Science's ongoing battle with obesity has highlighted the fact that many authors write with little regard for their readers. A short, punchy letter which makes a specific point is more likely to be read by a large audience, as compared to a verbose piece, which states and restates many well-known generalities. Commentaries and Opinion pieces must be sharply worded; succinct analysis is certainly likely to evoke a greater reader response, than a long winded essay, whose purpose is often unclear. The Book Reviews section is a powerful medium for thoughtful reviewers to sometimes reflect on the status of the specific discipline treated in the book. Cataloguing of the contents alone, does not make for good reading. In opening the pages of *Current Science* to the discussion of controversial topics, the intention was to provide a forum for debate on issues concerning science and scientists. Brevity and clarity are essential ingredients for stimulating debate. A numbed and somnolent readership is hardly likely to respond.

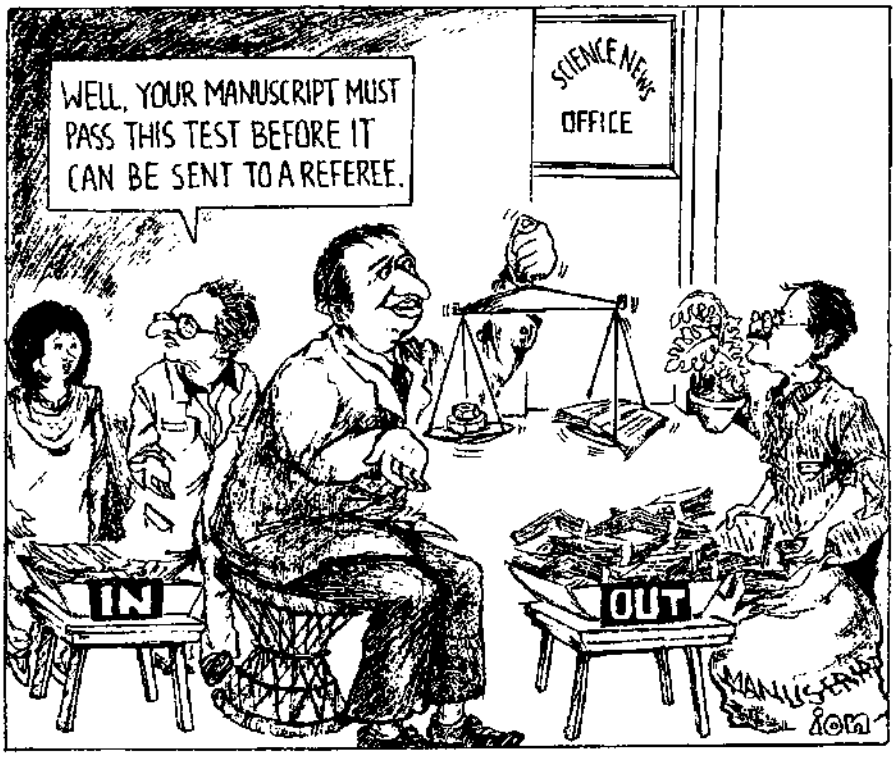
A concerted effort will now be made to enhance the research content of the journal. We have been and will continue to be flexible (within reasonable limits) in the sizes of articles accepted under the Research Communication and Research Article sections. Limited reports of research findings that appear in the Scientific Correspondence section must strictly conform to prescribed size limits. In attempting to enhance the range of subjects represented in the journal, we invite authors in disciplines poorly represented on our pages – physics, chemistry, molecular biology and engineering sciences – to submit some of their best work for consideration. The two sections of the journal with the highest rejection rate for unsolicited manuscripts are Research News and Meeting Reports. Research News articles must highlight in a broadly readable manner, important new findings that appear in the scientific literature. Such manuscripts must be written in a manner that the major thrust is clear to a general audience of scientists. They should also attract specialist readers with a sufficiently scholarly overview. Meeting reports are intended to provide highlights of important workshops and symposia in different fields. They must not become a report of inaugural ceremonies, exhaustively listing the dignitaries who invariably appear on the dias on these occasions. They should also not be a mere catalogue of speakers and topics, but must focus on the science discussed.

Apart from a healthy inflow of good manuscripts, a sound financial position is, of course, an absolute necessity for any journal. Since personal subscriptions are 'subsidized' (an unpopular word, nowadays) we must turn to institutional memberships, advertisements and science funding agencies of government for support. An increased base of personal subscriptions will make the journal a more attractive advertising medium. Indian scientific journals have generally avoided vigourous pro-

motional campaigns; partly a consequence of governmental 'umbrellas' and partly an inability to connect commercial and scientific success. *Current Science* may be gingerly taking the first steps into this uncharted territory.

At a time when most professional, specialist journals in India are faced with diminishing visibility, there is a unique opportunity to promote a truly interdisciplinary journal. In this task, we must necessarily rely on the active support of authors, reviewers and readers, many of whom wear all three hats interchangeably. The inputs received from readers are most valuable in identifying areas for improvement.

P. Balaram



CURRENT SCIENCE

Volume 76 Number 6 25 March 1999

EDITORIAL

Strategic follies

Two recent news items should bring good cheer to the scientists in the laboratories of the Defence Research and Development Organization (DRDO). First is the enhanced pay given to DRDO scientists 'keeping in view the role played by them in the development of high technology and systems for strategic applications'. This special pay hike has been approved 'taking all relevant factors into account and in order to attract, retain, inspire and motivate scientists to give their best contributions'. The second announces the institution of a set of handsome annual awards for development of defence-related technologies. While the pay increases appear to be restricted to the DRDO, the awards will also go to their partners; often organizations like the DAE or academic institutions. These measures act as a shot in the arm for military S&T and underscore the extraordinary emphasis that is currently being placed on the development of defence-related 'strategic' research programmes.

The encouragement given to defence science can be contrasted with the apparently wilful neglect of the health sector, where major organizations like the Indian Council of Medical Research (ICMR) and the few national medical centres like the All India Institute of Medical Sciences (AIIMS) languish under the control of the Health Ministry. Ironically even as the DRDO bonuses were being announced, the AIIMS faculty was agitating for better pay scales. What indeed is strategic science? Should not diseases caused by invisible pathogenic organisms like malaria, tuberculosis, hepatitis and AIDS be viewed as great threats to national security? The plague scare of a few years ago highlights the fragile nature of our public health systems and underscores the havoc that can be caused by an epidemic, real or imagined. Biomedical research in the advanced countries is experiencing an unprecedented boom; public and political support ensuring that basic advances in biological sciences are indeed applied to the most important problems that affect human health. In our surroundings however, there is an inadequate appreciation of the many manifestations of 'strategic science'.

Many areas of science in India are handicapped today by inadequate support, both financial and moral. Nowhere is the problem more starkly presented than in the decline (and fall) of the science departments of the major universities. It is in fact from many of these struggling institutions that the 'strategic scientists' of the future will emerge. Some years ago the decline of the university science system was discussed and debated in the hope that improvements are possible. Today even that measure of optimism is absent. The fact that even an organization like the Indian Science Congress has invariably been presided over in recent times by a prominent science administrator from Delhi, emphasizes that science as practised in academia no longer appears relevant or appealing even in a large gathering of 'scientists'. Focal themes at the Congress seem to reflect political compulsions, which invariably demand projections of unachievable social relevance. It is not surprising that much of what transpires at the Congress is simply not science.

In the mindless euphoria that marked the aftermath of Pokhran II, there was even talk of the development of a 'military–industrial complex'. In this scenario, technological advances (with attendant fall out in the civilian sector) would be fuelled by 'strategic requirements'. Unfortunately, no prescription for technological advance is likely to be successful if we ignore the relentless erosion in the base of the scientific enterprise. The Fifth Pay Commission boosted salaries of teachers and scientists dramatically enhancing the salary bills and depleting the coffers of already resource-starved institutions. The motivation for the pay hikes was of course, the insatiable demands of a bloated and unproductive, but immensely powerful bureaucracy. All sectors of course necessarily follow suit, with administrators carefully seeing that every pay commission enhances disparities, which then become an endless bone of contention. By the time the ripples and controversies of one Pay Commission subside, it is time to start the whole process again. The fall out from the selective acceptance of the Fifth Pay Commission's report may be eventually even more far reaching than that from the nuclear explosions. One lesson is however, clear. While there appears to be some limited appreciation for what constitutes 'strategic science' there is in fact no clear strategy for promoting science.

P. Balaram

CURRENT SCIENCE

Volume 76 Number 7 10 April 1999

EDITORIAL

Biotechnology courses

Biotechnology is a popular and fashionable word in scientific circles, nowadays. The perceived utility of the spectacular advances in basic molecular biology, spawned the biotechnology industry in the United States, over two decades ago. Today, biotechnology research is an integral part of the efforts of major multinational companies, to carve out a piece of the cake in the area of agricultural and biomedical products. While small research-based companies are still a very prominent part of the biotechnology scene, the successes of the last decade have persuaded large, intrinsically conservative, industry giants to invest substantially in the area of biotechnology.

Classical biotechnology is even older than this century. Fermentation and the production of vaccines, predate the many advances of 20th century science, that sowed the seeds of modern biotechnology. The modern era can, arguably, be traced to the fundamental advances made in molecular biology, which allowed not only an understanding of the structure of DNA and the transfer of genetic information, but also made possible the cloning, amplification and manipulation of genes. The ability to transform cells with foreign genetic material was realized in the 1970s, ushering in the era of genetic engineering; probably the most widely discussed topic in biology in recent times. Indeed, genetically engineered crops and vaccines are the subject of much scientific interest and public debate. The success of projects to clone animals, of course, has raised the spectre of human cloning, adding yet another dimension to the controversies that surround the remarkable growth of biotechnology. Genomics is the most recent of biology's new frontiers. Even as genome sequences of diverse organisms pour into computerized databases, an end appears in sight even for the enormously complex (and expensive) human genome programme; a project that may prove the final testing ground for both sequencing technologies and the computational handling of sequence data. Genomics appears to have opened up apparently limitless possibilities, not the least of which is the ability to define the extent of our understanding (or indeed our ignorance) of an organisms genes and their products.

Even as the scope of modern biology explodes, the great potential for converting scientific advances into technological successes has brought a new (and at least in India, a somewhat unfamiliar) commercial dimension to the practice of biology. How many of the claims of modern biotechnology will stand the test of time and the demands of the marketplace remain to be seen. Nevertheless, the promise of the field is enough to attract both investors and speculators and to act as a magnet for students hoping to embark on careers in an area that appears to be exploding.

In India, the government moved quickly to create a Department of Biotechnology (DBT, initially the National Biotechnology Board, NBTB) in the 1980s, whose initial efforts to promote research and training in areas of modern biology were largely successful. Unfortunately, today. DBT is stretched thin; its activities encompass human resource development, funding of biological research, running several frontline institutions and serving as the nodal agency for coordinating many emerging ('mission mode') programmes of practical utility in the areas of agriculture and medicine. DBT pioneered the M Sc Biotechnology programme in a few chosen Universities; a postgraduate initiative that provided an entry into biology for students initially trained in all areas of science. Ample funding for infrastructure and a largely committed faculty ensured that these programmes, in the early years, provided a much needed infusion of bright and motivated students into the area of biological research. Unfortunately, the initial momentum has not been maintained at many centres. The problem has been compounded by a mushrooming of 'Biotechnology' courses, including programmes even at undergraduate (B Sc) level. Part of the blame must rest on the ill advised move of the University Grants Commission to imagine that 'Biotechnology' would be a 'vocational course'. A consequence has been that we now have a surfeit of indifferent biotechnology programmes, which focus neither on basic modern biology nor on its technological fallout. To some extent the problems of biotechnology courses resemble those of management or computer applications courses. There are

a few good academic programmes buried in a sea of indifferent and sometimes inappropriately advertised courses.

The fundamental problem, of course, is that it is hard to define the discipline of biotechnology. Botany, zoology, microbiology, fermentation technology, molecular biology, biochemistry, biophysics and even some fringes of chemistry emerge in biotechnology courses. Inevitably the precise slant of a 'biotechnology' programme is largely determined by the composition of the departmental faculty. Many institutions have simply chosen to rename old courses, while others have already begun bachelor's and master's degree courses, without any formal teaching department in place. Some of these 'virtual biotechnology' programmes are so seriously flawed (sometimes entirely dependent on guest lecturers), that they can only be viewed as a marketing strategy to attract students to courses, that have no academic merit. The approach of universities and autonomous educational institutions in starting new programmes with complete disregard for academic considerations merits serious attention.

P. Balaram

CURRENT SCIENCE

SUBMISSION IN ELECTRONIC FORM

Authors who have been informed of acceptance of their manuscripts may send the final version in electronic form on floppy diskette (3.5" preferred; IBM PC format only, *not* Mackintosh). The text of the manuscript only should be supplied as a plain ASCII file with no formatting other than line and paragraph breaks. (WordStar 5.5 or 7.0 and Microsoft Word for Windows 6.0 are acceptable, but ASCII is preferred.) A hard copy of the text, with all typesetting information (italics, bold, mathematical type, superscripts, subscripts, etc.) must accompany the electronic copy. Tables and figures must be supplied only as hard copy. The diskette must be labeled clearly with the following: manuscript number, file name, file information (ASCII or WordStar, version number, etc.).

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CURRENT SCIENCE

Volume 76 Number 8 25 April 1999

EDITORIAL

Promoting Young Scientists

Encouraging young scientists is a big business these days. One of the most popular methods is the institution of awards. The Golden Jubilee of India's independence provided a wonderful opportunity for the science departments of the government to emphasize their commitment to the cause of promoting science. The Department of Science and Technology announced the Swarnajayanti Fellowships, a scheme that would provide a few chosen individuals under 40, with a handsome research grant and an even handsomer personal fellowship, which was guaranteed to make them the objects of envy, even to the heads of their institutions. The Department of Biotechnology (DBT) also instituted, somewhat less well endowed, fellowships to mark the jubilee. The first round of Swarnajayanti Fellowships was important (?) enough to merit a Prime Ministerial presence at a function. The publicity ensured that the DBT would follow suit; a new set of 10 National Bioscience Awards for scientists below 45 has just been announced, each of which carries a personal award of Rs 1 lakh. This, in fact, is of the same value as the much older Shanti Swarup Bhatnagar (SSB) Prize of the Council of Scientific and Industrial Research (CSIR), where each year one or two scientists are chosen in each area of science. DBT's 'new initiative' will overlap to a considerable extent with the SSB award in the Biological Sciences. While CSIR's award numbers are limited, the DBT is far more optimistic in its ability to locate scientists worthy of such recognition. Undoubtedly, as numbers increase, credibility of the selection process (which has never been high) will diminish even further. Ironically, some years ago 45 was considered as a respectably advanced age and the Bhatnagar award served to dispell 'middle aged blues'; today at 45 many awards celebrate the 'youth' of their recipients. There are also real 'young scientist' awards given away by CSIR, ICAR and INSA (which invented the process), with upper age limits of between 32 and 35 years. Some of these awards are intended to provide encouragement to promising 'young scientists'. Others, like the Bhatnagar award are intended to be a recognition of relatively early achievement. Over the years, the proliferation of awards – with differing age restrictions – have ensured that the dividing line between promise and performance has become extremely blurred.

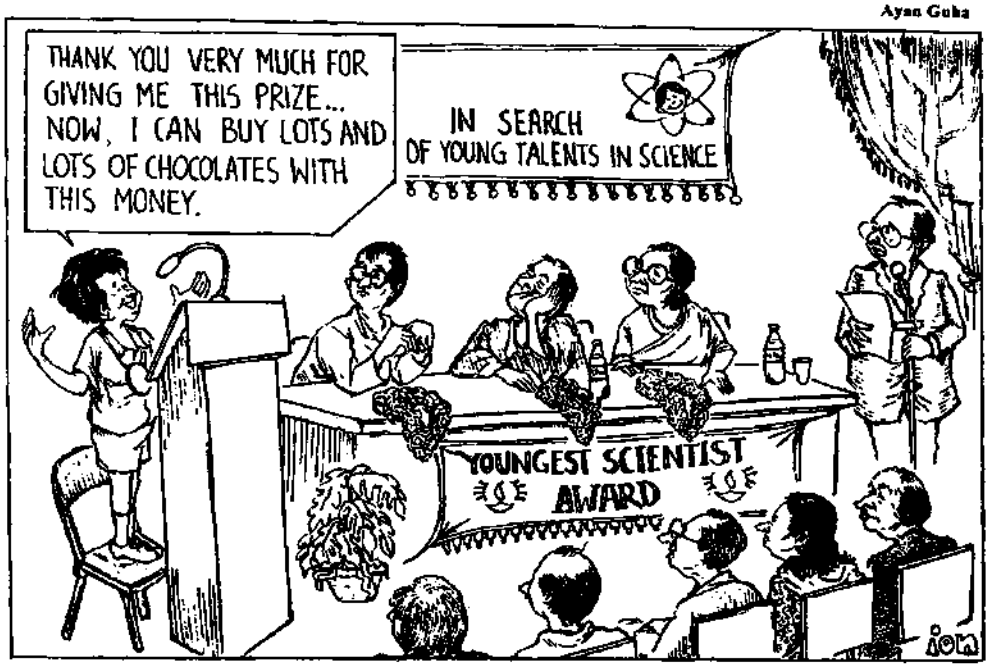
The Academies have not been far behind in the general desire to promote the young. Confronted with an ever-increasing 'average age' of their fellowships (largely a consequence of a general and desirable increase in longevity), the Academies have often frowned gently on attempts to elect those beyond 50 to their club. This preoccupation with 'young scientists' has left many accomplished and scholarly middle aged and even 'old' scientists completely in the cold. Age in itself cannot be an over-riding consideration in determining scientific recognition. The bottom line must be performance and achievement; hard comparisons cannot be avoided.

While the new spate of awards are meant to encourage those who are already committed to a career in science, there are new inducements provided to the young to join science. The announcement of the Kishore Vaigyanik Protsahan Yojana (KYPY) initiative by the DST is an end of the millennium revival of an old and almost forgotten scheme, the National Science Talent Search (NSTS), administered for a long time by the National Council for Educational Research and Training. The KYPY will now provide up to 60 fellowships of Rs 2000 per month for Class XI and XII students (Rs 3000 after Class XII), opting for science. In the Engineering and Medicine streams the fellowships are higher (Rs 3000 pm), while the number of fellowships (25) are smaller. The hope is that these financial inducements will prove attractive enough to entice 'bright young students' to research careers. The experiences of the earlier NSTS scheme were mixed. It remains to be seen if money alone, whether in the form of the Swarnajayanti Fellowship or the KYPY scheme is a sensible solution to enhancing the quality of science in India. An interesting feature of these schemes is that they appear to emanate with a ministerial pronouncement; apparently little

thought having been given to the mechanics of administration or the consequences.

The proliferation of special awards coincides with an increase in the difficulty of getting normal research grants, because of administrative and financial constraints. Curiously, the agencies charged with promoting science do not find this situation incongruous. The prestige and credibility of most awards is determined by the accomplishment of the awardees. The danger that indiscriminate multiplication of awards will erode their significance appears to have gone unnoticed. Even more importantly, awards given to the undeserving send out a completely wrong signal to the community of 'young scientists', whom the agencies are trying to woo. The government's science departments would be better off addressing their primary responsibility; the fostering of cutting edge research in our many scientific laboratories by providing meaningful support for the institutions themselves. Maybe it is time for yet another 'millennium initiative'.

P. Balaram



CURRENT SCIENCE

Volume 76 Number 9 10 May 1999

EDITORIAL

Sanctions

It is almost a year since sanctions were imposed by the United States, Japan and some countries of Western Europe, following the Pokhran explosions. While the sanctions were primarily targetted at laboratories associated with strategic research programmes, there have inevitably been some effects felt by institutions largely concerned with basic research. For example, the Tata Institute of Fundamental Research (TIFR) in Mumbai is almost exclusively devoted to 'blue sky' research, but is funded directly by the Department of Atomic Energy (DAE). The historical association of TIFR and the DAE, of course, dates back to the days of Homi Bhabha. This relationship has been a very fruitful one, with the TIFR budgets being insulated from the many uncertainties faced by institutions outside the DAE umbrella. Unfortunately, connection with the DAE now means that even scientists doing basic biology, chemistry or physics (mathematicians, fortunately are apparently immune to the effects of sanctions) face great difficulties in getting chemicals and equipment from several overseas sources. Even the Tata Memorial Hospital, which is exclusively devoted to cancer therapy, appears to be suffering because of its DAE connection. Sanctions have also led to completely unacceptable behaviour on the part of several companies which refuse to provide warranty replacements for expensive equipment which has already been paid for. The official position (understandably) has been that sanctions are having no effect. In reality, however, there will be pockets of scientists whose individual researches are being affected by the non-availability of materials.

There is little doubt that sanctions are a knee jerk reaction to the nuclear explosions. Are wide ranging sanctions justifiable? How do we minimize the impact of sanctions? These questions should be broadly addressed. It does not help to say that sanctions do not exist or that they are without any effect on our scientific research. Even more importantly we must assess our own response to countries and companies which have proceeded to impose restrictions on the flow of materials and sometimes, even on the travel of scientists. Several months after the explosions a group of American high-energy physicists from Fermilab were denied permission by the US Department of Energy to attend a conference at TIFR, an action clearly inconsistent with the norms of international scientific exchange. What has been really surprising is the complete silence of Western scientific bodies to actions which are intended to curb free, unfettered international scientific discourse. Even the voice of individual scientists has been strangely muted. A notable exception was an editorial in *Science* (1999, 283, 184) by Irving A. Lerch which did conclude: 'The efforts of government officials to slow this engine of exchange under the disguise of preventing weapons technology transfers threaten debilitating consequences for science here and abroad'. The present indifference of international unions and scientific societies is in marked contrast to their apparently activist stances in the past. Many of these bodies have always been the most vocal in their protest, when there has been the slightest indication that freedom of travel to scientific meetings has been curbed. Most notably, during the apartheid era, the scientific unions invariably dominated by the West, were foremost in demanding the entry of South African scientists (white, of course) to conferences in India. The countries that are the foremost proponents of sanctions are also the very same countries which have no difficulty in shipping scientific equipment and chemicals to many countries possessing nuclear weapons (and research capabilities in this area).

What have our scientific bodies been doing to build pressure in the international scientific community against sanctions? From the public record at least, nothing seems to be happening. The Indian Science Congress played host in January at Chennai, to Bruce Alberts, the President of the US National Academy of Sciences (NAS). Whether the position of the NAS on sanctions was probed or not is unclear. There have been no public statements on this issue, which are intended to give the feeling that Indian scientific bodies are going to lobby vigorously with their counterparts elsewhere. The Indian National Science Academy which holds a unique position in representing this country on international unions has a special responsibility in coordinating efforts to rally support in the international scientific community against sanctions, which affect research in all areas, indiscriminately. At the same time we must also carefully evaluate the pros and cons of many international exchange programmes that we have with the countries imposing sanctions. It is also time that efforts are made to help those Indian scientists, whose work is affected, by some collective action.

P. Balaram

CURRENT SCIENCE

Volume 76 Number 10 25 May 1999

EDITORIAL

Peer review

The noun 'peer' (in the context of science and evaluation) is defined by the *Oxford Dictionary* as 'one equal in civil standing or rank; equal in any respect'. Among scientists the term 'peer review' is often the subject of much discussion. Artists, musicians, authors and poets are evaluated by their public following, who do not usually pretend to any special skills necessary for sitting in judgement. In fact, specialist critics who write in the literary sections of newspapers and magazines can be devastatingly critical, without having the slightest influence on the public perception of an author's work (and most importantly on the sales of a new book). But in science, matters are different. Scientists invariably pride themselves that they are evaluated by their 'peers'; and not by a public mass, ignorant of the subtleties of their craft. Science has become so specialized that many scientists are firmly convinced that anyone who does not belong to a specific sub-discipline is hopelessly incapable of making any meaningful judgement. 'Peer review' is the accepted norm and is a process which permeates every area of scientific activity, including evaluation of papers for publication, grants for funding and decision making on awards and rewards of every kind. In evaluating academic scientists for promotion, their performance is almost always judged by peers; immediate superiors are expected to play only a minimal role. The composition and constitution of peer groups thus, assumes very great significance. An intriguing feature of scientific activity is that many scientists reach a stage where they seem to be on both sides of the fence; one day they are the judges, while on the next they are supplicants. Indeed, the only members of peer groups who do not seem to have a substantial conflict of interest are scientists, who have retired from active science, but are still highly visible on evaluating committees. Since peer decisions impinge on every aspect of a scientist's professional life, it is not surprising that most researchers have a very strong view on the practice of peer review.

The editorial offices at science journals are usually caught in the crossfire between authors and referees. Most authors (probably, rightly) are convinced of the merit of their work and the lucidity of their presentation. Many respond in a positive manner to criticism of their manuscripts by referees and indeed even acknowledge reviewers in their final published work. Some are overly sensitive and often trigger a duel with referees in which the editorial office acts as a passive spectator (and facilitator). Authors who are most sensitive to criticism are often the most critical of referees, themselves. Woe betide the editor who accepts a paper against their recommendation. In discussing Third World science (and publications) there has been some debate on whether the process of peer review (which of course, includes editors) at Western journals is biased against authors from developing countries. Western editors strongly deny any bias and there is ample evidence that the best of Third World research still appears in top ranking Western journals, suggesting that geography and national origin may not unduly influence the peer review process.

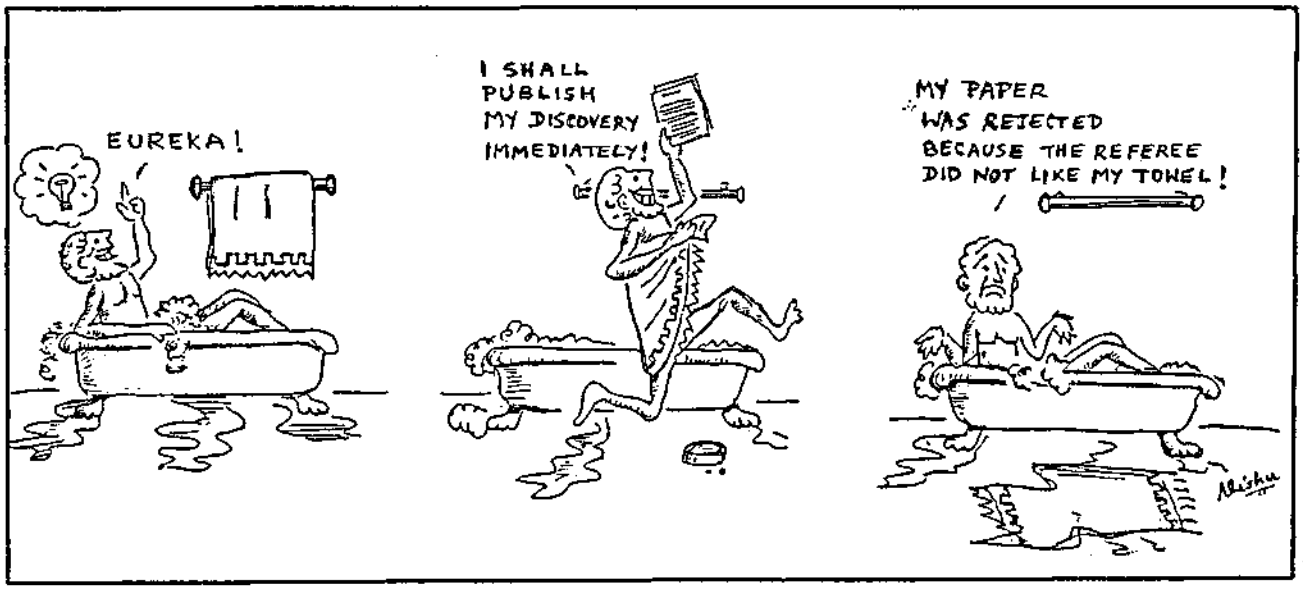
The peer review process in our journals (*Current Science* is a prime example) is also the target of sniping. A recent (unfortunately, anonymous) letter is from a disgruntled reader (fellow scientist and presumably sometime author) who criticizes a published paper, charging editors with using 'mediocre referees' who allow poor papers to appear in print. He attacks referees and charges them with prejudice: '*It is well known to the whole world that many referees of Indian journals are a prejudiced, lazy lot who often do not bother to read critically the manuscript sent to them spending time and energy, but pass a manuscript as suitable for publication merely on the basis of the institution from where it comes. For instance, if a paper is from one of the Central government supported institutes, then even without reading the article carefully, it is passed for publication. ... But the same hypocritical referee may retain a*

well written article of international standing coming from a less known institution or student of science, for months and years together without commenting and finally under pressure from the author (routed through the editor of the journal of course, because the referee is kept anonymous), sends back the manuscript after making some silly comments, concluding that it is not fit for publication.' Harsh words, indeed, but have not the same thoughts crossed our minds when manuscripts are returned after apparently superficial and frivolous review. Our anonymous correspondent offers to reveal his name, provided the veil of anonymity is lifted from referees also. Journals the world over have debated the issue of anonymous reviewing, but the vast majority of the most prestigious publications have not come up with a superior alternative.

In the case of grants (and many awards) peer review procedures are often more transparent. Even though anonymous referees reports are sought, most often the decision making is done by committees, whose membership is no secret. Here the 'peers' are out in the open, but the rumbles of discontent with the system are no less. The problem of course, is that even collective decision making is subjective and for every winner there are several losers.

The peer review process is unique to scientific activity and sometimes the 'peer groups' can be separated by continents. Citation analyses can often reveal transnational groups; clusters of geographically separated scientists who work in the same area and are fully aware of each others work. Indeed, editors often choose referees by scanning reference lists, although this primitive practice is being replaced by computerized databases of potential 'peers'. Despite all its obvious imperfections it is unlikely that peer evaluations are likely to be replaced by a radically new system.

P. Balaram



CURRENT SCIENCE

Volume 76 Number 11 10 June 1999

EDITORIAL

Symposia and congresses

Symposia, seminars, workshops, discussion meetings and congresses are the life-blood of a scientist's existence. Scientists are social animals and congregate frequently. Despite the enormous number of journals and the internet, the most direct way of information transfer is by personal contact. Not surprisingly, many scientists flock to symposia quite regularly. While the small meetings attended by relatively few scientists can be academically intense, the large congresses may provide mixed experiences. Plenary lectures are usually delivered by well-known speakers, generally stars in their chosen fields. These are well attended and can prove inspirational to younger scientists hearing the lectures for the first time. For more experienced congress attendees the plenaries invoke a sense of *deja vu*. This is largely because the superstars are frequently featured as lead speakers and quite often have little time to change their lectures and add dramatically new material. Indeed, growing invitations to lecture can be a sign of scientific success, inevitably leading to larger absences from the laboratory.

Symposia can also provide insights into the importance of presentations in determining perceptions about a scientist's work. Sitting through interminable sessions where unintelligible speakers run past their allotted time (while session chairpersons quietly wring their hands in despair), is a professional hazard in a scientist's life. Very often interesting results are hidden by badly prepared and poorly articulated talks. Sometimes, the lack of clarity in presentation is a consequence of muddle-headed thinking, which extends to the research itself; most often it is merely a reflection of the poor preparation of the speaker. Indeed, the importance of public presentations is being stressed so much in today's management circles, that scientists might try to learn a trick or two from marketing men. Scientific research and the results obtained by hard labour in the laboratory may also be viewed as commodities, whose importance, utility and reach, must be effectively sold. While publication of results is the ultimate mechanism for dissemination, oral presentations of the work are an important forerunner. Participating in seminars and symposia is thus, an essential feature of scientific life and an important element in the training of new entrants to the profession.

Having conceded the importance of congregations of scientists, it is worthwhile asking a more provocative question. Do we have too many scientific meetings, national and international? There has been a proliferation in the number of scientific societies and academies over the years. Every subfield now has its own specialist society. In interdisciplinary areas the same people are members of several societies. The Academies have their own meetings, often as many as two every year. These somewhat 'elitist' gatherings, give an impression of belonging to another, more genteel age, where the typical audience was expected to have a broad understanding of diverse areas of science. The reality today is that at any instant, only a very small fraction of the audience seems to understand what is being discussed by speakers, who make few concessions in being intelligible to non-experts.

In trying to establish and foster international connections many societies also attempt to act as hosts for major international congresses. This is akin to trying to host major international sporting events. Office bearers make trips abroad and make presentations to international councils, which then choose a venue from many claimants; a process not too different from the way the sites for the Olympic games are chosen. (Fortunately, scandals like the one currently afflicting the International Olympic Association are unheard of.) In India, the only city, which seems capable of hosting such mega scientific events is Delhi, although there will be others in the not too distant future. The large congresses usually have in excess of a thousand participants and are generally held in a five star ambience. Occasionally major programmes are held in public auditoria or even outdoors. An unpleasant feature of these congresses is that attendees are required to pay exorbitant registration fees, partly necessitated by the opulence of the five star surroundings. Government grants (subsidies) to these meetings sometimes run to tens of lakhs of rupees, inevitably raising the question – Is the effort and expenditure worth the outcome? Students who will benefit the most from hearing the invited speakers

cannot usually attend in large numbers – travel and registration costs being prohibitively expensive. Do organizers benefit academically by helping to host large congresses? Probably not, since they are usually too busy with the mechanics of running the show; arrangements for housing and feeding delegates, putting together cultural programmes and tours are major responsibilities. Professional management of scientific meetings is unheard of in our surroundings. Most attendees at these congresses seem to be well-travelled, symposium-goers, many of whom appear to be there so that another by-line can be added to their curriculum vitae. In the larger meetings more people can be found outside the lecture halls than within. All this leads to an inevitable question: Should we have so many scientific meetings? Should we invite large international congresses to India, when the councils of societies involved insist on unaffordable registration fees? The most influential international unions are dominated by the First World; interests of the less well-endowed countries are hardly a matter of pressing concern. Should government tighten the purse strings in providing subsidies for these international jamborees? At a time when funding for research and support for science seems less than optimal, academies and societies would do well to practice a little austerity in the conduct of meetings.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Who's afraid of impact factors

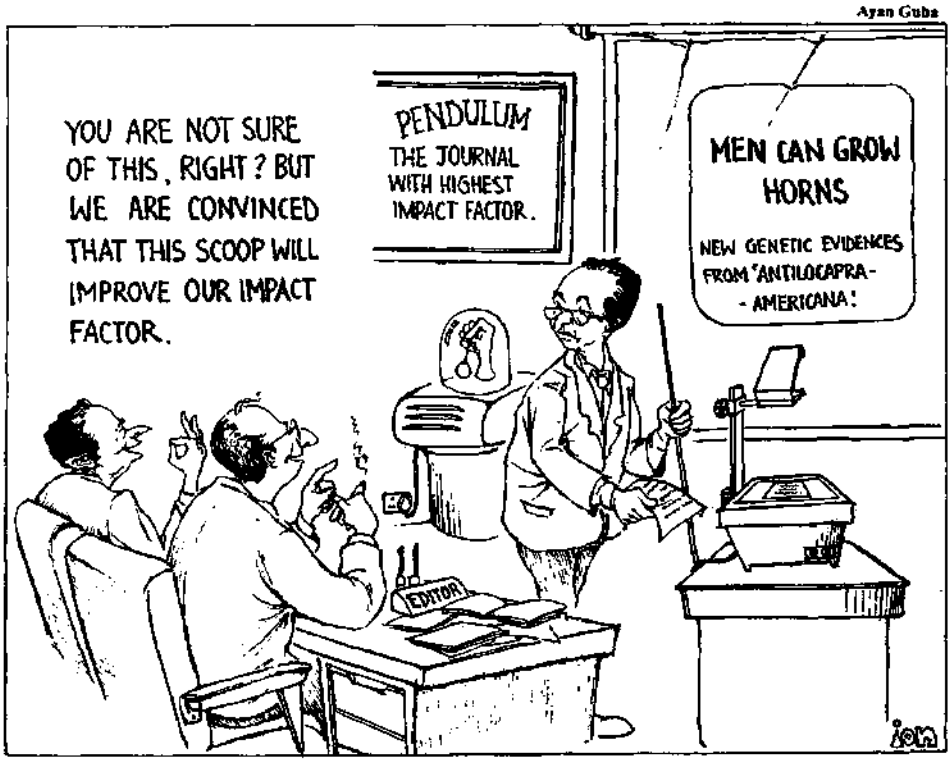
Being an editor of a journal is a difficult (and uncomfortable) business at the best of times. Nowadays, it is even harder thanks to the many precisely-defined measures for assessing journal quality. It is hard for an editor to escape the penetrating question – 'What is the impact factor of your journal'. Presumably, the editors of the glamorous journals (*Nature*, *Science* and *Cell* amongst them) smirk and preen when asked this question. (Indeed some of these journals proudly announce their ranks in colourful advertisements.) Those in charge of lesser known periodicals merely pretend not to have heard the question and heartily wish that their tormentors would one day be appointed as journal editors. It was thus comforting to read that impact factor calculations provoke even the editors of well established, highly regarded professional journals like the *Journal of Bacteriology* (see G. C. Walker, *J. Bacteriol.*, 1999, **181**, 1–3). A recent letter in *Current Science* quotes from the *J. Bacteriol.* editorial and calls attention to the many 'forgotten citations' which are excluded from the calculation of impact factors (J. Gowrishankar, *Curr. Sci.*, 1999, **76**, 1424). This letter further highlights the fact that a science establishment enamoured with impact factors forgets the real purpose of research and 'devotes itself not to making the important measurable but to making the measurable important.' (P. A. Lawrence, *Nature*, 1999, **397**, 487–489). Harsh comments indeed, about a parameter that is now an almost universally accepted measure of a journal's importance.

A little understanding of the formula used for impact factor calculations may help those who wonder why journals cannot easily enhance their ratings. The impact factors announced in 1999 will consider all citations (in journals covered by the *Science Citation Index*, of course) in the years 1997 and 1998, to papers published in the journal in 1995. The 'forgotten citations' are to those made to papers published in the same journal before 1996. Thus, there is an element of immediacy, a touch of fashion and maybe a bandwagon effect in operation, when impact factor counts measure short-term citation rates. Fast moving fields, areas where hundreds (and sometimes thousands) of groups hunt for gold must be well represented on a journal's pages, in order to assure instant citation success. In the recent past there have been many such rapidly moving fields of research, which have dramatically illustrated how 'hot areas' can explode and later subside. The physicists will recall the heydays of high temperature superconductivity; the fullerenes are still fresh in the memory of chemists; while biologists seem to have more long lasting fashions – apoptosis, signal transduction and cell cycle research staying at the top of the charts for some time. The high impact journals (particularly the interdisciplinary ones) must then carefully select the papers that they publish. Editorial decisions can then be guided by perceived importance in an immediate context. The more staid professional journals can hardly subscribe to a scientific fashion show. Not surprisingly, the *J. Bacteriol.* editorial emphasizes that the journal publishes 'truly the best papers in the field and does not bias "decisions by considering the perceived popularity of the topic"'. Since impact factor calculations use short-term citation counts in the numerator and the total number of papers published annually by a journal in the denominator, periodicals which publish a large number of papers are almost always at the bottom of the ranks. The moral, necessarily, is simple – publish fewer papers and try to publish the best (or at least the most 'attractive' papers) in rapidly moving fields in order to enhance impact factors.

In the Indian context, impact factors have assumed a larger-than-life role. All kinds of new (and poorly conceived) indices are being devised, not for evaluating journals but for assessing science and scientists. It is

common to see publication lists being scanned and a scientist's worth assessed by computing an 'average impact factor', based on the journals in which an individual's papers have appeared. In these calculations, the possibility is not even considered that some of the papers published in high impact journals may not be cited, while those in lower impact publications may in fact, have attracted attention. From individuals to institutions is only a short jump. Many analyses of publication profiles currently doing the rounds in scientific circles are characterized by a woeful lack of understanding of the databases used, the parameters derived and most importantly, a complete ignorance of the processes of science. Not surprisingly, ill-informed analysts then draw erroneous conclusions about the scientific health of institutions, departments and individuals. Scientometrics is a powerful tool, but trained practitioners are hard to find. In making decisions about where to publish, prospective authors are sometimes misguided into choosing journals based on 'impact factor criteria'. The simple rule is that papers should be published where they are noticed by others in the field. The discussion on citations and impact factors is never ending but an answer to the question posed in the title is clear – journal editors, of course.

P. Balaram



CURRENT SCIENCE

Volume 77 Number 1 10 July 1999

EDITORIAL

A profusion of academies

A senior scientist recently asked me a question – 'Why are there three Academies of Science in India?'. The question was probably rhetorical, but it dawned on me that I did not know the answer; the reasons were presumably buried in the forgotten past of Indian science. The origins of these Academies were highlighted some years ago in this journal (*Curr. Sci.*, 1994, **67**, 633–676) by S. Ramaseshan. Re-reading these pages left me somewhat bemused. Chronologically, the first Academy to be registered with a 'national name' was the Indian Academy of Sciences (Bangalore) on 27 April 1934. The Academy of Sciences of the United Provinces of Agra and Oudh (registered on 4 December 1930) changed its name to the National Academy of Sciences (Allahabad) on 5 December 1936. Sandwiched between these events the National Institute of Sciences held its inaugural meeting at Calcutta on 7 January 1935; another name change transformed it into the Indian National Science Academy (Delhi). The events of 1934–35 were undoubtedly a consequence of the C. V. Raman–Meghnad Saha equations, which dictated that two separate entities be born to accommodate these larger than life personalities. While Raman presided over the Bangalore Academy for the rest of his life, Saha presided over the Allahabad Academy as its founder president and over the National Institute of Sciences as its first Indian president in 1937–38.

In subsequent years other Academies have sprung up to meet perceived needs in various fields like engineering, medical sciences, agriculture and in even more specialized disciplines. An important feature of Academies is their need to be exclusive with a fellowship that is elected by existing members. This is the model followed the world over. Exclusivity, automatically implies that election to the fellowship reflects a level of peer recognition, that presumably follows when a scientist reaches a certain desirable level of scientific accomplishment. The reality, of course, is otherwise. Elections (all kinds) invariably become political affairs with the competing (and often contradictory) interests of many subject groups having to be reconciled. After all, there must be a broad representation of disciplines in a science Academy; the only way for the Councils to achieve this is to impose a subtle 'reservation system' that ensures a moderate level of disciplinary parity. An inevitable consequence is that new Academies spring up to cater exclusively to disciplines, which appear underrepresented in the original bodies. This proliferation of Academies would be of little consequence if they were self-sustaining bodies with different sets of members, operating primarily to promote science in their surroundings. Unfortunately, the science Academies in India need substantial government grants to sustain their activities. It is therefore inevitable that the issue of relevance of these activities to the promotion of science and its beneficial applications, must be addressed. The most important activity of the Academies (unfortunately) appears to be election of new members and the conduct of annual (and biannual) meetings and the arrangement and support of symposia. The preoccupation with elections is not an Indian phenomenon alone. Richard Feynmann is reported to have resigned from the U.S. National Academy of Sciences because he felt that the organization did little else (an overstatement no doubt) than elect new fellows.

To an outsider it would seem that Academies of science must have some role to play in the manner in which Government decides science and technology policy. Academies, whose fellowships are presumably a storehouse of scientific knowledge, should, in principle, be best suited to an advisory role. Indeed in the U.S. the National Academy of Sciences and its sister bodies, the National Academy of Engineering and the Institute of Medicine (together constituting the National Research Council), serve as an important advisory arm of government, broadly representing the scientific community. Two of our Academies, the National Academy of Sciences (Allahabad) and the Indian Academy of Sciences (Bangalore) largely confine their activities to arranging scientific meetings and producing journals. The latter, in recent years, has also introduced new initiatives for promotion of science, by starting the journal *Resonance* and by providing opportunities for students and teachers

to spend short periods in research laboratories. The truly 'official' body is the Indian National Science Academy (INSA), located in Delhi, 'with its overwhelming air of officialdom' and surrounded by the almost suffocating air of one of the capital's busiest traffic intersections. INSA represents this country in all the world's scientific councils, sends delegations abroad, signs memoranda of understanding for international exchange programmes, distributes awards and indeed acts as an extended arm of government. INSA does not, however, have any apparent official standing as an advisory body of the government. Thus, on important issues which concern scientists and the community at large, like atomic power (in all its forms), genetically modified crops, hazard assessment related to earthquakes and floods, the declining quality of science education, funding of science and a host of others, there is no reasoned and largely acceptable view. Indeed, debate and controversy are usually avoided in the Academies, making them sterile (and elitist) clubs, which serve little or no useful role in the context of the larger scientific community in India.

Why then do we have so many Academies? Even if they cannot merge (see S. Ramaseshan, *Curr. Sci.*, 1994, **67**, 633–636 for an account of an attempt at unification in 1947), there must be efforts to act coherently and avoid the impression that the multiplicity of Academies reflects a fragmented and poorly organized structure of science in this country. Eschewing overlapping activities and reducing the number of meetings may be a good beginning. Academies of science must also collectively learn to lobby for greater support for scientific research, work towards enhancing the public perception of science and contribute effectively towards enhancing the quality of science in India.

P. Balaram

CURRENT SCIENCE

Volume 77 Number 2 25 July 1999

EDITORIAL

Supporting retired scientists

The Royal Society recently advertised an interesting scheme – 'A Scientific Relief Fund' (*Nature*, 6 May 1999), essentially a benevolent fund for old scientists. While researchers and their families, fallen upon hard times, could seek succour, the notice also offered financial assistance to retired scientists, who wanted to continue their academic activities. Coincidentally, at the same time there appeared the news that Sunil Gavaskar was donating the proceeds of his many awards to a fund which would assist indigent cricketers. This was presumably intended to support the large numbers of 'reasonable' players who struggle to eke out an existence as professionals (in a short career span), without making it to the top ranks, where money, sometimes, is more abundant than runs or wickets. Scientists, in our country today are 'professional' in that they (like almost all others in government) are paid regular, inflation indexed salaries and are covered by pension schemes, which provide some insurance against penury in old age. No longer are academicians paid much less than everybody else in government, such that a researcher must be financially hard pressed in order to pursue his chosen calling. That privilege is now reserved for artists and authors, until they hit the jackpot, of course.

Not so long ago, college and University teachers were grossly underpaid and covered by retirement plans, which were hardly calculated to be attractive. Now successive Pay Commissions have ensured that individual bank balances are more important than institutional (or indeed governmental) finances, with the result that large numbers of reasonably well paid researchers work in laboratories with little money left over to support academic activities. For a substantial number (unfortunately) of 'scientists' this is indeed a comfortable state of affairs. There is a ready-made excuse for non-performance – 'there are no funds or facilities within the institution for research'. Many government laboratories spend a frightening proportion of their budgets on salaries to pay their staff; they then turn their scientists loose to fend for themselves in competing for research grants from government funding agencies, which were originally set up to promote scientific activity in the University sector. In putting on a brave face, we are often told that 'competing for peer reviewed grants is a good thing' and that the responsibility of national institutions ends with providing salary support (and utilities like power, water and shelter, wherever available).

Returning now to the Royal Society's benevolent fund, the one conclusion that may be drawn is that Indian scientists who retire nowadays from their positions are unlikely (unless they have been profligate) to be in dire financial straits. What kind of support would then benefit them? Although the age of retirement has been enhanced recently from 60 to 62 years, in many academic institutions (mostly centrally supported), even this age finds many scientists (and others) in the pink of health and energetic enough to resent having been put out to graze. In the United States there is no formal age of retirement in federal institutions and many Universities place no limits on the age of their faculty. They do however, subtly and effectively, induce the non-performers to voluntarily (?) leave; a system that of course, requires that tenure be initially granted after a tough and uncompromising review. Europe still maintains an age of retirement, although it is somewhat higher than in India. There have consequently been instances of superstars of European science, who have moved to the United States in their twilight years.

What inhibits our support of productive scientists even after they formally retire? Our system now suffers from a chronic inability to make credible judgements. Beginning at the start of a scientist's career, all the way through to retirement, we operate in a wonderland, where no distinctions are made between performers and non-performers, workers and shirkers, honest and dishonest, gifted and mediocre. We therefore find it extraordinarily difficult to make any judgements regarding the selective support of the research of scientists who are intellectually active even when they reach formal

retirement age. With the manpower inputs to scientific institutions diminishing in quantity and quality, it would appear futile to summarily withdraw facilities to the best of our scientists. Unfortunately, every scheme introduced to sustain, scientifically, the more productive individuals has floundered because of our inability to be brutally judgemental. Emeritus scientist schemes are now viewed more in terms of additional financial support as personal honoraria, rather than as a means to facilitate academic activity. This view then results in judgements that are based more on friendships and emotions, than on cold academic calculation. Reemployment schemes at some Universities have been in vogue for many years; these are a poor cover for their real purpose – a blanket enhancement in retirement age, with complete disregard for performance.

The Royal Society's scheme does point to an area where science administrators would do well to ponder. Can separate funding mechanisms be created to provide research support to many active and scholarly 'retired' scientists, which would address the important issue of research grants rather than honoraria or fellowsh Indeed for both young and old scientists, the gov ment agencies appear to feel that personal payment a greater inducement to work, than research facili Grant mechanisms for 'retired scientists' must nece ily operate significantly more expeditiously than nor Time is, after all, literally running out. For the r who undoubtedly endorse the idea of ever increa honoraria and salaries, one can only point out that demic research is still a very rare profession, where practitioner gets paid to do what he or she enjoys. unfortunate reality in many of our laboratories is increasing number of scientists who do not seem to joy their research, but are blatantly careerists.) I more importantly, the researcher is very often his o own master. Under these circumstances, would it be much to ask for a greater emphasis on research sup and enhanced facilities for research, even if this come at some personal cost.

P. Bal

CURRENT SCIENCE

Volume 77 Number 3 10 August 1999

EDITORIAL

Deserting disciplines

'Brain drain' was a phrase popular in the 1960s and 1970s, to describe the flight of trained scientists and engineers to the West, particularly the United States. The term became less commonly used in the 1980s, when it became apparent that our production of engineers and scientists far exceeded the capacity of our institutions to absorb them. Indeed the best of our institutions are quietly reconciled to training research students purely for export purposes. Hundreds of Indian Ph Ds man the benches in the most productive laboratories across the world. Concerns about the exodus of trained manpower have slowly begun to give way to a feeling of pride that Indian institutions produce scientists and technologists, who are capable of competing with the best in the world. In the last decade or so, the worries about the brain drain appear to have diminished, although an occasional scheme, like the Swarnajayanthi Fellowships of the Department of Science Technology, is justified by its ostensible purpose of attracting talented young Indians from overseas, to return to this country. In reality, the scheme has been instantly converted to cater exclusively to scientists already in India; an inevitable and, probably, justifiable transformation.

In simpler times the flight of talented scientists and engineers, to greener pastures, was easy to comprehend; the practice of their chosen professions was more facile in the West. Today there is an even more insidious mechanism that is responsible for the drain of trained scientists and technologists from their fields of specialization, catalysed simply by the growing differential in salaries and employment opportunities in science and technology on the one hand and management, finance and 'software development' on the other.

The training of scientists and engineers in India is enormously subsidized, despite the recent increases in the fee structure of some of our most prestigious institutions, like the Indian Institutes of Technology. Students at the IITs are drawn from a large pool by a tough selection procedure and then trained in various disciplines of engineering and science. Their basic training provides them mathematical and computational skills and a quantitative outlook, which are more than adequate to allow them to perform effectively in the 'softer' disciplines of 'management', accounting, finance and even software development. Indeed, an alarming number of our 'technologists' desert their chosen professions even before they start a career. This is ironic since there is such a headlong rush to enter the IITs and other engineering institutions, only to flee from the discipline once the degrees are awarded. For many years now IIT graduates (and others of the same class) have been entering management schools and financial institutions in the United States. More recently, liberalization has provided ample opportunities for such transitions in India. The pity is that this diversion not only depletes the pool of technologists but also subverts the process for which these institutions were created. Software development and the myriad opportunities for keyboard employment generated by the 'information revolution', have now become major avenues by which trained scientists and engineers are lost to their professions. This situation is compounded by the drain in resources; considerable expenditure is involved in training students at the best institutions, in disciplines that they will desert as soon as their degrees are conferred. While there is a glut of 'software engineers' and financial managers (e-commerce will soon be a buzzword), there will necessarily be a shortage of technologists capable of innovation on the shop floor. Even the laboratories involved in strategic research, like the institutions of the Departments of Atomic Energy, Space and Defence will eventually (if not already) be hardpressed to find 'real' engineers and scientists to solve practical problems of design and construction, associated with their missions.

It is hard today to find good engineering students, who would like to pursue a career in the discipline in which they have been trained. All the streams of aerospace, chemical, civil, metallurgy, mechanical and of course, electrical engineering flow inexorably into the sea of software development. Even the students of masters degrees in science at the IITs are converted into 'software

engineers'. The better and more ambitious students quickly realize that 'management' (particularly, financial) is a better option. In this scenario one cannot but wonder about a brave new world, where everyone is either peddling information, managing money (and people) or modelling production in a virtual environment. The engines of a modern industrial economy are driven by technological innovation. As the pool of good engineers gets increasingly depleted, imitation and import will be viewed as a comfortable, but eventually dangerous, solution. The phrase 'brain drain' may have a different connotation today, but it is a problem that must be confronted.

P. Balaram

CURRENT SCIENCE

Volume 77 Number 4 25 August 1999

EDITORIAL

Purposeless research

In the common perception, chemists have the reputation of mixing, stirring, boiling and generally torturing ('coaxing' is a widely used euphemism) chemicals to form new substances, which sometimes turn out to have useful properties. This view of chemistry finds support in the fact that the discoveries of exploitable properties are often serendipitous. Of course, this picture of chemistry excludes the 'clean chemists' who sit in air-conditioned rooms in front of computer screens or in the company of extremely sophisticated instruments. This category would be indistinguishable from physicists, but for the fact that disciplinary boundaries are sometimes as jealously guarded in science, as the Line of Control. Traditionally, chemistry has been practiced in smelly and messy surroundings which do not appear to be conducive to intellectual activity. Our mental image of experimental laboratories is strongly influenced by college and university experiences. Under these circumstances, I was not too surprised to recently hear the statement (from a fellow scientist, of course): 'Inorganic chemists are prone to make compounds without any particular purpose in mind.' Apart from illustrating the all-too-sharp disciplinary prejudices in science, this statement raises an important issue: Is science practiced without a 'purpose' important, relevant or even useful?

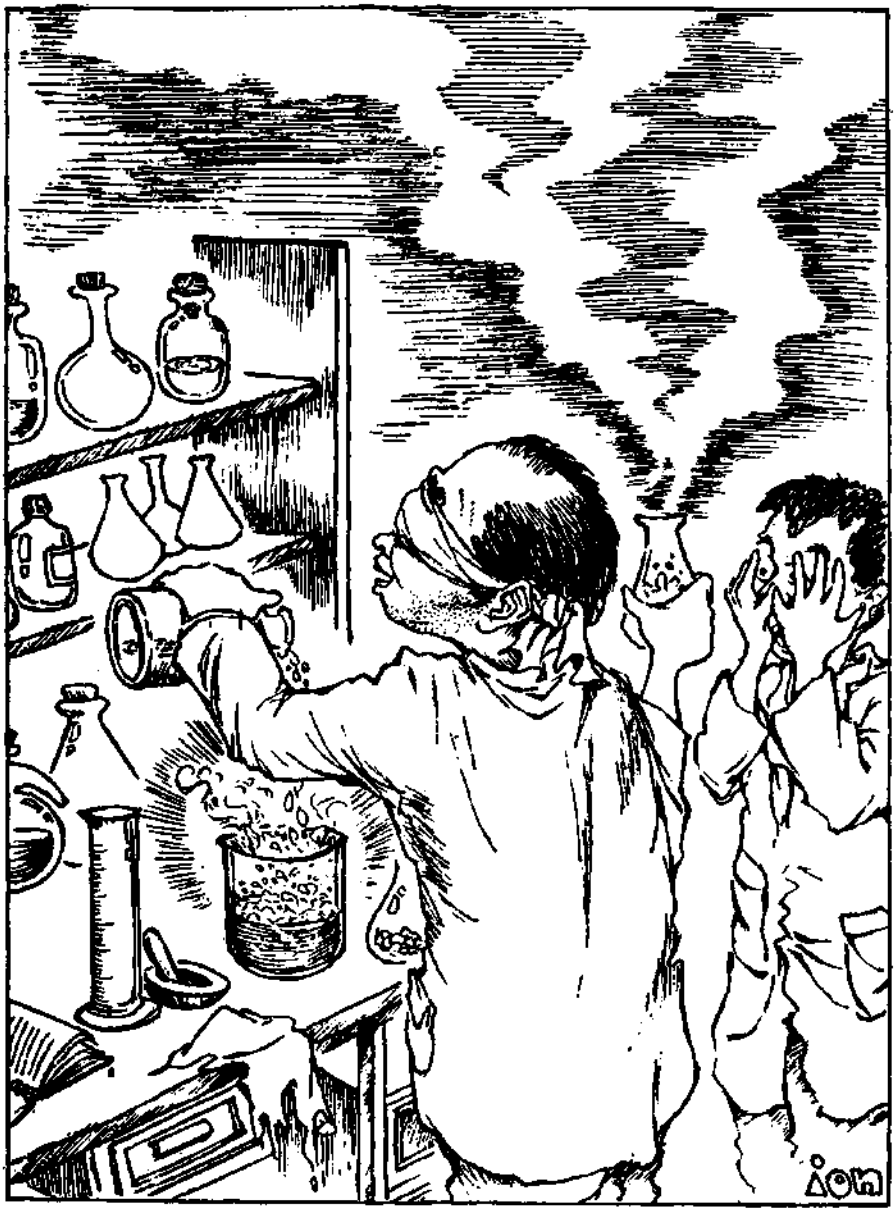
It is almost sacrilegious nowadays to defend the practice of apparently inapplicable ('useless') science. Today most scientific projects are driven by a clearly defined purpose; indeed we have become used to associating a missionary zeal with their protagonists. Government agencies are happiest with targetted 'missions' and rightly so. After all there are a myriad problems of real life that are waiting to be solved by spectacular new technologies that seem to be just around the corner. Scientific investigations must then be nudged in a direction which will eventually give birth to these wonderful technological solutions. Science has also become a big business, with large amounts of public money being spent on research. No wonder that the purse strings are loosened only when 'useful' science is promised. We are also living in times where market forces and global politics dictate the direction of research and development efforts. In such a scenario, is there a place for the 'amateur' scientist who does experiments merely because it is a pleasurable activity? It may be worth recalling that there was a time when the demonstrations and lectures at the Royal Institution in London were witnessed by the rich, indulgent and largely idle English aristocracy; this audience was privileged enough to have watched Michael Faraday make history. The situation today is a far cry from the pre-World War II years, when research was loosely organized, relatively little money was spent and science was the pastime of a few dedicated practitioners. The transformation of scientific research from a low-budget, intellectually driven activity closely associated with teaching, practiced by modestly paid academicians, to a professional and business-like activity has been dramatic. There is a parallel in sports.

Not so long ago, English cricket still had the divisions of 'Gentlemen' and 'Players', the former representing the often aristocratic amateurs who played for enjoyment, while the latter were the more plebian professionals who played for a living. Readers who watched the TV serial 'Bodyline' may have noted the subtle tensions between the arrogant 'amateur' Douglas Jardine and the perfect professional Harold Larwood, in their efforts to tame Don Bradman. Ironically, Jardine's approach would be applauded today as ruthlessly 'professional'. Tennis too had its divisions, with hallowed tournaments like Wimbledon, being the exclusive domain of amateurs. But professionalism was on the rise by the 1960s and today there are no real 'amateurs'. Science too has gone the same way, the transition more gradual; the rewards and the public interest are, of course, much less.

But to return to the original theme; must every laboratory experiment be the result of cold calculation and must we only celebrate the brutally efficient, professional successes of science? Is there no room for those

who would like to pursue a hobbyist's approach to science? In these commercial times it seems incongruous to suggest that doing experiments for fun is justifiable. After all, a hobby cannot be subsidized by public funds. But, maybe there still is a place for an old fashioned, amateur's approach, within the precincts of academic institutions, where many young minds are still to decide whether or not to stay with science. Even cricket and tennis must be fun to play, in order to attract the young. The most hardened performers hardly give their best once the enjoyment is gone. So it is with science. We are unfortunately living in times when the pressures of delivering results 'useful' are beginning to overtake even the 'most academic' of our scientists. While we have generally failed to develop an efficient professional cadre of scientists in many important areas, we are in great danger of also eliminating the mildly irrational dreamers. Even cricket would be a hard game to play if we had neither 'Gentlemen' nor 'Players'.

P. Balaram



After a series of failures, Professor decided to rely more on his sixth sense to manufacture that wonder drug.

CURRENT SCIENCE

Volume 77 Number 5 10 September 1999

EDITORIAL

A century of science

In a provocative 'millennium essay', Vaclav Smil (*Nature*, 1999, **400**, 415) picked out the Haber–Bosch process for producing ammonia as the most important scientific advance of the century. Indeed, as the end of the year approaches, the number of essays looking back on the century and indeed the millennium will increase. Why was the ammonia synthesis so honoured? The author's argument is compelling. After all we can indeed live without computers, lasers, spacecraft and will probably live better without atomic bombs. We might have even muddled through the century if the structure of the atom and that of DNA were not understood. But would we have reached our present level of subsistence if food production has not kept pace with the demands of a burgeoning population? Indeed, the Malthusian disasters anticipated in the last century have been avoided. To a large measure, the credit for the enhanced yields of twentieth century agriculture must be ascribed to the use of fertilizers; ammonia of course is the key chemical in the fertilizer industry. The celebrated success of the green revolution has overshadowed the unobtrusive role of ammonia. While biological nitrogen fixation is accomplished by a few organisms in a subtle and sophisticated fashion, man only has the Haber–Bosch process available to convert the abundantly available and splendidly inert molecule of dinitrogen to ammonia, opening the route to urea synthesis. For most of us, the Haber–Bosch process, the conversion of nitrogen to ammonia at high temperature and pressure in the presence of catalysts, is a nightmare of forgotten chemistry. To most students, the Law of Mass Action and Le Chateliers Principle, which accompany the ammonia synthesis in textbooks, are associated with the most unattractive aspects of chemistry. To be told at the end of the century that the ammonia synthesis has transformed our existence may indeed come as a surprise. A moment's reflection will however convince most readers that Vaclav Smil has a point. For us in India, the fact that our population has now reached a billion people and that we will reach a billion and a half by the middle of the next century should be alarming. As the number of mouths to feed increases inexorably we may as well hope that the early years of the coming century will yield scientific advances that will match the impact of the ammonia synthesis on agricultural production.

In making the case for ammonia the author has no doubt exaggerated his case. Science, unlike armies, does not march on its stomach alone. Is it easy to list the dozen most important scientific events of the century? Undoubtedly, not. Even sportswriters would have difficulty in naming the hypothetical 'all time great' cricket team of the century. Such lists are often more distinguished by their omissions, rather than their compositions. Nevertheless, looking back on a century of scientific achievement may be instructive and even entertaining. In some fields the cast of characters is too well known to enumerate. Who after all does not know the names of the immortals who transformed 20th century physics. The consequences of the development of modern physics are common knowledge, but I suspect that in assessing the impact of each individual's contribution, opinions will differ and sharply so. While nuclear fission may be the event that has most dramatically transformed the public perception of science in this century, few general readers may recall the names of Otto Hahn and Lise Meitner; the latter, of course, was even forgotten or deliberately ignored by the Nobel committee. The history of science is difficult to recount and we would do well to remember the old dictum that 'time blots out small merit and fattens big glory'. In fields which are far from the public eye the names associated with fundamental advances are often obscure. Even as biology slowly moves to centre stage, it is clear that in the public perception no single event will be as indelibly etched in our memories of this century, as the discovery of the structure of DNA by James Watson and Francis Crick. What will undoubtedly be forgotten will be the enormous amount of painstaking, careful and quite often, remarkably original work that has brought modern biology to its present state. Like the Taj Mahal, the Pyramids and other monuments of yore, the edifice of modern science has been built by hundreds and thousands of dedicated individuals. Looking back on the contributions of 20th century science may indeed be a good way of recognizing our debt of gratitude.

P. Balaram

CURRENT SCIENCE

Volume 77 Number 6 25 September 1999

EDITORIAL

Ministering Science and Technology

If elections are here, can ministry formation be far behind? Periodically, we are treated to the spectacle of politicians, flushed with success, jostling for portfolios, some of which are more prized than others. Few will contest the fact that today Science and Technology is a quiet, backwaters ministry, which might attract public attention only when someone claims by an alchemical trick to have transformed grass into gold and as once happened, converted herbs into fuel. Many important components of the Indian science system like the various arms of strategic and defence research, the medical and agricultural research establishments and the department of environment lie outside the Ministry of Science and Technology. Academic science also derives its basic support through the Ministry of Human Resources Development and the University Grants Commission. Not surprisingly, no important, political or even social, agenda can be pursued from within the Ministry. Inevitably, the stewardship of science and technology is an 'additional charge' for an important functionary or is relegated to a minister who must be found a portfolio. The days where science and technology were sheltered under a Prime Ministerial umbrella are clearly over. In the last decade, there has been a steady slide in the perceived importance of the Ministry, with all the attendant difficulties of diminishing resources and declining manpower inputs of high quality. If indeed the Ministry is important for the conduct of scientific research, then scientists must view its slow regression with concern. The marginalization of the Ministry is evident in the battle over animal experimentation. All the shots have been called from the Ministries of Welfare and Environment, encouraging activists to literally invade a national laboratory (National Institute of Nutrition, Hyderabad), inevitably requiring a judicial restraining order.

Are ministers important for the functioning of ministries? Presumably so, but if the popular British TV serial 'Yes Minister' is any guide, ministers are usually putty in the hands of a trained bureaucracy, whose primary purpose is to perpetuate their supremacy. Ministers do have their uses. Many schemes of doubtful value can be railroaded past an unwilling scientific establishment by invoking a 'ministerial diktat'. In Delhi, the secretaries in the science ministries are formally 'technocrats', but are almost invariably shackled by generalist administrators holding the purse strings and deciding the rules of the financial game. The importance of the politician 'minister' is really in acting as a broadminded referee in this pointless internecine battle for turf. The principal sufferers in this scenario are the thousands of scientists and hundreds of institutions that constitute the backbone of the scientific enterprise in this country.

Surely then, there is a case for having ministers who have a modicum of commitment to their portfolios and who have a vision for the ministries under their charge. We cannot forget the role played by C. Subramaniam at the Agriculture Ministry, where he shepherded the 'Green Revolution' and his stewardship at the Ministry of Science and Technology in the early 1970s, where he created the National Council of Science and Technology, the forerunner of today's science funding agencies. Politicians, if they choose to do so, can indeed influence in a positive manner many of our important and essential institutions. Unfortunately, there are far too many examples to the contrary, to engender any hope. A recent editorial in *Nature* noted the passing of US Congressman George Brown, who indeed championed the cause of science funding in a Congress determined to trim budgets. If the scientific enterprise in the West is strong, it is largely because of the broadbased political support that is given to the cause of science and technology. Research is no longer cheap. High investments of public money require a substantial political will and a confidence that the 'returns on investment' will be commensurate. Suboptimal investments and the absence of strict evaluating mechanisms will only hasten the decline of many institutions into an abyss, from which recovery in the future will be almost impossible. Science in India is practiced in institutions that vary widely in quality, re-

sources and administrative structure. The 'showcase' laboratories are only the tip of a hidden iceberg of indifferent quality.

Scientific and technological progress, the world over, will continue with increasing pace in the new century. The gap between cutting edge, international science and the efforts made in India will inevitably grow. The high profile successes of 'strategic science' and the growing tempo of the drum beating on information technology, have in the recent past, diverted attention from the many important areas of basic sciences, biomedicine and agriculture, which need to be carefully fostered in the coming years. The major battles in biomedical research in the future, will undoubtedly require that the troops are well trained in basic biology, chemistry, physics and even computer science. Those charged with the responsibility of 'ministering' science and technology may need to devote their full and undivided attention to the difficult task of galvanising a lethargic, far flung and disspirited scientific establishment to face the future with enthusiasm.

P. Balaram

Current Science

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CURRENT SCIENCE

Volume 77 Number 7 10 October 1999

EDITORIAL

Women in science

The number of women entering research laboratories in search of Ph D degrees has been steadily growing over the years. Venerable institutions like the Indian Institute of Science have substantially increased on campus residences for women, in order to accommodate the larger numbers who are admitted each year. While biology used to be an area where the number of women students was the largest, the disparity, between the sexes, in other disciplines has been slowly breaking down. There are still some areas that are surprisingly thinly populated by women. Commenting on the growing crisis in the area of organic synthesis (total synthesis), highlighted in a report in *Science* (1999, July 9, p. 184), Carl Djerassi, a prominent organic chemist and one of the developers of the oral contraceptive had this to say: '. . . Even worse I know of no serious total synthesis team that has more than a couple of female members. Any practitioner of this testosterone-drenched sub-branch of chemistry should consider what practices deter talented women from even entering such academic laboratories, let alone leading them.' (*Science*, 1999, August 6, p. 835)

Specific areas apart, we may soon see in India, a complete reversal with more women than men being selected through the elaborate processes of admission to our major research centres. This influx of women is undoubtedly welcome and may even bring new and potentially beneficial attitudes to the practice of science in India. The representation of women on the Faculty of major institutes and universities and in the Academies of science is however, still poor. There are conscious attempts to redress this imbalance; approaches that are politically correct but fraught with the dangers of introducing a patronizing 'quota system'. This underrepresentation is indeed a worldwide phenomenon, which is being widely discussed (cf *Nature*, 9 September 1999, p. 99). Politics, where a few highly visible women occupy central positions, is largely male-dominated; so it is with science.

The difficulties faced by women in pursuing research careers are often poorly appreciated or deliberately ignored. With the median age for entering a Ph D programme nearing 23, most aspirants are close to 30 when they eventually obtain doctoral degrees. Ph Ds, in good institutions are becoming increasingly long drawn out affairs for both men and women. Post-doctoral research experience, often up to at least three years, preferably in an overseas laboratory, is a minimum prerequisite for selection to the regular faculty at most prestigious institutions. The inevitable social and biological necessities of marriage and child bearing (and rearing) are responsibilities which coincide with the most critical formative period of a research career. It is hardly surprising, that a majority of prospective women scientists are severely handicapped, in a professional sense, when compared to their male colleagues. Research, when pursued seriously, can be a demanding profession, particularly in terms of time and mental commitment. The few women who are successful in the scientific rat race, usually have supportive families (often with husbands also in science) or are single. Juggling the demands of families and a profession that can be all consuming, is a difficult proposition.

The role models for women in science are many, but their lives illustrate several of the problems which are encountered even today. Marie Curie, one of the 20th century's most enduring icons of science, had Pierre Curie toiling by her side, until his death. Dorothy Hodgkin, probably the most decorated woman in England, was fortunate to have a supportive family and indeed a personality that allowed her to run her research group like an extended family. Barbara McClintock, whose impact on genetics has been profound, ploughed a lonely furrow. So too, did Gertrude Elion, who along with George Hitchings, developed some of the most widely-used pharmaceuticals of our times. The travails of women in science has not been limited to personal sacrifice alone; often the prejudice and arrogance of male-dominated establishments have been major hurdles. Rosalind Franklin, of DNA fame, and Lise Meitner the co-discoverer of nuclear fission are famous examples, where due recognition was denied, even as their male colleagues (and competitors) were highly feted and celebrated by the Nobel Committee. There have been errors of omission

even in the recent past, presumably the result of unconscious bias, even as pressures build for equal opportunity and equal recognition.

Some years ago, in our midst, prejudices used to extend to even selection committees interviewing candidates for entry level research positions; but the clearly superior performance of women students emerging from postgraduate courses has successfully removed this impediment in many places. Times are indeed, changing. Women are here to stay in science, in large numbers. In a country like ours, the entry of women into the research profession augurs well for the future. In our otherwise bleak landscape, this may be the stray silver lining. It is important to ensure that the various schemes that are being designed to facilitate a research career are tailored specifically to meet the demands created by a large number of women in the scientific workforce. Postdoctoral research associate programmes and schemes designed to provide funding for temporary research positions must have flexible age limits and extended duration, with provision for attractive fellowship enhancements each year. Rules must be interpreted favourably or if necessary modified, to cater to specific needs of women scientists. This will facilitate the reentry into research of women who have taken a break after completing Ph D degrees. Innovative career development schemes that provide stable research support are needed. Our institutions must also develop a more women-oriented façade with significantly better physical facilities. It is sometimes embarrassing that even adequate rest rooms are unavailable at our institutions and venues of symposia; a definite sign that thoughtfulness is not one of our most abundant characteristics. The absence of child care centres (crèches) inside our campuses and at the sites of our innumerable major meetings limit participation of women to a great extent. In many places in the West, these facilities are almost mandatory. Many mundane matters need to be addressed to make life more pleasant for women in science. If these are attended to, life for all scientists, irrespective of sex, may indeed be better.

P. Balaram

CURRENT SCIENCE

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CURRENT SCIENCE

Volume 77 Number 8 25 October 1999

EDITORIAL

Presenting science

Sitting in innumerable lectures and seminars in darkened rooms, staring at indistinct slides while a soporific speaker drones on, is one of the occupational hazards of a scientific career. In crowded and poorly ventilated seminar halls, the effects of oxygen depletion and carbon dioxide enrichment are quickly visible as the less resilient members of the audience slowly drop off to sleep. Physiological insults apart, speakers too contribute to the generally sedative effects of most seminars. Not surprisingly, it is customary to serve coffee before seminars, presumably in the hope that caffeine will succeed where speakers generally fail. I was recently in the audience when a large group of recent Ph Ds from across the country, made brief presentations of their research work. With very few exceptions, the ability to articulate and the even more important quality of thinking clearly and simply were notably absent. Senior scientists are sometimes no better. Attendance at any major symposium will clearly reveal that the quality of scientific presentations is generally woefully inadequate. In these days of declining verbal and written skills of students entering science, we might do well to wonder if greater attention needs to be paid to teaching the skills of effective scientific presentations. The management and marketing professionals very often, worry primarily about packaging and advertising; presentation is indeed almost always the key to commercial success. A technically superior product can fall by the wayside, while an adequate but inferior competitor is swept to success by 'imaginative marketing'. Unfortunately, in these times, even something as pristine as science needs to be effectively sold. To sell science, scientists must be good salesmen (the politically correct, 'salesperson' somehow does not sound correct). Inarticulate, poorly organized, sometimes ill-informed and inadequately prepared presenters can hardly do the job.

This brings us back to the original theme – what indeed constitutes a minimum requirement for an effective scientific presentation? Undoubtedly, the speaker must know what he or she is talking about, an obvious requirement but one that does not always seem to be fulfilled. With slides and transparencies now being the norm, the least that one can demand is that these are legible. Gone are the days, when speakers wrote neatly and carefully on blackboards (later 'greenboards') allowing audiences the time to follow the drift of an argument. The painstaking drawing of chemical structures or the slow evolution of complex equations was sometimes a pleasure to behold. Instead, we now have lectures packed with slides or overheads, some containing so much material ('busy' is a curiously American description), that an audience is soon benumbed into stupefaction. At the other extreme are the 'corporate scientists' with PowerPoint presentations, bewildering for their varied colour backgrounds, but generally devoid of data and confined to well-known generalities. Most often the presentations made at symposia, by scientists from companies are an insult to the intelligence of any member of the audience interested in science. Their defence is usually corporate paranoia about secrecy. Why then participate in symposia? The next money-making breakthrough might as well be achieved in studied silence. There are also some speakers who hide the material on their transparencies with a sheet of paper, driving the more curious members of the audience to distraction. At a recent symposium I heard an overseas visitor remark that Richard Feynman would under such circumstances, walk up and strip the offending mask away. We also have speakers who have as many as fifty slides for a twenty-minute presentation, spend as much as half the time gripping the podium and talking on and on without even beginning the display.

But these are superficial problems which can presumably be adjusted by appropriate coaching; practice under a coach's watchful eye is something that should not be restricted to sports alone. The more fundamental issue is that a large number of 'scientists' and 'prospective scientists' do not seem to be clear about their own goals, the implications of their results in the context of their fields of specialization and the inherent

limitations of their own approaches. In a country like India, with widely dispersed research laboratories of differing quality and infrastructure, it is necessary that investigators are at least aware of limitations. Unfortunately, a large cross-section seems to live in a world of fantasy, imagining a scientific relevance for work that should not have been undertaken in the first place. The blame lies primarily in the unfettered recognition accorded to universities to award Ph D degrees, with many research supervisors not fulfilling any of the real requirements that are necessary before they act as mentors. For this sorry state of affairs, the University Grants Commission, the Academic (?) bodies of the universities and indeed the scientific community as a whole must bear the responsibility.

In seminars featuring speakers from the West, there appears to be greater attention to detail, more discipline in sticking to allotted time limits and clearer take home messages. In most universities in America and many in Europe, training students for presentations is automatic, since there are many reviews, group meetings and departmental requirements that necessitate oral presentations. These practices appear to be restricted to relatively few institutions in India. Most universities and their faculties appear to have abdicated all collective responsibility with regard to the training of Ph D students. The enormous variation in quality and performance of Ph Ds produced in our innumerable institutions is a clear reflection of the deficiencies of our academic system.

But raw Ph Ds are not the only offenders who perform in front of despairing audiences. What can one say when senior scientists present jumbled slides in random orientations and cut into the time of speakers to follow in meetings and symposia? Fortunately on most occasions few non-scientists are present. Otherwise they may really wonder whether scientists whose presentations are confused and badly organized are completely in control of their science. Anticipating criticism, I must say that there have been great scientists who have been inarticulate in public; Neils Bohr reportedly among them. But then being inarticulate is not a necessary and certainly not a sufficient condition for doing good science.

P. Balaram

Current Science

SUBMISSION IN ELECTRONIC FORM

Authors who have been informed of acceptance of their manuscripts may send the final version in electronic form on floppy diskette (3.5″ preferred; IBM PC format only, *not* Macintosh). The text of the manuscript only should be supplied as a plain ASCII file with no formatting other than line and paragraph breaks. (Wordstar 5.5 or 7.0 and Microsoft Word for Windows 6.0 are acceptable, but ASCII is preferred.) A hard copy of the text, with all typesetting information (italics, bold, mathematical type, superscripts, subscripts, etc.) must accompany the electronic copy. Tables and figures must be supplied only as hard copy. The diskette must be labelled clearly with the following: manuscript number, file name, file information (ASCII or Wordstar, version number, etc.)

Text may also be transmitted as ASCII only by e-mail to currsci@ias.ernet.in.

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CURRENT SCIENCE

Volume 77 Number 9 10 November 1999

EDITORIAL

Editors

The last year has seen the removal of two editors of leading medical journals in the United States. First, George Lundberg of the *Journal of the American Medical Association* (*JAMA*) was sacked for publishing expeditiously an article on a study of sexual preferences, which may have had a bearing on the Clinton impeachment trial in the US Senate. Second, Jerome Kassirer of the *New England Journal of Medicine* (*NEJM*) lost his job in a battle over commercial exploitation of the journal's name and its pages. The major issue in both cases was – who owns and controls the journals and their contents. Floyd Bloom, Editor-in-Chief of *Science*, comments (*Science*, 1999, 285, 1207) that 'editors must be free to navigate the editorial path, but also be accountable for their journal's overall performance'. While mourning 'the distasteful and disruptive spectacle of editor-sacking' he points out that 'without editorial independence, there will be little content worthy of distillation into knowledge'. The principle that editors must be unfettered is generally accepted in the world of publishing. Editors have long tenures and often function with little of the democratic niceties that characterize the discharge of duties in other positions of responsibility. Indeed, most journals seem to function efficiently and well only under a benign editorial dictatorship. The pressures of deadlines and decision making, which invariably will invoke either the ire of authors or the wrath of referees (and sometimes readers), leave little room for collegial committees that move with a glacial pace, to an obvious conclusion. Since speed is of the essence in most editorial decision making, mistakes abound. Despite this, most science journals which cater to a wide, general audience provide a useful and invaluable source of information and opinions on rapidly changing fields of science and policy.

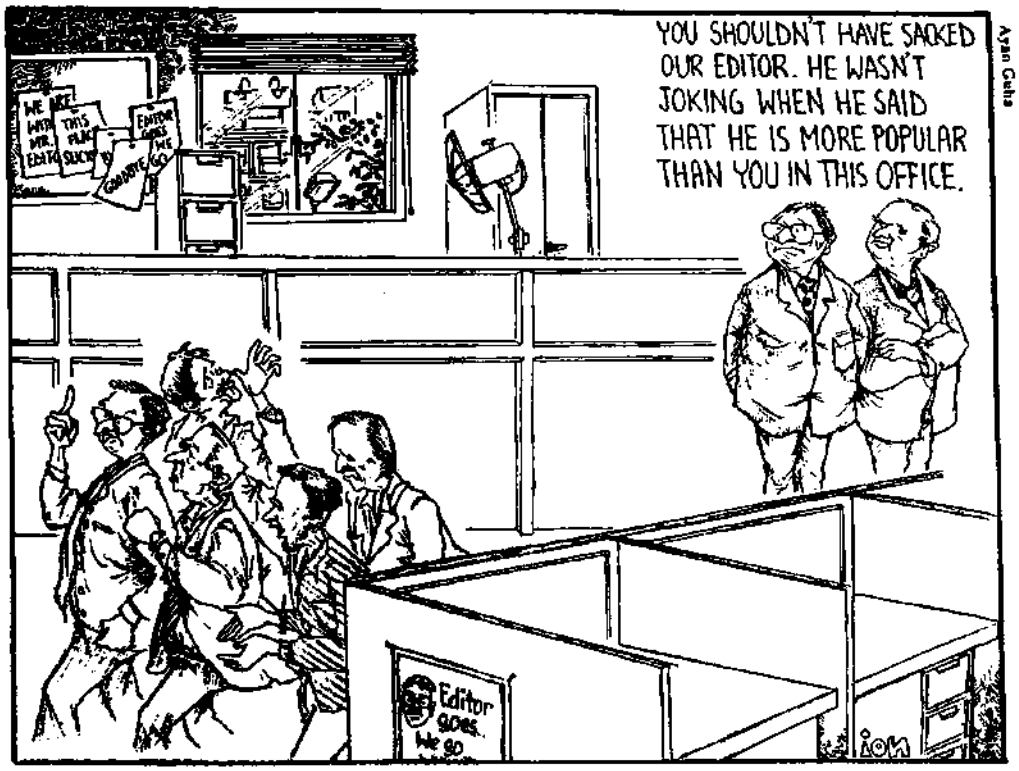
In the last few years *Current Science* has attempted to fill a niche in Indian science, by providing a forum for discussion of not only science but also policy and issues that concern scientists. This gradual change from a staid, technical journal to a more 'newsy' incarnation has not been without problems. We, in India, have generally been accustomed to discourse and discussion, where policies and programmes are rarely questioned in print. Indeed many vocal critics on any scientific issue under discussion are more likely to express their views in coffee houses or private conversation, but are always loath to put pen to paper. Many of our movers and shakers are indeed bold enough to state that they will never express an opinion on paper that is contrary to what is perceived as an accepted, establishment view. Indeed, in such a climate criticism, complaints and contrary opinions are only voiced by the 'fringe', who are then brushed off as a needless annoyance. If criticism sometimes strikes too close to home, there are injured protests; with some going so far as to state that dissension and disagreement do damage to the fragile fabric of science in India.

The editor needs to tread an inevitably treacherous path, in deciding on what is eventually published and in expressing what are essentially personal views in an editorial column. Since editors tend to pontificate on subjects with which they may have only a nodding acquaintance, editorials act like a lighting rod to attract criticism from more informed readers. I was told by a well-meaning colleague recently that my editorials only touch upon issues that I have some familiarity with, to the exclusion of many pressing matters that affect science in our country. This is a just and reasonable charge and the deficiency should hopefully be rectified by guest editorials and opinion pieces which articulate reasoned views on critical matters. On page 1134 of this issue, Amulya K. N. Reddy makes a telling comment on this journal's reaction to the Pokhran explosions of May 1998: '... after an initial silence on the subject (as if it never happened) *Current Science* publicized the official/ government version of the "kilotonnes yield" of the test bombs but rejected/ suppressed estimates of the hundreds of thousands of innocent non-combatants who would be killed if even a primitive bomb were exploded on Mumbai/Karachi'. Pleading guilty is sometimes the best defence. The suddenness of Pokhran and the editor's inability to quickly take a position led to the initial silence. But is the editorial decision to publish the Pokhran estimates correct? I believe this was the right thing to do, because the subject was topical and only the BARC group had the best available data, although all of it

was not presumably disclosed in the paper. Should journals publish reports which withhold information either on national security grounds or because of the potential for commercial exploitation? This is an increasingly difficult question that editors must address. Was *Current Science*'s rejection of the paper dealing with the effects of nuclear explosions on thickly populated cities correct? I believe so, because such estimates have been made before and widely discussed; more importantly, as pointed out by a referee, even in India several decades ago, the problem was addressed by D. S. Kothari. Editors, of course, have an advantage. They do not normally have to defend their decisions on manuscripts, except in the rare cases where both published and rejected papers attract public attention. This point has been highlighted by the very recent publication in *The Lancet* of a paper on the effect of genetically modified potatoes on the growth of rats by Arpad Pusztai, whose apparently flawed experimental design has attracted the ire of the Royal Society. In the context of the raging debate on the safety of genetically modified foods the paper in *The Lancet* appears to have been shepherded through the review process because of a strong editorial position on the subject. Clearly, editorial prejudices can cut both ways and there is a strong temptation to play to the gallery. Editors, necessarily have to steer a course between the Scylla of public passions and the Charybdis of unemotional totalling of referees' reports. An editor's primary concern must eventually be the standing of the journal that he or she produces.

Finally, why do editors write editorials? The purpose, I believe, is to provoke thought and discussion in the hope that incessant articulation of well-known problems will eventually result in reasonably successful solutions. It is really in the feedback that is received from readers that one is able to guage the utility of a fortnightly essay. A perceptive reader recently wrote, 'Your editorials are most of the time concerned with important issues, but I have noticed that your language and presentation is changing towards somewhat non-serious journalism'. He went on to add that 'sombre articulation may be better'. While this may be construed as sharp criticism, I suspect that it might in fact, be a well disguised, if unintentional, compliment. Good humour, even in editors, may be a virtue. It is important not to take oneself too seriously.

P. Balaram



CURRENT SCIENCE

Volume 77 Number 10 25 November 1999

EDITORIAL

Footsoldiers of science

At the end of the century one cannot but wonder at the dramatic changes in the practice of science. Universities in Europe were the major centres of science in the early years of this century. The structure of research groups, even in the experimental sciences was simple. There was a 'Professor', possibly with an assistant or two and a smattering of students, who sought entry to the profession by way of an apprenticeship. Large groups were uncommon, even in well-developed, manpower intensive areas like organic chemistry. Doctoral degrees were awarded for work of substantial significance. In any field, the major (and even supporting) actors were easy to identify. Almost everyone in a specialized field knew their colleagues, despite constraints of geography. The transition from Eurocentred science began in the 1920s, close on the heels of the Great War. The Americans began to make their presence felt in the years between the two World Wars, but it was the postwar years that really saw the beginning of the remarkable growth of science, particularly in the United States. Vannevar Bush's report which envisioned the 'endless horizon' of science was the crucial catalyst for governmental support. The rapid rise of academic research in American Universities has transformed the way science is practiced at the end of the century.

Today, most successful research groups in the West have a three-tier level of organization. There is a tenured professor or untenured Assistant Professor at the head of a team, which consists of postdoctoral fellows, who already have Ph D degrees and graduate students who work towards a degree. Research groups also have well paid, often highly skilled technicians; a species not generally seen in India. As science has increased in complexity so have the sizes of the research groups in most areas of experimental research. It is not uncommon to see laboratories manned by dozens of postdoctoral associates, often working as a team, with the group head acting as a manager of research; a pattern reminiscent of research laboratories in industry. Faculty members who run large groups must then spend considerable amounts of their time on raising resources to pay for their research and their postdoctoral fellows. In such an environment, new graduate students are often left to their own devices, working on peripheral problems, while the major research targets are attacked by the more experienced postdoctoral group. Students then find surrogate mentors amongst the senior members of these large groups.

Two features of this system are critical in determining the dynamics of academic research in America. First, is the remarkable expansion of opportunities for postdoctoral research in America. Second, is the brutally efficient 'tenure' system for faculty. The United States acts as a magnet for a steady stream of postdoctoral fellows from all over the world, with India being a significant contributor. The strength of the American system lies in its ability to assimilate and extract the best out of scientists trained in enormously diverse laboratories, spread across the globe. This may be contrasted with the amazingly insular policies of our country, which make it difficult to even entertain a foreign student or visitor, in the best of our institutions for a substantial period of time. Of course, one might ask, why would anyone want to come to an Indian laboratory in preference to a Western institution; but that is another issue.

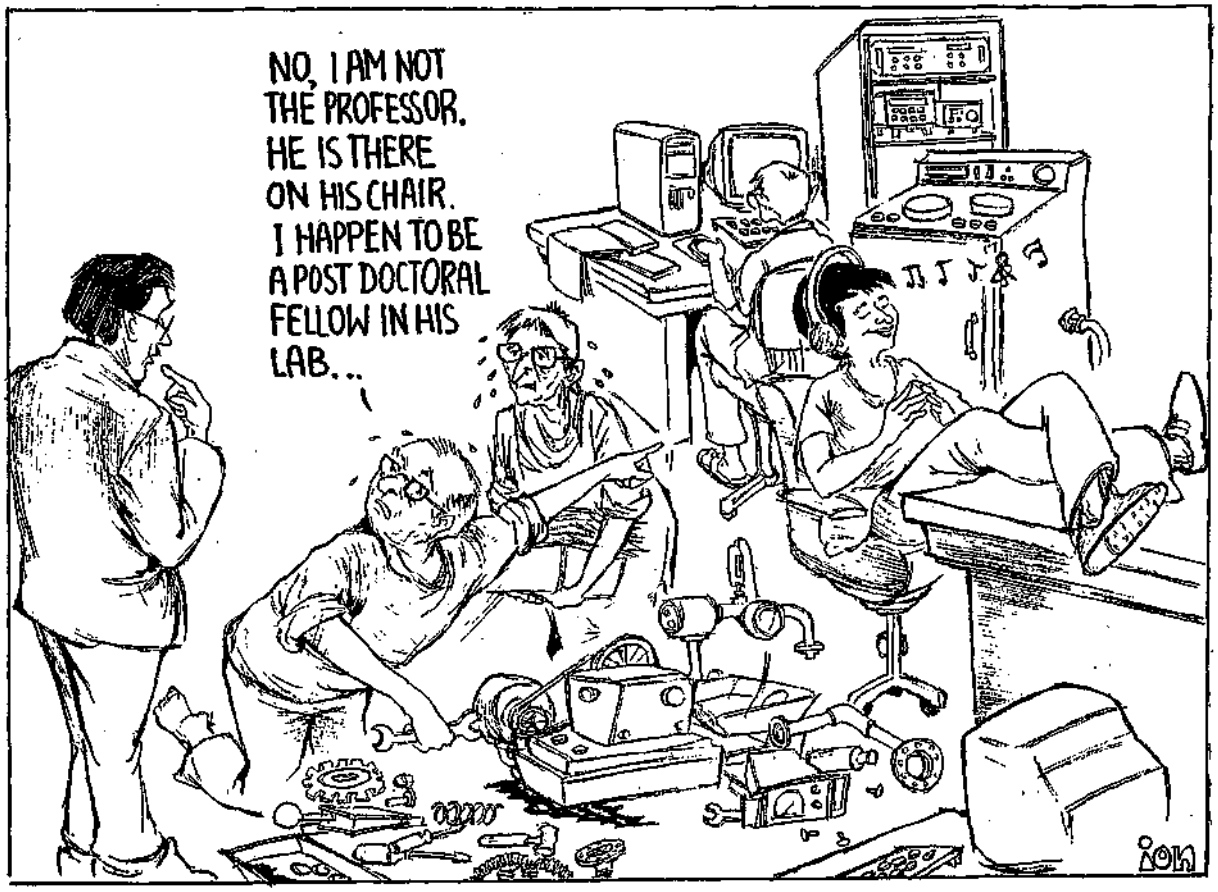
The postdoctoral fellows are an increasingly important factor in the sociology of science. As the size of research groups has grown, fuelled by scientific and personal ambitions, so too has the number of postdoctoral associates. Unfortunately, the potential for 'permanent' or regular employment has not kept pace with the growth of the number of Ph Ds. The result is that an increasing number of scientists are compelled to move from one temporary postdoctoral position to another. Consequently, there are an ever-growing number of researchers, in their mid to late 30s (an age which is almost mid-career in most professions), who have yet to settle down in regular positions in academia or industry.

The demand for postdoctoral researchers is driven by the requirements of major research efforts; in many areas of chemistry and biology, teams of as many as 20–30 'postdocs' toil away at a single problem. In a recent issue, *Science* (3 September 1999) focused on the growing problems of the burgeoning community of postdoctoral researchers.

The American academic system thrives on a Darwinian selection process, which ensures that competition for survival results in the best institutions, allowing only the most promising graduate students and postdoctoral associates to reach the level of 'tenure track' faculty. The tenure system affords a further filter and an opportunity to correct for errors of judgement. Permanence is thus achieved only after considerable scientific accomplishment. Success once tasted, becomes a heady elixir, guaranteeing no let up in research efforts, at least until age becomes an inhibitory factor. This robust and at times brutal system has provided the underpinning for the advance of science in the second half of this century.

In India, postdoctoral research is not a well established practice. Although the CSIR runs a regular programme for Research Associates, whose tenure appears to have been reduced from five to three years, there are relatively few postdocs in our laboratories. In our better institutions almost all Ph Ds produced head westward for postdoctoral positions; converting our graduate programmes into mere training centres which work towards staffing the most competitive overseas laboratories. The few postdocs in our laboratories come from the less developed institutions. They use the opportunity as an intermediate step on the westward journey. Temporary postdoctoral positions in our laboratories are unattractive in every respect; low salaries, difficulties of housing, poor facilities and few prospects for permanent employment. Together with Ph D students, postdoctoral fellows are the backbone of the scientific enterprise. They are, indeed, the true footsoldiers of science. It is time that scientific institutions, government departments and industry recognized that postdoctoral scientists are an important segment of the community that need to be supported and nurtured. Any initiative to enhance the scope of support for postdoctoral research must of course be tempered by the need to work towards increasing employment opportunities for trained scientists. In an environment dominated by deteriorating institutional finances, constitutional and other provisions for reservations, regional chauvinism and local political factors, this is not going to be an easy task. In tending to science much attention is paid to the generals; it is time the footsoldiers had their day in the sun.

P. Balaram



CURRENT SCIENCE

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EDITORIAL

Conflicts of interest

Economic liberalization has brought in its wake a host of hitherto unknown problems, in the interaction of our research institutions with industry. Consultancy work undertaken by academic faculty at Universities and research institutions was generally minimal; Indian industry clearly managing to survive in a government-controlled economy with little need for research and development. The most active 'academic consultants' were usually in the engineering disciplines; civil, mechanical, chemical and electrical engineering being amongst the most favoured. Industry–academia interactions were then largely restricted to institutes of technology and a few national laboratories. But times have changed. Several years of an 'open economy' and increasing globalization of many activities, have suddenly seen a dramatic spurt in the interest that both Indian and multinational companies have shown in our research institutions. At several national laboratories and institutes there is now a steady stream of visitors from industry, scouting around for possible collaborative arrangements. Opening out to industry has meant that our institutions have to cope with many unfamiliar situations, involving issues of consultancy arrangements, technology transfer fees, institutional overheads on research grants from industry, extent of involvement of staff, on projects unconnected with the primary mandate of the institution. While these problems are not of even passing concern to the vast majority of our Universities, which do not support major research programs, they have become important in the 'creamy layer' of academia where scientific research in areas of contemporary interest is reasonably well developed.

At the national laboratories, particularly the chain of institutions managed by the Council of Scientific and Industrial Research (CSIR), the external cash flow (ECF) is a parameter used to judge a laboratory's performance. The ECF represents the laboratory's earnings from sponsored projects; in a strict sense limited to the inflow of money from industry. Laboratories, whose primary mission is in areas far from the interests of local (or even international) industry, tend to show even contributions from government funding agencies as inputs to their ECF. In most cases, such support is obtained for basic research programs, formulated by individual scientists, which have very limited potential for application, even in the long run. At the more successful laboratories, major projects are sometimes funded by multinational companies. The result is that although the infrastructure has been built by the government, which also picks up the entire salary bill, the research activities are dominated by the requirements of a few major industrial sponsors; ironically, most often these are multinationals, which view our laboratories as convenient sites for contract research. With government funding on the decline, the national laboratories have little options, but to generate resources by such arrangements.

At the academic institutions like the Indian Institute of Technology (IIT) and the Indian Institute of Science (IISc) the situation is different. Here, interactions with industry have traditionally been through individual arrangements with faculty. But the situation is rapidly changing as major multinational corporations have begun to make efforts to enter into wide ranging 'collaborations'. New models have come into existence, where even research laboratories of companies have entered campuses, in a manner reminiscent of arrangements in the West. These happenings have brought in their wake many problems that need to be addressed. Very often the memoranda of understanding (MOU) signed between the academic and industrial partners are shrouded in secrecy. The lack of transparency fuels speculation on the nature of the arrangements; the general impression is that academic institutions negotiate from a position of weakness, resulting in agreements that are loaded in favour of the industrial partner.

The government has sought to promote the interactions of local industry with our academic institutions by introducing several schemes like the creation of a Technology Development Board, instituting a special program in the area of Drugs and Pharmaceuticals and in initiating the Technology Development Mission, that operates at the IITs and IISc through the Ministry of Human Resources Development and the Planning Commission. In most of these schemes, the promise of limited industry support

can catalyse the input of a much larger amount of government money into specific research projects. Unfortunately, several of these initiatives have degenerated into a convoluted mechanism for attracting research funds, to support projects with little chance of technological success. Industry is generally content to limit its involvement to a minimal level, sometimes even showing internal investment on R&D as a contribution to these joint projects.

But these are early days in a new era of industry – academia cooperation. Presumably, many highly successful collaborations will emerge in the future. However, with success will spring a new range of problems, some of which can already be scented. As faculty move towards greater contacts with industry, there will be an increasing polarization within our institutions. Conflicts of interest of varying kinds will inevitably arise. Should faculty members, who command formidable consultancy fees and who bring in substantial 'overheads' money to institutions, be allowed to compromise on their academic responsibilities? Will a time come when research results cannot be openly discussed, even within institutional precincts because of the demands of commerce? These are only illustrative questions; there are undoubtedly many more, some of which can pose uncomfortable ethical problems. But the scenario of the future, if the Western model is followed (and it appears inevitable that this will happen) will also see the evolution of a new type of academic, the professorial entrepreneur, who combines an academic vocation with the job of running a 'start up' company, based on the fruits of research carried out in the laboratory. The two areas of research where this kind of activity is likely to happen are 'information technology' and biotechnology. In the late 1970s the growth of the, then fledgeling, biotechnology industry in the United States was largely catalysed from the campuses of some of the best known Universities. In the early days, even in a comfortably capitalist milieu, academics were uncomfortable to be seen making large sums of money. Times have changed and very often an American professor's success is related to the number of stock options that he or she possesses. In the area of 'information technology' there is already a danger that faculty may find it more lucrative to teach in 'commercial courses', rather than to interact with students at their own institutions. We may, in fact, in the not too distant future have to worry about the strains that commercial success will place on the already fragile structure of our academic institutions. It is clear, however, that academic science administrators are going to have to wrestle with many issues relating to different kinds of conflicts of interest.

But they should take heart from the attitude of a successful chancellor of an American University who is reported to have said: '. . . at a good research institution it is the job of faculty to create conflicts of interest and the job of administrators to manage them, so as to avoid undesirable outcomes.' (Murray, R. W., *Analytical Chemistry*, 1999, **71**, 713A).

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Biology at millennium's end

Biology is a vast continent, with a great deal of virgin territory that remains to be explored. Biology's borders with the neighbouring areas of chemistry, physics, medicine, technology and even mathematics, are extended and diffuse. Specific sub-disciplines of biology comfortably straddle the 'no-man's' land between the 'hard' and 'soft' sciences. Indeed, biology provides a somewhat tenuous link between the traditional sciences and the 'social sciences'. Extrapolation from studies of animal populations and their behaviour to the human condition can often be seductive. The promise of 'applicable' biological research, to enhance the quality of life, has been dramatically realized over the course of this century. The explosive growth of molecular biology and biotechnology and their myriad applications in agriculture and medicine have propelled biology to the forefront of the sciences. It is now a cliche to say that we live in an age of biology. Unlike its sister sciences, biology is an all-encompassing field, a forgiving religion, which welcomes all comers. It is almost impossible to enter physics, chemistry, mathematics or engineering after the first flush of youth has passed. Formal degrees and a well-defined pedigree are a necessary passport to work in these fields. In contrast, defections to biology are easy to effect (and often profitable); remarkably, in many areas formal training in other disciplines is a great advantage. Mathematical and analytical skills, familiarity with chemical language and appreciation for quantitative thinking are best acquired when young. Biology's nuances can be appreciated at a more mature age.

Many of modern biology's major triumphs have been heralded by the 'converts'. Max Delbruck deserted physics at the height of the quantum revolution to found the area of bacteriophage genetics, an historic move in the context of the development of molecular biology. Linus Pauling, the quintessential chemist, displayed remarkable prescience, when he recognized the molecular basis of disease. The story of sickle cell haemoglobin is now commonplace; the connection between point mutations in genes and pathology has been repeatedly established. The molecular analysis of genes has been profoundly helped by physicists in search of greener pastures; most notably Francis Crick, whose discovery of the double helical structure of DNA, with James Watson, is now assured immortality and Walter Gilbert, a convert from particle physics, who ironically pioneered the chemical method for sequencing DNA. In India too, the major advances in biology have come from outside the traditional departments of botany and zoology. G. N. Ramachandran's path-breaking analysis of the structure of collagen and his profoundly important insights into polypeptide chain folding were born in Madras University's physics department, eventually legitimising a new subdiscipline of 'molecular biophysics'. Structural biology, now an area devoted to analysing the enormously complex molecular structures, that are a hallmark of cellular chemistry, is a creation of crystallographers and spectroscopists, who have traditionally been physicists or chemists. Biological research has indeed been enriched greatly by the immigrants; the New World drawing its strength from the pilgrims.

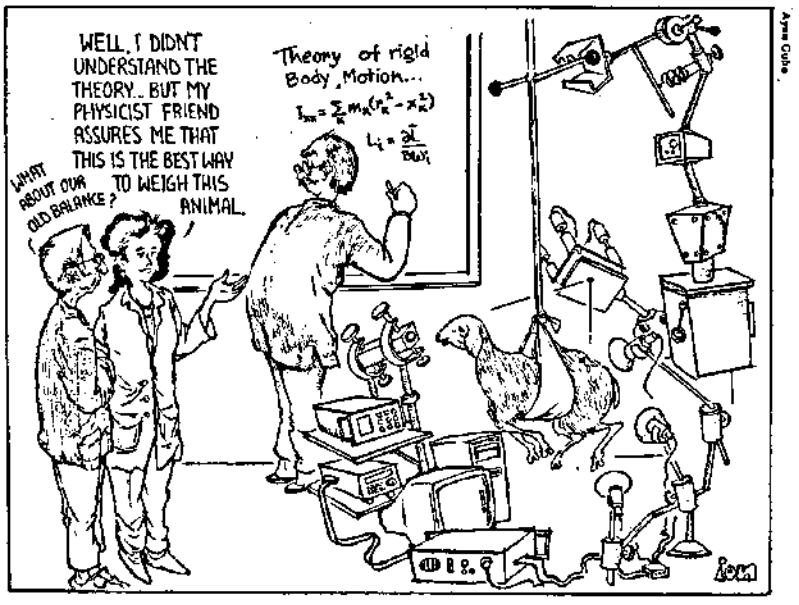
At the turn of the century, biology had its inheritance; Linnaeus, Mendel, Darwin and Pasteur ranked among the immortals. In the dying years of the 19th century, Eduard Buchner (somewhat surprisingly more famous for his funnel) demonstrated cell-free fermentation by the 'juices of yeast', establishing that life processes undoubtedly follow the laws of chemistry (and physics). Biochemistry was born and quickly divorced from traditional chemistry; a clear separation of the animate from the inanimate. But, in the caste system of the sciences, biology ranked low. Almost half a century was to pass before the ugly duckling began to turn into a swan. The transformation was complete by the 1970s as biological research moved from triumph to triumph on an extremely broad front. The results of research appeared to quickly relate to practical advances in diverse fields, medicine and agriculture among them; areas of direct societal impact. These developments have been largely fuelled by the ability to isolate, clone, amplify, sequence and manipulate DNA; a package of techniques that form the heart of recombinant DNA technology. Even as the 20th century neared its end, biology's first mega project began to take shape. The 1980s saw the beginning of the discussion on the scientific utility of sequencing whole genomes of organisms. Deciphering the order in which the letters (bases) are strung together in a genome, provides a comprehensive view of the genetic instructions encoded by nature; somewhat picturesquely called the 'Book of Life'. Few would have anticipated, even in the early 1990s, that these 'mega projects' in biology would be completed so quickly. In the last few years, several genomes of microorganisms have fallen to the onslaught of large-scale sequencing. These include, pathogens like *Helicobacter pylori*, the bacterium associated with gastric ulcers and most importantly, *Mycobacterium tuberculosis*, the causative organism in TB (S. T. Cole *et al.*, *Nature*, 1998, 393, 537). Knowledge of their genomes provides a high resolution map of the enemies' territory, presumably providing insights that may be used for eventual conquest. The results of major international efforts

in genome analysis are pouring in at present. The fall of chromosome 2, of *Plasmodium falciparum*, the most dangerous of the malarial parasites is a signal advance (M. J. Gardner *et al.*, *Science*, 1998, **282**, 1126). A *tour de force* in the sequencing arena is nearing completion, the genome of the fruitfly, *Drosophila melanogaster*, which constitutes a staggering length of ~180 million bases. In principle, the keys to the vaults containing the secrets of differentiation and development of a complex organism are now available. The human genome project appears to be moving so fast, that this may be one instance, where a major collaborative international project is completed ahead of schedule. A conservative completion date, 2005, projected at the start has now been advanced to as early as 2002. The first battle has been won, with the sequencing of chromosome 22 (I. Dunham *et al.*, *Nature*, 1999, **402**, 498) although the thought is sobering that in reaching this objective only 33.4 million bases have been sequenced, as compared to the approximately 3.5 billion, present in the human genome. The paper reporting the chromosome 22 sequence had 217 authors spread over nine institutions on three continents, a clear signal that biology is now a clear rival to particle physics in the area of 'big science'. On another front, the determination of the detailed structures of the molecular assemblies that constitute the cellular machinery, the advances in the last year have been sensational. Even as 1999 draws to a close, the structure of the 30 S and 50 S subunits of the ribosome have succumbed to X-ray diffraction (W. M. Clemons Jr. *et al.*, *Nature*, 1999, **400**, 833; N. Ban *et al.*, *Nature*, 1999, **400**, 841). There is now a near atomic resolution picture of these massive multimolecular complexes. Close on the heels of these successes, a 7.8 Å structure of the intact ribosome, the factory for cellular protein synthesis, which has a molecular size of ~2.5 million Da, has been reported, albeit at lower resolution (J. H. Cate *et al.*, *Science*, 1999, **285**, 2095). The transition in the space of a little less than a century from the crystal structure of common salt to the detailed architecture of assemblies of hundreds of thousands of atoms, signifies a pace of scientific advance that is truly breathtaking. While these achievements may be hard to appreciate by the casual observer, some of biology's successes are more widely visible. Animal cloning is no longer the realm of science fiction. Even a project to resurrect the woolly mammoths from the frozen wastes of Siberia does not seem completely far fetched (*Science*, 1999, **285**, 1007).

Biotechnology, the practical arm of biology, primarily fuelled by recombinant DNA methodology, is now a widely appreciated field. Vaccines, drugs, gene therapy, gene-based diagnostics and genetically modified crops are frequent subjects of discussion. Vaccines and drugs are now sought against diseases which pose major public health problems, most notably tuberculosis, malaria and AIDS. The task is not made easy by the complexities of immunology and the infinite capacity of pathogens for eluding and countering attack. A balanced and rational approach, to evaluating the potential of biotechnology to contribute to human progress, requires a wider understanding of biology and its inherent complexity. The raging debates on genetically engineered crops and the ongoing discussion on the efficacy of even long-used vaccines for TB and polio, are a pointer that public discussion of the fruits of biology is likely to intensify in the future. There is a great imperative to enhance the breadth, scope and quality of biology education. Future administrators, politicians and even lay people may benefit from some exposure to the nuances of a field that is likely to dominate the future.

At the millennium's end, there is a temptation to speculate on the years to come. A thousand years is however, a timescale beyond the comprehension of ordinary humans. A century, which seems closer to the biblical lifespan of three score and ten, seems more manageable. But science is the graveyard of futurologists. Biology's century began with the underpinning of Darwin's grand evolutionary synthesis. But the last one hundred years have been relentlessly dominated by triumphant reductionism. However, there are already signs that the pendulum has begun to swing the other way. The preservation of our biological heritage is already a major concern. Taxonomy and systematics appear to be staging a comeback. There can be few certainties in science; but you do not need a crystal ball to hazard the guess that at least the first few decades of the coming century will be dominated by biology.

P. Balaram



CURRENT SCIENCE

Volume 78 Number 1 10 January 2000

EDITORIAL

Visions and nightmares

The old 'millennium' is thankfully over and the new one is upon us. Mercifully, we can be spared further talk of Y2K, problems and all. The continuity of time is self-evident and despite fond hopes of science fiction writers (and the aging population, which of course, includes all of us) time's arrow speeds along in only one direction. The new year, century and millennium are then, but an artificial punctuation mark imposed by the grammar of the Gregorian calendar. But, like all punctuation marks it has made us pause; in some cases it has catalysed reflection and in most, almost inevitably, celebration. The impending 21st century was first publicly sighted by our late Prime Minister, Rajiv Gandhi, in the mid-1980s; surprisingly, there was little talk of the 'third millennium' then. In the intervening decade and a half we have been flooded with visions for the future, some modestly targetted to the early years of the new century, while others are more ambitious in venturing optimistic forecasts, which may eventually be forgotten. Unsurprisingly, most 'visions' invoke a major role for science and technology in shaping the future; after all we live in an age of science. Most of these public 'visions' are authored by key functionaries in the apparatus of government science; unfortunately, much of science in India, like many other activities, remains a completely, centrally controlled and funded operation. Any 'vision', which envisages a role for science, cannot be taken seriously if it is completely myopic about the state of science and higher education in science in the country.

Over the last few years, two areas of crucial importance to the cause of science have deteriorated dramatically. Firstly, University science departments, with very few exceptions, have been fighting a losing battle on several fronts; diminishing budgets, declining quality of enrolment, inability to hire new faculty, the gradual disappearance of active research groups with professorial retirements and most importantly, the poor appreciation of most University administrators for the special requirements of science departments. Secondly, our research institutions, specialized laboratories and the mechanisms for funding research have been under increasing strain. The diminished purchasing power of the rupee, the extraordinary salary burdens imposed by the last Pay Commission on vastly overstaffed institutions and most significantly, the rapid decline of administrative mechanisms within funding agencies are matters that need to be urgently recognized and addressed. Few senior administrators of science are willing to even concede that there is a problem; for them it is business as usual. While dramatic improvements are hard to achieve, the least that one can hope is that the tide of deterioration can be stemmed.

A key element in the conduct of basic science is the support of individual investigator generated projects, which go through the elaborate process of peer review. In addition, infrastructural support in the form of regional sophisticated instrumentation centres has been important in making accessible major equipment, which are essential for the conduct of modern research. The Department of Science and Technology (DST) and the Department of Biotechnology (DBT) are two of the key funding agencies, which have contributed greatly to raising the level of science in India over the last two decades (the extramural activities of other agencies like the CSIR, ICMR, ICAR, DAE and DRDO are more limited). However, in recent times both the DST and DBT are beset by internal difficulties. The average time for sanctioning a grant has increased; the time taken to receive the first instalment of money is alarming and the likelihood of receiving subsequent instalments on time, decreases continuously.

The ostensible reason for this situation is the diminishing resource base for science, coupled with the whimsical interpretation of financial rules that have been in place for over half a century. Strangely, the very same procedures were in effect over ten years ago, when the flow of grants was smoother. The funding agencies are manned by a staff trained in science, often armed with doctoral degrees. But, unlike their counterparts in the administrative sections of other ministries, they are shackled by their specialization, to work in the same department for inordinate lengths of time. The absence of mobility and fresh challenge can very quickly act as a dampener for even the most enthusiastic entrant. The

perceived superiority of the financial officers contributes to sagging morale. Many scientific officers at the agencies are inevitably caught in a crossfire between their internal financial sections on the one hand and the laboratory scientists on the other.

Another dangerous trend, that has surfaced and almost became institutionalized, is the tendency to support mega projects of apparent 'societal impact' and the creation of major new laboratories, with little regard for the quality of leadership and manpower that will eventually man these structures. While projects with an expenditure of several crores of rupees can sail through a 'green channel', the minor individual projects, which determine the fate of small and active groups are consigned to the slowest possible tracks of officialdom. It is easier today to consider the possibility of eliminating AIDS, malaria or tuberculosis with a magical (and sometimes mythical) vaccine as part of a 'national mission'; to evaluate the possibility of an Indian mission to the moon; or to promise safe, cheap and unlimited power through nuclear energy, than to address the problems of ordinary scientific research that is being done in hundreds of laboratories across the country. Presumably, the many minor advances that have resulted from these incremental researches are easily forgotten.

As part of the grand bouts of introspection that have accompanied the death of the 20th century, countries all over the developed world have reexamined their science and technology infrastructure and decided on new initiatives and set new priorities. In the West, change is always contemplated; the calendar is not the sole catalyst. The acceleration of the pace of scientific change worldwide, places developing countries at a decided disadvantage. The playing fields of the present and the future will be anything but level. Even a traditional establishment like the Chinese Academy of Sciences has embarked on an ambitious and wide ranging plan for revamping the infrastructure for science (*Science*, 1999, 286, 1671). Japan, Israel and Canada have in the recent past set in motion major reform processes. The Europeans have unobtrusively consolidated and revamped their science journals, in a clear move to face the increasingly difficult challenge of keeping pace with American journals. Across the developed world, academies and scientific agencies are acutely conscious of the need for purposeful change. We, on the other hand, have been content as a community to wait for initiatives to emanate from politicians, who sometimes have little interest in their portfolios. Science is neither glamorous nor is it a subject with a wide public appeal. The few initiatives that have emerged appear fatally flawed, like the Swarnajayanthi Fellowships (DST) scheme or the companion schemes envisaged by the DBT. Some of the special award schemes are hard to justify in an environment where money is in short supply for routine research grants. The argument that these schemes will promote 'excellence' appears hollower with each passing day. These initiatives appear to be based on the premise that personal awards attract greater attention, than a focus on the detailed mechanics of research funding. No one would cavil at schemes to promote recognition of scientific merit, if equal attention and enthusiasm were devoted by the agencies towards the streamlining of their financial and internal administration.

Visions for the future, even in the limited area of scientific research, must be based on the ground realities of the present. There is an urgent need to rebuild the institutions associated with the conduct of science. The dawn of a new century can provide an excuse for action if the visions for the future are not to turn into nightmares.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Life

There is nothing over which a free man ponders less than death; his wisdom is to mediate not on death but on life.
Spinoza

The preface to Erwin Schrödinger's book *What is Life? The Physical Aspect of the Living Cell* (Cambridge, 1944) contains the quotation from Spinoza, setting the stage for a famous analysis of biology, particularly heredity, by one of the founders of quantum mechanics. Schrödinger's book, based on a series of lectures at Trinity College, Dublin, delivered in the middle of the war years, 1943, was influential in persuading many physicists to look towards biology in the hope of discovering new laws of Nature. Schrödinger phrased his questions clearly: 'The large and important and very much discussed question is: How can the events *in space and time* which take place within the spatial boundary of a living organism be accounted for by physics and chemistry?'. He was emphatic in stating that 'the obvious inability of present-day physics and chemistry to account for such events is no reason at all for doubting that they can be accounted for by those sciences'.

To Schrödinger, the fidelity of the hereditary process displayed a remarkable degree of determinism, in contrast to the indeterminacy so favoured in quantum mechanics and the 'probability mechanisms' in physics. In his lectures, Schrödinger highlighted the 'Habsburger Lippe', a peculiar, inherited disfigurement of the lower lip of the members of Austria's Habsburg dynasty; a feature which he termed as 'a genuine Mendelian allele'. In analysing the transmission of genetically coded information to successive generations, Schrödinger noted: '... the gene has been kept at a temperature around 98°F during all the time. How are we to understand that it has remained unperturbed by the disordering tendency of the heat motion for centuries'. He went on to worry about the structure of chromosomes, influenced clearly by C. D. Darlington's book, *The Handling of Chromosomes* (Allen and Unwin, 1942). At a time when Oswald Avery's clear demonstration of DNA as the 'pneumococcal transforming principle' was still a few years into the future and a decade was to pass before the Watson–Crick structure of DNA, Schrödinger's musings focused attention on biology as science's new frontier. He was particularly struck by the irregularity and complexity of biological structures, leading to his famous characterization of the 'chromosome fibre' as an 'aperiodic crystal'. To him the inanimate, periodic crystals of physics and chemistry were profoundly different from the rich tapestry that he foresaw in the 'aperiodic crystals' of biology. Despite the uncertainties of the times, Schrödinger optimistically concluded: 'In the light of present knowledge the mechanism of heredity is closely related to, nay, founded on, the very basis of quantum theory'. He summarized the most important attributes of biology: 'The most striking features are – first, the curious distribution of the cogs (chromosomes) in a many celled organism ... and secondly, the fact that the single cog is not of coarse human make, but is the finest masterpiece ever achieved, along the lines of the Lord's quantum mechanics'.

Over half a century has passed since Schrödinger's analysis. In the intervening years, molecular genetics has witnessed more than one revolution. Biology, once a collection of disparate disciplines, and the molecular sciences, physics and chemistry, have integrated in a manner that would have been impossible to foresee in the 1940s. The genome analysis projects of the 1990s are rapidly providing a wealth of molecular detail, as diverse organisms succumb to the power of modern DNA sequencing techniques. The relationships between organisms and the interplay between mutation and natural selection in evolution are clearly becoming understandable in molecular terms; a language that clearly derives its grammar from chemistry and physics. We are, in fact, wonderfully positioned to ask Schrödinger's question once again: 'What is life'.

The year 1999 provided scientific journals with a unique opportunity to bring out special issues, devoted to retrospective analysis and sometimes, a little bit of uncharacteristic speculation about the future. In a marvellous 'Millennium issue', *the Philosophical Transactions of the Royal Society (Biological Sciences)*, a journal with hoary

traditions dating back to 1665, examines, in a collection of essays, the state of biology on a very broad front. Of particular interest to our present theme is a thoughtful essay entitled, 'Life: past, present and future', by Kenneth Nealson and Pamela Conrad, which traces 'the connection between life and planetary geochemistry, the interaction between these two forces and the pattern of evolution seen in both' (*Philos. Trans. R. Soc. London*, 1999, **B354**, 1923–1939). These authors highlight a view of the planet that is particularly 'prokaryote-centric', a tribute to the myriad, invisible, micro-organisms that constitute an overwhelming majority of living creatures on earth. The prokaryotes, single-celled organisms, lacking a membrane-limited cellular nucleus, are distinguished from their apparently more sophisticated 'eukaryotic counterparts by their toughness, tenacity and metabolic diversity'. Views of life on earth, which centre most often on *homo sapiens* and the few plants and animals that humans fear or cherish, ignore the teeming multitudes of adaptable organisms, that are capable of surviving almost any conceivable man-made or natural disaster. Some organisms, like the radiation insensitive bacterium, *Deinococcus radiodurans* R1, have already drawn the attention of the genome hunters (O. White *et al.*, *Science*, 1999, **286**, 1571–1577). Nealson and Conrad offer a definition of life and highlight its major attributes: (i) Life has some structure, with hardware for energy conversion. (ii) Life has unique chemistry. (iii) Life strives to replicate with fidelity. (iv) Life evolves; mistakes during replication permit inbuilt variability. (v) Life consumes energy from the environment. (vi) Life must develop some means for escaping from its own metabolic end products.

While the tendency of classifiers of life has been to view organisms in terms of structure, Nealson and Conrad prefer to think in terms of 'energy flow and metabolic capacity'. Their classification into functional groups is provocative, as they divide the living world into 'physicists, chemists and biologists'. The physicists are 'those organisms that use physical sources of energy such as light or heat (e.g. photosynthetic organisms); the chemists 'use chemical energy, either organic or inorganic', while the biologists are 'organisms that feed on other organisms, using behavioural adaptation to gain organic carbon, which they use as chemists'.

The growing body of 'extremophiles', prokaryotic micro-organisms, that have adapted to survive under conditions generally thought to be incompatible with life, may provide important clues to the 'evolutionary past' of our planet and may allow us to think of life elsewhere in the Universe. According to Nealson and Conrad: 'In essence prokaryotes spurn life as biologists while optimizing their skill as chemists'. It is this ability of rugged and adaptable micro-organisms to survive the harshest, most inhospitable and bizarre chemical environments, that makes them the best candidates for existence in extraterrestrial environments, which may be probed by man in the not-too-distant future. The Mars Surveyor programs slated for 2003 and 2008 may indeed be a beginning.

Interestingly, discussions of extraterrestrial life have now been consigned to the fringes of science and do not seem to attract much attention amongst the lay public. In the early and middle years of the 20th century, science fiction revolved around the possibility of life on other planets and the eventual colonization of outer space. It is hard to imagine now that Orson Welles' radio rendition of H. G. Wells' 'War of the Worlds' could have caused a public panic in the 1930s. Generations of Americans grew up on the popular TV serial *Star Trek*. Isaac Asimov and Arthur Clarke were read with enthusiasm and no one who watched the late Carl Sagan could have doubted that life would very soon be found away from the earth. But times have changed. The spirit appears to have gone from the searches for extraterrestrial life; the thrust for space exploration sinking into the quicksands of technical difficulties and diminishing financial inputs, even in America; clearly a sign that public opinion is turning inward. But to read Nealson and Conrad is to return to the halcyon days of the 1960s and 1970s, when the flush of success of the Apollo moon missions, raised hopes that the search for life on other planets, solar systems and even galaxies appeared to be worth dreaming about. Their conclusions are inspiring: 'The study of past and present life on our planet tells us that life has dramatically impacted the geochemical evolution of the planet, and suggests that if we really understood the intimate relationships of these two processes, then it might be possible to locate life by measuring these effects at scales presently not even conceived. This is the challenge, and this is our future'.

P. Balaram

CURRENT SCIENCE

Volume 78 Number 4 25 February 2000

EDITORIAL

Research facilities

Modern scientific research in many disciplines is critically dependent on the availability of sophisticated instruments and major research facilities. In disciplines like physics, chemistry and biology, the range of equipment necessary to conduct contemporary and competitive research is growing at an alarming rate. For example, high field NMR spectrometers (with proton frequencies as high as 800 MHz), X-ray diffractometers equipped with CCD detectors, tandem mass spectrometers, confocal and atomic force microscopes, cell sorters and automated DNA sequencers are becoming commonplace in biological laboratories across the world. Synchrotrons provide the most powerful X-ray sources for probing biological crystals. The unparallelled spatial and temporal resolution provided by pulsed lasers has allowed chemical and biological phenomena to be addressed in a manner that could not have been anticipated a few years ago. Early watchers of the genome sequencing programs could hardly have forseen the enormous technological advances that would bring genomics to its present, preeminent position, in such a short time.

This increasing sophistication in the tools of science has resulted in a widening of the gap in performance between the well-endowed laboratories of the developed world and the institutions located in the less developed countries. Even as we search for global competitiveness in various fields, many difficult issues concerning science need to be addressed in India. We need to ask if our laboratories can be modernized; not so much with regard to their superficial appearances, but with respect to the research facilities that they house. It is all too easy to build new buildings, quite often elegantly designed by well-known architects, when new institutions are conceived; it is however, far more difficult to raise resources for cutting-edge research facilities and even more difficult to maintain and use them to yield competitive research results. Modernization is, unfortunately, associated with hefty costs and in the present climate a fresh infusion of substantial funds for capital equipment does not seem a realistic possibility.

Any plea for a relook at the 'true infrastructure' for science in our major institutions is likely to be misunderstood by those who hold the purse strings. Do we not already have many wonderful laboratories, with imposing facades, which have been steadily funded for many years? Is it not true that the performance of many major facilities has been woefully inadequate in scientific terms? Did not C. V. Raman carry out his, now legendary, experiments on a shoestring budget? Will a critical cost–benefit analysis of half a century of government-supported science yield a 'positive' balance sheet? Are scientists, like Oliver Twist, always asking for more? These are legitimate questions, all of which can be addressed.

After the first flush of enthusiasm, most newly created laboratories do not have sufficient inputs for capital equipment, which will permit constant renewal. There have been major research facilities in India, distinguished, principally, by their lack of usage; but these black sheep cannot be used to tar everyone with the same brush. It would indeed be wonderful to be back in Raman's times; unfortunately even the theoretical physicists, who need little money, cannot recreate the golden days in Gottingen. Finally, the apparent benefits of much of scientific research are hard to quantify; there is undoubtedly an undeniable case for scientific institutions as training grounds for generating the people, who will absorb and implement the technologies of the future. While basic science has few friends in places that matter, even the rare well wishers of science are sometimes unable to comprehend the importance of quickly and decisively upgrading the facilities for research.

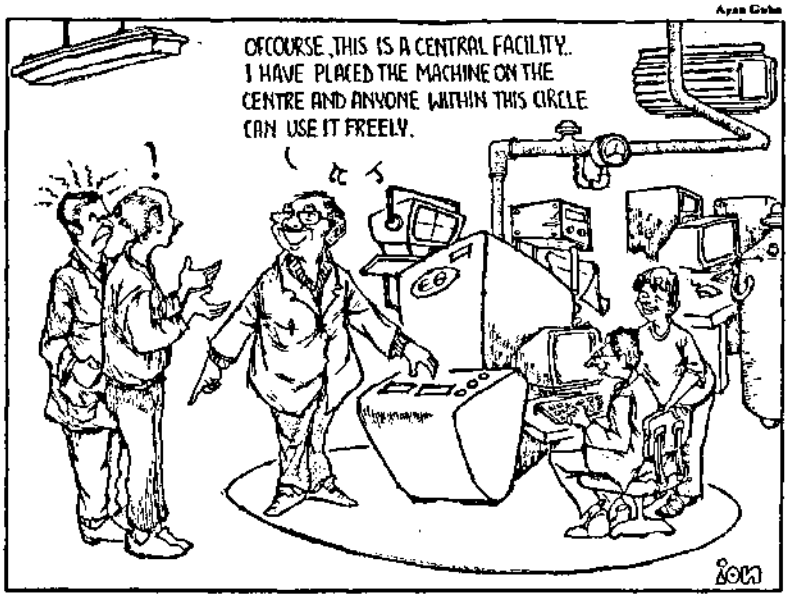
In the 1970s the Department of Science and Technology, in a youthful flush of enthusiasm, created the Regional Sophisticated Instrumentation Centres (RSICs) and related units housing expensive equipment, that would cater to a large number of researchers. Traditionally, these centres housed spectrometers of various kinds and provided analytical services primarily for chemists who were generally drawn from Universities and local industries. With time, some major research

facilities, like high field NMR ('midfield' by today's standards) became available at a few places, dispersed across the country. The major influx of research funds in the 1980s ensured that similar sophisticated instruments became available even in some individual laboratories. Over the years, the RSICs house equipment that can hardly be called sophisticated and there is a growing perception that they may have outlived their usefulness. What is forgotten in the critical analyses that are made is the invaluable service that these centres rendered, which indeed made it possible for a large number of researchers from poorly endowed institutions to access essential analytical facilities. The availability of centralized instruments, managed and maintained by dedicated staff contributed immeasurably to raising the general level of research in many areas of chemistry and biology. If the present level of funding to RSICs is continued, there will be a steady erosion in their capabilities to perform sophisticated experiments, inevitably leading to their complete decline. There is clearly a need to consider enhanced support for central facilities. The DST's FIST program is a most welcome new initiative.

There is another side to the story. For the dismal state of availability of major facilities, the scientific community must also share the blame. We have failed to create a culture of shared instrumentation, efficiently managed and optimally available to a large cross-section of users. Even occasionally-used equipment must often be unnecessarily duplicated in our laboratories, because of a lack of professionalism in interpersonal dealings. Departments and institutions instead of investing in a diverse portfolio of facilities, duplicate fairly expensive instruments. The almost complete absence, in our laboratories, of trained, competent and willing technicians makes the maintenance and efficient use of facilities difficult. Matters are not helped by the fact that almost all sophisticated instruments are imported and that spares and trained troubleshooters often have to fly in from overseas. Budgets for equipment maintenance are vanishingly small and funding agencies look askance at requests for annual maintenance grants. In this scenario, it is not surprising to find scientists jealously guarding equipment under their charge; every potential user is viewed with suspicion that should be reserved for saboteurs. Acquisition of major equipment is sometimes an end in itself, creating islands of scientific affluence surrounded by a sea of poverty. There is nothing more frustrating than to find that a major facility, located nearby, is completely inaccessible. Research students in India are also rarely allowed to handle major equipment themselves. As a consequence, many chemists and biologists (and physicists too) go through a Ph D program, with almost no feeling for the nuances of the measurements that they report in their theses and papers. Even more importantly, this situation legitimizes the 'black box' approach to science and trains yet another generation to be blissfully unaware of the importance of learning to nurse and maintain major research facilities.

The inexorable march of science has ensured that the tools for modern research will become increasingly complex and expensive. We need to prepare to wage a war on two fronts. Firstly, we must collectively ensure that pleas, for a major modernization plan of our scientific infrastructure for basic research, do not go unheard. Secondly, we must gear our institutions to develop mechanisms for acquisition, maintenance and optimal and equitable use of sophisticated research facilities. This will go a long way in raising the level of scientific research in India.

P. Balaram



CURRENT SCIENCE

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EDITORIAL

Information overload

We live in an age of information. There is so much information available that it is sometimes difficult to decide what to do with it. Information technology (sometimes even labelled as science), informatics, knowledge economies and knowledge-based industries are popular phrases, frequently found in speeches and articles that exhort us to face the challenges of the new millennium. While information and knowledge have always been central to most human activity, there is a heightened perception now that information is the key to success in diverse spheres, ranging from commerce to science. The information revolution is, of course, powered by the advances in computer and communication technologies, most dramatically exemplified by the prodigious growth of the Internet. The surfeit of information available, literally at one's fingertips (the click of the mouse may soon be the most easily recognizable sound), is particularly affecting the way science is practised. The days of a leisurely stroll to the library to glance through a week's supply of journals appears to be over; there are simply too many journals in every discipline and even the richest libraries cannot afford to subscribe to all of them. In the unlikely event that a library did stock up its shelves, it would be a foolhardy individual who could suppose that all the printed material, even in a relatively circumscribed area, could be digested by even the most determined reader.

The remarkable expansion of the Internet and long distance access to journals and websites, that provide digests, have transformed the way scientists search for useful information. Leading journals like *Science* now have dedicated 'web watch' pages, which highlight the most useful websites in specific areas. Libraries are increasingly going 'digital' and even 'virtual'. The PubMed initiative spearheaded by the National Institutes of Health in the United States and the alternatives being considered by a consortium of commercial publishers will provide unparalleled electronic access to journals. The issue of who will pay for journals which are freely accessible on the Internet is unclear, but there is little doubt that new paradigms in scientific publishing are emerging.

The generation gap in the area of information gathering is clearly visible in our institutions. The old timers prefer the printed word, which appears at a comfortably slow pace in our libraries; the new generation is more computer-savvy and quick to download the latest offerings from the Internet. Traditionally, libraries housed much of the information that researchers and others needed; librarians were the sole custodians of the books and journals in their charge. The scholarly librarian is an endangered species today, replaced by a new breed of 'information scientists'. This poorly defined term encompasses those who practise scientometrics, database managers and various shades of analysts, who track the course of science and technology. The decline of libraries and the disappearance of the classical librarian have coincided with the spectacular progress in storing and transmitting information electronically. Today, it is becoming common in the developed world, and even in some of our well-wired institutions to access databases and libraries across the globe in a matter of minutes (if not seconds). This ability provides researchers with access to information, which would have been difficult to imagine even a few years ago. Irrespective of the mode of acquiring information, the traditional library or the Internet, there seems to be an uncomfortable glut of knowledge.

There is, however, a growing problem as the amount of information on diverse subjects increases. It is becoming difficult to filter the useful from the useless; the accurate from the inaccurate and the reliable from the unreliable. The traditional whetting of research results by conventional peer review procedures may soon give way (as it already has in some fields) to electronic bulletin boards, where papers are displayed without any editorial interference. One unintended consequence of the growth of this unfettered mode of electronic publishing is that there may be a gradual decrease in the impact of the ubiquitous 'impact factor'. The consequences for the sociology of science will undoubtedly be interesting.

In India, the information industry has suddenly grown; there is even a Ministry for Information Technology.

There has been a sudden spurt in the number of institutions devoted to spreading the new gospel. Magnificently endowed, Indian Institutes of Information Technology (tantalizingly abbreviated as IIITs) are springing up, which will undoubtedly attract hordes of students, who will presumably be trained in a discipline, whose content and contours are still unclear. Even established institutes of science and technology have been quick to jump on to the information bandwagon: sometimes relabelling old programs to invest them with a new aura. To the casual observer, a degree in 'information technology' provides an entry to some of the most successful industries of recent times. The importance of information is undeniable. Few will argue against the virtues of an expanded knowledge base. However, the growing crescendo of drum beating in agencies concerned with the 'information sector', will lead many to conclude that 'information' is the key to many of our problems. Marie Antoinette upon hearing of the restive Parisian mobs in the run up to the French revolution is reported to have remarked—'if they cannot have bread, let them have cake'. Those spearheading the information technology upsurge would appear to be offering information.

P. Balaram

CURRENT SCIENCE

Volume 78 Number 6 25 March 2000

EDITORIAL

The ides of March

Caesar : *Who is it in the press that calls on me? I hear a tongue, shriller than all the music, Cry 'Caesar'. Speak; Caesar is turn'd to hear.*
Soothsayer : *Beware the ides of March*
Caesar : *What man is that?*
Brutus : *A soothsayer bids you beware the ides of March.*

Julius Caesar

March is the month which marks the almost abrupt transition from a relatively pleasant season to the harsher days of an Indian summer. A gentle and prolonged spring is a feature that is unfortunately absent in our midst. March, of course, is the month that boded ill for Julius Caesar; immortalized in the reference to the Ides of March in Shakespeare's classic (Ides, the dictionary tells us refers to the 15th day of March, May, July or October or the 13th day of the other months). March is also the month which marks the transition in India from one financial year to the next. The distinction between the 'calendar year' and the 'fiscal year' appears to have been made long ago. Somewhat ironically, the new financial year begins on April 1, which is widely marked as All Fools' Day. In most cash-strapped, government-supported institutions (and there are many in this class), financial managers can heave a sigh of relief, because the new year (fiscal, of course) brings with it an authorization to spend budgeted amounts. Paradoxically, March also marks the month where some government-aided institutions spend money in a rush, to avoid 'lapse' of their sanctioned grants. The headlong rush to exhaust resources can lead to strange situations. Roads are repaired, buildings are painted and huge amounts of stationery purchased, even as the financial year slowly ebbs away. In some laboratories large sums of money are committed to major purchases in February and March, with salesmen for equipment, circling like vultures, in anticipation of a feast. Libraries, which have spent the year in comfortable somnolescence, are suddenly active; indiscriminately acquiring books that are thrust upon them, by suppliers eager to divest themselves of their stocks. In the final charge to spend allotted budgets, many needless and avoidable purchases are made. The financial and administrative procedures are sufficiently archaic, to ensure that optimum use of resources is never an overriding concern.

The origin of the problem is, of course, the manner in which budgets are sought and approved; compounded by the inevitable delays in finalizing sanctions and releasing funds, once the new fiscal year gets underway. For many researchers poised on the verge of penury (only as far as their research activities are concerned), a natural urge is to hoard funds for a rainy day; particularly, as large unanticipated expenditures can accompany breakdown of critical equipment or result from a need to urgently perform, previously unplanned experiments. However, a surplus of funds as the end of the financial year approaches can trigger off a spree of hasty purchases, driven almost solely by the expedience of exhausting allocated funds. The sanctity of specific budget heads in sponsored projects, with prospects of crossover almost non-existent, ensures that savings in one head cannot be transferred to another.

Curiously, government departments which spend excessively, often with little to show by way of return, are considered efficient and successful. In contrast, departments that return unspent money are labelled as inefficient and can expect to have their budgets cut in future years. Why is profligacy rewarded? The answer is clear. Government departments are allocated funds for developmental activities; the science agencies and ministries being primarily charged with promoting research and technological advance. Failure to utilize funds is a sign that the primary function of the department is not being properly addressed. Even the formidable reports of the Comptroller and Auditor General of India draw attention

to the cardinal sin of unspent allotments. Clearly, then, it is in everybody's (?) interest to rush into an orgy of spending as March draws to a close.

At the level of individual laboratories and institutions, dispassionate observation of the March 31st syndrome, can be quite enlightening. Repairs and maintenance, which should have been carried out in the normal course of events, are invariably rushed to completion as the end of March draws near. The imperatives that dictate the prudent management of personal finances, are rarely dominant in determining the course of financial management, at the laboratory or institutional level. The big spenders are invariably guaranteed a larger share of the cake in subsequent years, as each succeeding budget uses the expenditure figures for previous years. The aura of mystery that accompanies the annual Union budget, which in a 'politically good' year arrives at the end of February, is largely misplaced, as far as allocations to Government departments are concerned. We are generally comforted, by the fact that expenditures of public money are always accompanied by the inevitable audits. However, financial audits appear to be almost always concerned exclusively with the issue of whether the rules in force have been followed in the process of spending money. Only infrequently are questions asked about whether the money has been spent wisely and usefully. This weakness in our public management systems results in many superfluous acquisitions of materials and equipment as the financial year draws to a close. Visitors to many government laboratories attached to various ministries, flush with R&D funds allocated for uncertain purposes, are often surprised at the range of sophisticated (and sometimes unused) equipment that have been acquired in the dying days of March. Fortunately, most academic institutions, universities prominently among them, have little to spend on equipment. Their excesses in March are therefore modest and only mildly amusing.

P. Balaram

CURRENT SCIENCE

Volume 78 Number 7 10 April 2000

EDITORIAL

Ageing

'The boast of heraldry, the pomp of power,
And all that beauty, all that wealth e'er gave,
Awaits alike th' inevitable hour:
The paths of glory lead but to the grave'.

Thomas Gray: Elegy.
Written in a Country Churchyard

There is no greater certainty in life, than death. The biological process of ageing is steady and unidirectional. But, a century of advance in the biomedical sciences has dramatically lengthened the human life span from an average expectancy of two decades at the beginning of the second millennium to well past the biblical 'three score and ten', in the developed world, a thousand years later. It is estimated in the United States that the fastest growing section of the population is the group over 85 years of age. With the remarkable control that has been achieved over the infectious diseases of childhood, the elimination of smallpox and the substantial gains that have been made in many parts of the world in limiting the impact of diseases like tuberculosis and malaria (and not withstanding the drumbeating on AIDS), people seem destined to live, on an average, to a riper age. In India, the visible explosion in the population has hidden an interesting fact; the annual growth rate has hovered between 2.0 and 2.2 per cent, a figure that assured doubling in the first 35 years after independence. But, this growth has masked the steady decline in both the birth and death rates, leaving their difference more or less unaltered over the period 1960–1990 (Mari Bhat, P. N., *Curr. Sci.*, 1992, **63**, 440). The consequence is, inevitably, that the number of elderly are on the rise, bringing sharply into focus a new set of problems that need to be addressed.

Oliver Goldsmith said it well:

'O blest retirement, friend to life's decline,
Retreats from care, that never must be mine
How happy he who crowns in shades like these
A youth of labour with an age of ease;
Who quits a world where strong temptations try,
And, since 'tis hard to combat, learns to fly!
⋮
Bends to the grave with unperceived decay,
While resignation gently slopes the way;
And, all his prospects brightening to the last,
His heaven commences ere the world be past!'

'The Deserted Village'

There is no greater desire in the elderly, than to fade away without a major impairment of their physical and mental faculties. But, the harsh realities of biology dictate that ageing populations face a threat from a host of diseases of old age, that can significantly restrict both physical and mental functions. The threat of strokes is enhanced and advancing age is a major risk factor for a spectrum of neurological disorders. In November 1906, the German physician Alois Alzheimer, presented a brief and unheralded report at a conference of psychiatrists entitled 'On a Peculiar Disease of the Cerebral Cortex'. Having worked for 14 years after his MD as an intern and chief resident at a 'lunatic asylum' in Frankfurt, Alzheimer, who was labelled as the 'psychiatrist with a microscope', was uniquely positioned to make his seminal observations on the pathology of the brains of patients suffering from senile dementia (Hoehn, H., *Curr. Sci.*, 1992, **63**, 407). Alzheimer's disease, now recognized as a 'multifactorial syndrome', is accompanied by the formation of neuritic plaques and neurofibrillary tangles, which consist of a hopelessly intertwined mass of insoluble protein. Together with Parkinson's disease and ischaemic brain injury in strokes, Alzheimer's syndrome is one of the greatest threats to the well being of the elderly (For an excellent overview of neurological disorders see, *Nature* (*Suppl.*), 24 June 1999, **399**). The diseases of old age indeed constitute a major challenge for contemporary biomedical research.

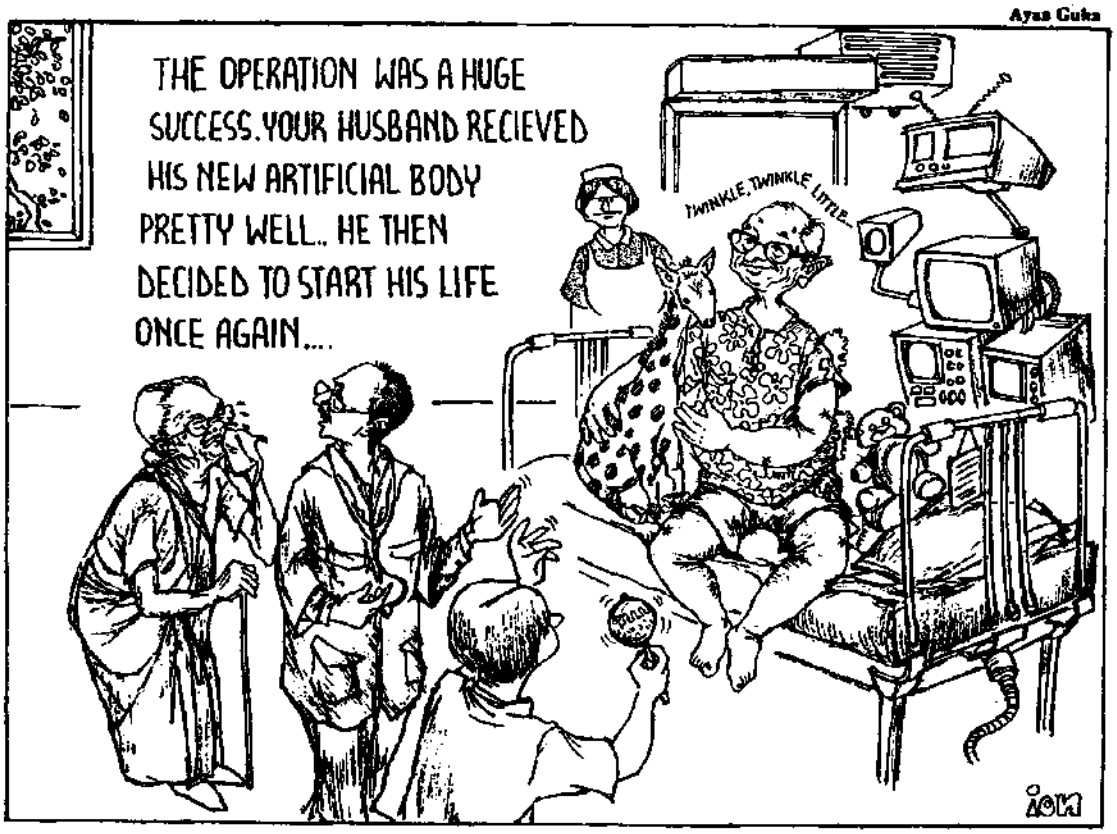
It is in this context that two recent essays in *Nature* merit attention. Leonard Hayflick (*Nature*, 2000, **403**, 365), noted for his finding that cells age and die in culture, argues that the success of the biomedical enterprise

over the last century has 'unmasked a process for which evolution never prepared us. One might conclude that ageing is an artefact of civilization.' Hayflick's thesis is that 'the expression of age changes is not essential for the survival of any species'; an argument that he buttresses by pointing out that 'humans have survived, and sometimes flourished, with a life expectancy at birth of 20 or 30 for more than 99.9 per cent of our time on the planet'. In analysing ageing, Hayflick has a clear agenda; to highlight the failure of science policy 'to distinguish research on age-associated diseases from research on the fundamental biology of ageing'. He argues that for all the sums of money spent on Alzheimer's disease research in the United States, 'its elimination will have only a trivial impact on life expectancy'. He ignores, in the heat of the argument, that a resolution of Alzheimer's disease will provide an enormously enhanced quality of life to an ever-increasing segment of the population. But it is hard to quarrel with Hayflick's thesis, when he says: 'greater attention must be given to the question that is rarely posed, yet which is capable not only of advancing our fundamental knowledge of ageing but also our understanding of age-associated diseases; why are old cells more vulnerable to disease than young cells?'.

In terming the past hundred years as 'a century of cognitive decline', Bruce Yankner (*Nature*, 2000, **404**, 125) argues that 'the twentieth century has witnessed a dramatic prolongation of life span, but little progress in preventing age-associated cognitive decline'. The key issue is the extent to which normal ageing processes are associated with cognitive decline. The abnormal pathology associated with Alzheimer's disease argues 'against the idea that senility is normal' and gives 'rise to the hope that Alzheimer's disease could be prevented or reversed'. Yankner points out that 'the continuum between normal ageing, mild cognitive impairment and Alzheimer's disease provides support for an old idea, namely that Alzheimer's disease may be an accelerated form of brain ageing'. Here, Yankner raises a sobering thought: 'if we live long enough, will we all become demented'.

The last decade of the twentieth century was, with customary fanfare, labelled as the 'decade of the brain'. It does, however, appear that the brain will take considerably longer to yield its secrets. Many questions of age-related changes in brain functions may take decades to answer and immediate therapies to arrest cognitive decline do not seem to be around the corner. But, the management of the cognitively impaired will benefit greatly from the widespread recognition that loss of seemingly routine abilities are an inexorable and unavoidable consequence of biological processes that we do not yet fully understand. We might all be reminded of the famous saying 'Ask not for whom the bell tolls; it tolls for thee'.

P. Balaram



CURRENT SCIENCE

Volume 78 Number 8 25 April 2000

EDITORIAL

Drug research: A growing gap

The costs of developing a new drug keep escalating as the research behind the discovery of every new candidate molecule becomes ever more sophisticated and the regulatory process becomes more stringent and long drawn out. An estimate made some years ago suggested that it costs as much as 500 million dollars (Rs 2000 crores) to develop a single successful product. There are lower estimates, but these would only scale down the cost by a factor of two, at best. The costs, of course, include the expenses of following many false trails. In order to keep ahead of the competition multinational pharmaceutical companies need to make huge investments in research and development; an imperative that has catalyzed many mergers in the drug industry. The marriage of Glaxo Wellcome and Smith Kline Beecham has created a behemoth, with an estimated revenue last year of $23.6 billion and an annual R&D expenditure of about $3.7 billion. But, if these figures look large, the Pfizer–Warner Lambert alliance seems to have created a competitor for the position of champion R&D spender; with the research budget of $4 billion from a total revenue of $29.1 billion. (To help readers, who like this writer find conversions difficult, 1 billion dollars translates to approximately Rs 4000 crores.) In analysing the spate of mergers a recent article sums up succinctly: 'being the biggest kid on the block has become the hottest game in town' (B. Agnew, *Science*, 2000, **287**, 1952).

But why is research aimed at new drug development becoming so expensive. After all Alexander Fleming discovered the antibacterial properties of mold secretions serendipitously; although it did take a few years and the skills of Ernest Chain and Howard Florey, spurred on by the exigencies of wartime, to produce penicillin in the 1940s. Did not Edward Jenner produce the almost magical smallpox vaccine based solely on his keen observation of the resistance of milkmaids exposed to cowpox? Was not Pasteur's rabies vaccine discovered with little investment? Is it not a fact that aspirin, so widely used today, has had many of its beneficial effects discovered accidentally over the course of a century of use? Unfortunately in modern times the process of drug discovery has become more complex, the criterion that must be met before human use has become extremely strict and the range of diseases for which therapeutics are sought has widened dramatically. The thalidomide tragedies of the 1960s provided a lesson that will not be forgotten. Today any new and promising molecule must pass through several phases of testing to detect toxic effects before approval for human use. These procedures cost both time and money.

An obvious corollary of the high cost of drug research is that companies will invest only in R&D activities, that ensure the highest returns if a successful product emerges. Research in the areas of diabetes, hypertension and cardiovascular disease, central nervous system disorders and the ailments of old age and cancer among others, may have the highest chances of a pay off, with large, anticipated markets in the developed world. In contrast, many 'Third World diseases' like malaria, filariasis, leishmaniasis ('kala azar') and a host of others may be poor targets for attack. Even if a successful therapeutic is developed, the possibilities of recovering the costs of R&D and turning a tidy profit are poor. Focusing on diseases which afflict a large population steeped in poverty, can hardly be considered as a viable strategy for a pharmaceutical company driven by the imperatives of the market place. The recent resurgence of tuberculosis research in the West may be traced directly to the reemergence of the disease in the developed world, in the wake of immune suppression in AIDS victims. The emergence of drug-resistant strains adds a new dimension of urgency. The fight against the orphan diseases of the Third World has sometimes benefitted from the munificence of rich governments subsidizing the costs of private R&D and from a few acts of philanthropy by large companies; African river blindness and trypanosomiasis ('sleeping sickness') are two examples, where successful therapeutics have emerged from multinational R&D laboratories. But, it is unlikely that future struggles against the diseases specific to the Third World can rely exclusively on well-intentioned charity.

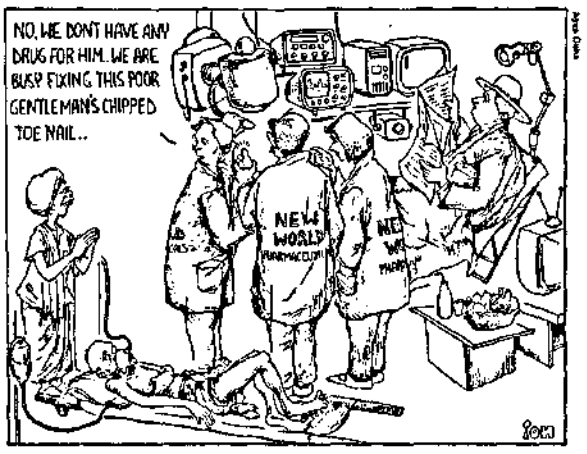
What is the situation in drug research in India? The pharmaceutical industry in this country has grown on two strengths; synthetic chemistry and chemical engineering. Clever process development has permitted the economical production of well-known drugs, under the

umbrella of patent laws which do not allow protection of molecules; the process patent and not the product patent has allowed cheap, legal production of bulk pharmaceuticals. Understandably, multinational companies which invest hundreds of millions of dollars on R&D have always felt cheated; but most often these companies have engineered abnormally high prices in their own native markets, pleading high costs of production. But the rules of the game are set to change soon, as the implementation of the TRIPS agreement will result in the protection of product patents in India. This sceptre has seen Indian pharmaceutical companies enhance their R&D spending; but no single company in India has the financial muscle power to even imagine competing with the multinationals. The pragmatic strategy appears to be the hope that R&D efforts can result in some leads, which can then be licensed to major international companies, which in turn will then underwrite the costs of future development. The Third World's basket of diseases are unlikely to attract much support in this scenario.

As in most other spheres, thus far the Government has been the major supporter of research in the area of drug development. Several national laboratories and academic institutions have ongoing programs in this broad area; the Central Drug Research Institute, Lucknow serving as the flagship of this enterprise. But, as in other areas, the sheer pace of advance in biomedical research has left Indian institutions completely in the lurch. From a field which relied predominantly on chemistry, pharmacology and clinical sciences, contemporary drug research requires major inputs from fast moving fields like genomics, structural, molecular and cellular biology, which constitute the fundamental core of modern biotechnology. The rapidly developing methodologies of combinatorial chemistry and high throughput screening, which are at the heart of the new paradigm of 'irrational drug discovery' are still largely unknown and unpractised in India. The level of technological accomplishment in the laboratories is primitive, handicapped as we are by a lack of resources and more importantly, manpower of the right kind. The cutting edge of drug discovery research is a confluence of several disciplines, which bud off from the major streams of chemistry, biology, physics and computer science. This interdisciplinarity poses many problems, in an environment where boundaries between departments are drawn in immovable stone. Modern drug research also requires an organized effort; an orchestrated team game in which individual interests may prove subservient to a larger goal. Paul Ehrlich knew what he was talking about when he said: 'Laboratory work is child's play in comparison; either a thing will go or it will not, and that is the end of it. But if you have to depend on hundreds of collaborators, and each of them believes that he can do better than any other, life really can be made rather difficult and bitter'.

Our successes in other 'mission mode' projects in the strategic R&D arena are sometimes held as models for the conduct of organized research, directed from the top. But it must be remembered that the committment of resources and organizations to these programs have been substantial and the technical goals clearly defined. The construction of an atomic bomb or even the vastly more useful communications satellite require the implementation of tested designs and procedures. The true 'intellectual property' is already available. In the area of drug research the identification of targets and the methodology for attacking the enemy are much less well defined. There are also no visible institutions and personalities to champion major initiatives in this area. But the fact remains that we need to effectively combat the many threats to human health, particularly infectious disease caused by microbial pathogens in our surroundings. Some years ago Daniel Koshland, then editor of *Science*, highlighted the problem by emphasizing the fact that 'because of the capacity of microbes to adapt to new circumstances there will probably be a continuing battle for many years, a subterranean war in which complacency and lack of determination can result in pain and death' (*Science*, 1992, 257, 1021). There are many wars to be fought in the quest for the new therapeutics of the future. It is time that our agencies and institutions recognize the magnitude of the problem and the all-too-obvious limitations of our laboratories.

P. Balaram



CURRENT SCIENCE

Volume 78 Number 9 10 May 2000

EDITORIAL

Gresham's Law

In the mid-16th century, Thomas Gresham, a financial adviser to the English Crown, postulated a principle that simply states, 'bad money drives out the good'. Textbooks of economics have elevated this statement to the level of a 'Law', which appropriately bears Gresham's name. In the world of economics Gresham's law enters discussions of bimetallic standards (gold and silver, of course) and is stated in somewhat more formal terms: 'When two kinds of money equal in nominal value but not in terms of demand are in circulation the less desirable will drive the better money out of circulation'. Gresham's law in its simpler form, quoted out of context inevitably, seems an appealing statement of a condition that does not seem to be bounded by any discipline, 'bad drives out the good'. Does a form of Gresham's law operate in science? More specifically paraphrased, 'Does bad science drive out the good science from our institutions?'

It is not entirely simple to define good science and bad science. A great deal of science that is practised may simply be pedestrian or indifferent, but is often carried out with the best of motivations. It is not uncommon to find a large body of data, often gathered without a guiding hypothesis or a purposeful goal, forming the bedrock on which major scientific advances are based. At its best, science provides revealing insights into the world around us. The most widely used technologies generally rest on a sound base of fundamental science. It is not often appreciated in lay circles that in the past some of the most dramatic technological revolutions have been born as unheralded curiosities, in the setting of an isolated laboratory or in the minds of far-seeing individuals. Faraday's discovery of electricity and Franklin's demonstration of the remarkable calming properties of a spoonful of oil on the turbulent waters of Clapham pond are etched in the pages of the history of science. More recently, few would have imagined that the nuclear magnetic resonance (NMR) images produced of two glass tubes nestling inside a larger tube, in the early 1970s, would lead to the flowering of a new and remarkably powerful mode of medical diagnosis, magnetic resonance imaging (MRI). Readers might note that by a clever sleight of hand public acceptability of the technique was assured when the word 'nuclear' was dropped in the name.

Few will argue with the definition of outstanding science; most will agree on what constitutes good science and many will have little difficulty in identifying what constitutes reasonable (and essential) scientific research. What then constitutes 'bad science'? Here, opinions may vary sharply and public postures may be completely at odds with private practice. But to any discerning observer of the teaching of science and the conduct of research in publicly funded institutions, bad practice is easily recognizable.

In our universities and academic departments across the country research done towards a Ph D degree is of extremely variable quantity and quality. In the best of institutions, the requirements for Ph D awards match international standards. In many others, limited facilities and resources constrain the nature of research carried out. But in poorly endowed institutions the flag of 'good science' is carried by a few sincere and scholarly researchers, who go to great pains to teach their students the methodology of research and the importance of dispassionately viewing the results of experiments, even when they run contrary to a pet hypothesis. Unfortunately, these intellectual islands are far and few between. They are isolated outposts in an arena where Ph D students are treated as a commodity to pad the mentors' curriculum vitae, with a large number of 'research students guided'. Quite often, students (of varying quality) are recruited to carry out research which has little relevance to contemporary science and, even more disturbingly, is not of even of passing interest to the mentors. Ph D theses are produced in a time-bound fashion; ironically the poorer theses are completed in very short times giving their authors a head start in a job market, which places great store on degrees. The 'autonomous' character of academic institutions ensures that no standardization of research requirements is possible. The Senates, Syndicates and Academic Councils of educational institutions are a bulwark against the outside world.

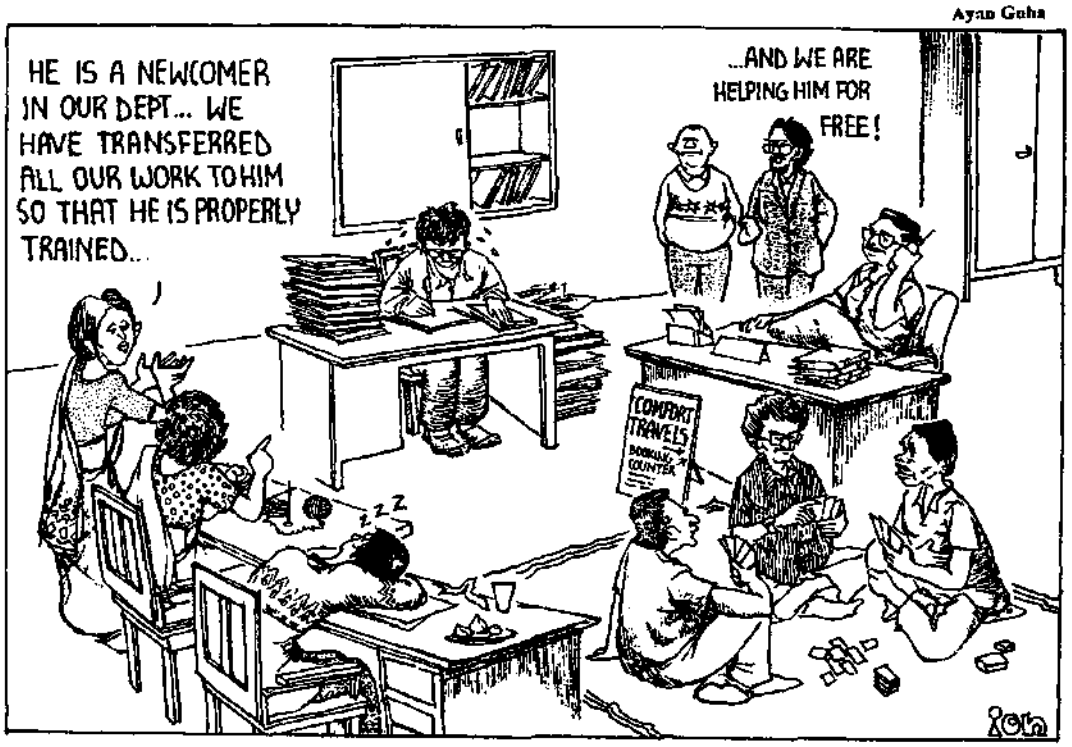
At the level of advanced research we have the spectacle of mega research projects which promise the desirable,

knowing fully well that deliverables are never going to appear. The creation of centres and institutions based on flimsy promises, which siphon a major share of the money for scientific research, is a disturbing trend. Institutions which will be hard pressed to recruit qualified and motivated researchers then become a focal point for the practice of what may be termed as 'bad science'. The signals sent to the scientific community by the continued support of projects on which the results obtained are obscure, is a means of shoring up and encouraging bad science.

The teaching of science is another area where it is important to recognize the 'bad'. In staff rooms across the country there are an increasing number of teachers who do not teach inside the classroom, but get away with impunity. There is a wonderful market for teachers in the parallel academic sectors – the mushrooming tutorial classes, which coach students for the ever-increasing number of entrance tests. The truly inspiring teachers who motivate students towards a career in science are an endangered species. The atmosphere in most staff rooms is hardly conducive towards maintaining the enthusiasm and interest of committed teachers. The distractions of an environment, where a significant number actively shirk the work for which they are paid, cannot be underestimated. In an effort to encourage research in colleges, agencies over the years have been keen to fund small projects to teachers in order to sustain their interest in original science. The takers have been relatively few and a common complaint is that the local ambience is generally very discouraging. The rise of 'information technology' and the demands for courses in this area have also created a new problem. More computer courses are likely to be offered commercially, while the very same classes within academic institutions are going to be approached with extremely limited enthusiasm. The lure of money is irresistible, and the 'new science' of the internet appears to have a direct connection to Mammon.

There will undoubtedly be many opinions on what constitutes 'good science and bad science'. Both facets of science may appear in many different manifestations. But, like in the simple tales of childhood, the 'good' appears to be constantly threatened and is in danger of being overrun by the 'bad'. Since I began with the somewhat unrelated issue of Gresham's law, it is appropriate to return once again to economics. In his famous textbook Paul Samuelson (*Economics*, McGraw Hill, 1973, p. 651), comments: 'Today no one, not even a Kansas farmer favours bimetallism'. Maybe we should all try to only favour good science.

P. Balaram



CURRENT SCIENCE

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EDITORIAL

The impact of publication lists

Scientists in academia are usually judged by their lists of publications. Unlike their counterparts in industry, whose work is assessed by their abilities to translate their work into useful products (and hence, money), the success of academic work seems inevitably pegged to the number of published papers, that emerge from a laboratory. In the academic world, writing thus, becomes a major activity. In the last quarter of a century, the growth of journals has been phenomenal; the present volume of scientific literature is unmanageably large. Every subfield or super-speciality has its own group of core journals. The truly interdisciplinary journals have reached a stage, where apart from the news and general articles on the first few pages, very few of the papers they carry are intelligible to anyone outside a narrow field. The torchbearers of the electronic revolution would have us believe that computers, unlimited internet access and digital libraries, powered with marvellous 'search engines' will allow scientists (among others) to cope with the torrent of information. But, few would argue with the fact that coping with the literature is becoming difficult and in some fields, indeed impossible. An enormous number of papers, published worldwide, therefore remain unread. Their only apparent utility is that they add another title to the lists of publications of the authors.

Why do scientists publish their results? Firstly, of course, there is the great desire to be recognized as having contributed to the solution of a scientific problem. Secondly, there is the more mundane imperative; published papers appear to be the only available yardstick to measure the work done by an academic scientist. Even a freshly minted PhD is unlikely to be considered for a postdoctoral fellowship, unless he or she sports a moderate body of published work. The CSIR, for example, limits the number of candidates being considered for senior research fellowships and research associateships using a quantitative 'publications filter'. Further up the totem pole of the scientific enterprise, publications acquire an even greater importance. Appointments committees and award-giving bodies hungrily turn to the lists of publications of the candidates being considered. It is hardly surprising that publishing papers becomes a central activity in academia; sometimes even pushing the actual conduct of research into the background.

How do scientists judge the quality of the lists of publications of their colleagues in the perpetual process of 'peer review'? Since it is almost always impossible to read all the papers and come to an informed judgement, it is critically important to read meaning into the titles of papers and the names of the journals in which they are published. Since judgements are invariably perceived to be subjective (and often, prejudiced), a clear, objective, impersonal criterion would be desirable. Eugene Garfield provided the 'breakthrough' in the area of 'science watching', by introducing the concept of citation counts and impact factors in the 1970s. The early days of citation analysis preceded the modern computer revolution (there was even a printed version of the voluminous *Science Citation Index*); but the methodology was ideally suited to the digital age. Garfield's idea was simple. Why not look through the reference lists in papers and catalog the number of times each paper is cited (and by extension such counts can be carried out for authors and journals). Citation analysis has now spread like wildfire; many analysts untrained in the methodology of science, ignorant of the limitations of the databases they use and intellectually unprepared to look beyond numbers, produce widely-quoted studies of dubious value in India. But the inescapable fact is that there are now clear numbers that can be written next to papers, authors, journals and even institutions; much like the dreaded 'marks cards' of schooldays. We have reached a wonderful stage, where all subjective judgements can be suspended; we do not even have to read a paper, much less understand it. Since counting citations to individual papers is a non-trivial task, assessments are simply based on the 'impact factor' of the journal in which they are published. Journals now flaunt their impact factors (obviously, only when they are high) in advertisements. Many scientists make decisions on the

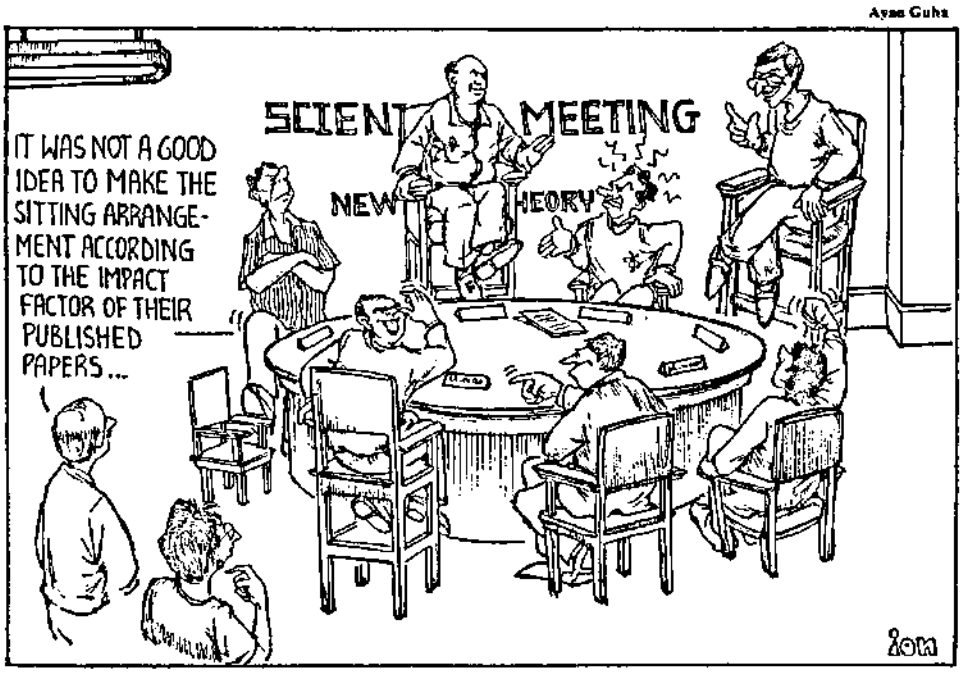
destination of their papers by scanning lists of impact indices. In committees that decide grants and awards there is an irresistible temptation to abdicate independent judgement at the altar of the impact factor. In the rush to be quantitative and impersonal, 'new parameters' of performance have been invented. There is the 'average impact factor', quite popular in India nowadays, where an individual's (or institution's) net worth is simply summarized by a single number.

The growing emphasis on impact factors signals a new trend. It is no longer important whether a paper has been cited substantially; it is sufficient if it has been published in a journal with a high impact factor. In a provocative experiment some years ago, the Indian Institute of Science carried out an analysis of faculty publications. Beneath the easily interpretable numbers lay some interesting facts. Papers that appeared in 'high impact' journals tended to be cited fewer times than the average for the journal. In contrast, papers that appeared in relatively 'low impact' journals tended to be cited more often than the journal average. The simple-minded interpretation (with a little license, of course) was that papers from India (or more specifically, the institution analysed) tended to lower the 'impact' of the top journals, while they raised the 'impact' of the middle level journals. A corollary conclusion, to which many Indian scientists would subscribe is that many good Indian papers are cited fewer times than similar studies by Western authors .

Why is this so? A possible reason is that many authors pad reference lists with citations to scientists who are likely to act as reviewers. Woe betide the author whose paper ends up on the desk of another scientist in the field, who has not been cited. Human nature being what it is, most of us are suffused by a warm glow, when we see our names in the reference lists of a paper we are asked to review. Since the likelihood that papers submitted to Western journals will end up in the hands of Indian reviewers is low, there is little need to be careful in citation of Indian work. There is a considerable degree of gamesmanship (and understandably so) in providing citation to the literature. Little clubs of scientists who cite one another but exclude other relevant work, spring up and prosper. Claims of originality can hardly be sustained, even for a brief period, if the author is overly generous in ascribing credit for past or related work. In these competitive times, selective non-citation may be an important weapon in playing down the contributions of rival groups. At the other end of the spectrum, it is sometimes easier to shepherd a paper through the review process at 'high impact' journals, by pandering to the egos of potentially powerful reviewers (and on occasion, editors).

In assessing scientific work, citation counts provide a more reliable indicator of a paper's worth, than the impact factor of the journal in which it was published. There is, of course, no substitute to reading published papers or hearing a scientist describe his or her work and making an informed (and necessarily, subjective) judgement. Since this is well nigh impossible, given the volume of ongoing work in most areas, the bare statistics of the scientometrists provide a seductive solution. But, the next time any influential decision maker states that a specific paper must be important, merely because it has appeared in a glamorous journal we might ask: 'Have you read the paper?'

P. Balaram



CURRENT SCIENCE

Volume 78 Number 11 10 June 2000

EDITORIAL

Intellectual Property Rights

IPR is an acronym that hardly needs to be expanded nowadays. Everyone, who matters in scientific circles, is talking about intellectual property rights and the importance of protecting scientific discoveries, with commercial potential, in a tight maze of patents. In any discussion on collaborative agreements between different institutions, public and private, the first question asked is: 'Has our IPR been protected'. In seminars and meetings on IPR, speakers expound on the technicalities of patent protection, the importance of valuation of intellectual property and the need to ensure that 'inventors' (and their institutions) get a substantial piece of the cake of commercial success. At times discussions on IPR turn so turgid and far removed from science, that I cannot help drifting back to simpler times. There was a time when many naive scientists associated patent offices only with the body in Switzerland, which provided shelter to the young Einstein as he worried about matters which were to become important (but unpatentable) in the future. There was a faint air of romance in the practice of science and there were still scientists who might be described as Alice was by Lewis Carrol: 'a child of pure unclouded brow and dreaming eyes of wonder'. People were drawn to science for differing reasons. Max Perutz said it well, in his review of Peter Medawar's book, *Advice to a Young Scientist*: 'It seems to me that, just as the Church did in former times, science offers a safe niche where you can spend a quiet life classifying spiders, away from what E. M. Forster called the world of telegrams and anger. To the ambitious poor, science offers a way to fame or reasonable wealth that needs no starting capital other than good brains and energy.' In reflecting on the transformation that has taken place over the last few decades, Perutz says: 'Young people entering science now need more talent and determination to make their way than they did in the 1930s. . . . Good science is no rose bed, but the romance is still there. The thrill of discovery outweighs the drudgery, the despair at one's inadequacy, the fight for financial support, the setbacks and mistakes, the long hours, and the nagging fear of being overtaken. A discovery is like falling in love and reaching the top of a mountain after a hard climb all in one, an ecstacy induced not by drugs but the revelation of a face of nature that no one has seen before. . . . A true scientist derives this feeling not only from his own discoveries but also from those of his colleagues.' In a more down-to-earth vein Perutz notes that 'research is supported by the state and by industry not primarily to finance such expensive ecstacy but in the hope that it will produce useful results. During the last ten years there has been much discussion about the proportion of funds that ought to be allocated to basic and to goal-oriented research and it has become difficult for young people to decide which way they should turn, especially since it is much easier to get funds for the latter'.

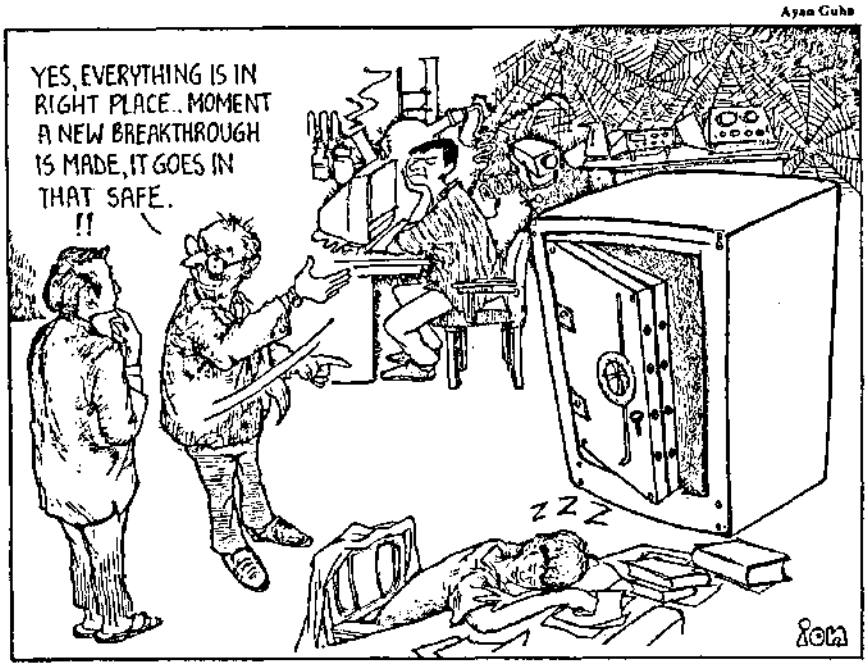
Liberalization and globalization have dramatically altered perceptions about science and its practice in India. The unabashed drive to patent and protect every conceivable scientific advance, no matter how incremental, has now reached a ridiculous level in the West. American and multinational companies never known for moderation and thoughtfulness, when commercial interests are involved, have set out to fence vast areas of science under the guise of protecting intellectual property. Discussions rage (and the only beneficiaries are often patent attorneys) on whether organisms and genes, surgical procedures and widely known methodologies can be patented. The public spectacle of Celera Genomics Corp., a private company, and the US National Institutes of Health led consortium making claims and counter claims on the human genome, is an indication that many bruising IPR battles lie ahead in the area of 'biotechnology'. In some areas, institutions frown on their faculty freely parting with research results and materials. The recent discussion on transgenic mice (*Science*, 2000, **288**, 255) highlights the legal hurdles that researchers may eventually have to cross in the conduct of their research. The attempt to patent turmeric uses and basmati rice illustrates that the IPR protection area can turn into an expensive theater of the absurd. In an article entitled 'Patent Absurdities' Seth Shulman asks: 'Is the delivery of health care a public service or a business? Should patent owners and their lawyers profit from carving up medical knowledge into privately held parcels?

Which aspects of medicine must be held in common for the greater good?' He makes the telling point that 'even a nation that champions private property can condone a national park system that preserves some land for shared use. Unlike land or other forms of tangible property, knowledge is not depleted by use. On the contrary, books, software programs and medical procedures lose their value and utility when they are *not* used. The development of new knowledge often requires a significant initial investment of capital, and of course those costly efforts merit recompense. But unlike traditional assets, once a new piece of knowledge exists, it incurs virtually no marginal costs from its ongoing use and dissemination'. Shulman concludes by asserting that 'unless society tackles the issue head-on, the privatization of knowledge assets will choke productivity, magnify inequities and erode our democratic institutions' (Shulman, S., *The Sciences*, Jan./Feb. 1999, pp. 30–33).

But, the world of IPR and commercialization of scientific success is largely one-sided and Western-dominated at present. There has been some pressure to ensure that inputs into science are transformed into useful products in India. Scientists, particularly those in national laboratories are being encouraged to patent before they publish. The most sought-after patent is, of course, a 'US patent', which costs a fairly sizable sum of money to file. Institutions and agencies are only too happy to underwrite these costs as we are after all engaged in the important business of protecting 'intellectual property'. Once granted, the holders of US patents are as proud as their counterparts who flaunt papers published in 'high impact journals'. The fact that most patents lie unread and unlicensed is usually conveniently forgotten; the triumphantly trotted out lists of patents often only document assets, whose value is purely notional. A large number of patents which are filed do not even have a chance of attracting a potential licensee; they are like the many papers in the literature, which lie buried, unread by anyone. Why then is good public money spent on protecting worthless 'intellectual property'? Presumably, this is done because there do not exist internal mechanisms in many laboratories for technical and commercial evaluation. Patents have also become a device by which considerable amounts of irrelevant work are masked under a camouflage of potential utility.

The endless discussions on protecting IPR mask a troubling question: Do we have a great body of 'intellectual property' being generated that merits protection? Disturbingly, many discussions centre on protecting 'ancient knowledge' and 'indigenous resources', that often lie unused, from external predators. Few focus on the fact that there needs to be greater emphasis on supporting internationally competitive research in our laboratories. Indian academia and industry have in recent times, been taking hesitant steps towards collaboration, with match making often facilitated by carrots profferred by Government schemes. However many agreements have run into rough weather over potential IPR clauses. This is rather like collaborative research, which flounders even before the first experiment is performed, because of disagreements on the order of authors in the eventual paper that will be published. Ironically, many agreements are worded in such a way that the pleasantest collaborations may be those where there is little chance of scientific or commercial success. The IPR debates are not going to fade away in the near future. We might, however, begin to wonder how we can generate more useful 'intellectual property' so that the task of protecting it becomes worthwhile.

P. Balaram



CURRENT SCIENCE

Volume 78 Number 12 25 June 2000

EDITORIAL

Genes and genomes

Large multiinstitutional and multinational projects have a tendency to run behind schedule and are usually characterized by substantial cost overruns. A major exception has been the Human Genome sequencing program, which was originally slated to end in about 2005, but appears to be reaching the finish line five years earlier. When whole genome sequencing was contemplated in the 1980s, most serious molecular biologists were loath to support a project, which appeared to be a monotonous, technology-driven exercise that might cause a large diversion of funds in the United States from the traditional 'small science' of individual investigator-driven projects. There was a general impression that the search for disease genes may be accomplished by approaches that did not involve the enormous labour of complete sequencing. But the skeptics were to be proved wrong, on almost all fronts, as sequencing technologies and data handling capabilities were revolutionized in the 1990s. The last few years have seen a dramatic acceleration of the rate of sequencing by consortia in Europe, Japan and the United States. Genome after genome has succumbed to this onslaught. The most notable recent victories on the battlefield of sequencing include the genome of the fruitfly, *Drosophila* (*Science*, 24 March, 2000), and a host of small genomes of pathogens, including *Mycobacterium leprae* (*Science*, 2000, **288**, 800). The publication of the sequences of chromosome 21 (M. Hattori *et al.*, *Nature*, 2000, **405**, 311) and chromosome 22 (I. Dunham *et al.*, *Nature*, 1999, **402**, 489), the smallest of the 24 human chromosomes, marks the beginning of the end of the sequencing phase of the Human Genome project. It is clear now that we will soon have available the sequence of the 3 billion plus DNA bases, that constitute the core of human inheritance. The last battles for the human genome have been marked by intense competition between publicly funded groups spearheaded by the US National Institutes of Health and a private company, Celera Genomics founded by Craig Venter, which appears to be repeatedly demonstrating that the best catalyst for scientific progress in the modern age may be the smell of commercial success. The race for an almost mythical finish line and the growing controversy over the release and sharing of data, have obscured the issue of what happens after all the sequences are in and safely stored in digital databases. One of the more difficult aspects of war is managing the peace that follows. The genome wars of today are likely to throw up a host of new problems that might still be unanticipated.

In pondering on genomes it might be useful to ask: 'what are genes?' Some years ago on the verge of the biodiversity convention at Rio de Janeiro, a newspaper featured a cartoon of the inevitable minister about to head for the jamboree, plaintively asking an assistant: 'what is a gene'. At first glance this appears a ridiculously simple question. The acceptable answers in a quiz or an examination are many. Some will hedge their bets and say generally, that 'a gene is an unit of inheritance'; classical genetics, of course, associates genes with specific phenotypic characteristics of organisms. A stiff and formal answer found in the book *Molecular Biology of the Cell* by Alberts *et al.*, states that 'a gene is defined as a nucleotide sequence in a DNA molecule that acts as a functional unit for the production of an RNA molecule'. And, elementary molecular biology tells us that if RNA is around the corner, proteins cannot be far behind. But, in today's context the genome gold diggers are primarily looking for sequences that will translate into protein products. After all, as Arthur Kornberg said some years ago, 'in my theater, the nucleic acids write the script but the enzymes (proteins) do the acting'. If all the protein sequences in an organism ('the proteome') are the real target of the genome programs, it is clear that the post-sequencing era will be dominated by efforts to decipher the segments of DNA that translate into protein products. The human genome, of course, contains an enormous amount of non-coding DNA (once dismissively labelled as 'junk DNA' by Francis Crick); the task ahead will be to sift the wheat from the chaff. If the human genome is viewed as an incredible book written with over three billion letters, based on a four letter alphabet, it is clear that the task of annotation will

involve clever procedures to recognize genes. While this appears a simple task once the grammar of nucleic acid sequences is clear and the punctuation marks defined, in reality the problem is formidable. The field of genome analysis today is the happy hunting ground of computer scientists eager to develop ever more efficient algorithms for decoding sequence data; even IBM has now directed its most powerful computer towards deciphering genome data, presumably a more useful activity than matching wits with Gary Kasparov or Vishwanathan Anand.

Since living cells rely very substantially on proteins to carry out all their housekeeping functions, it is natural to ask: 'how many distinct proteins do human beings have?' Since we do not have a definite answer, as yet, educated guesses abound in the literature. To set the problem in context it is useful to recall that the bacterium *Escherichia coli* has ~ 4000 genes, yeast ~ 6000 and the fruitfly, *Drosophila*, ~ 13,600. Gene numbers do not necessarily readily correlate with functional complexity; the worm *Caenorhabditis elegans* eclipses the fruitfly with a complement of ~18,400 genes. While the number of genes in humans may appear to be of interest primarily to biologists, it is worth noting that the shares of some biotechnology companies, on occasion, zoom upwards when gene estimates are raised and plummet when the number is lowered. Clearly, there is money to be made in genes; the larger the number, the greater are the chances for commercial exploitation in the post-genome era.

The present status of human gene counts is highlighted by a set of three reports that appeared in the same issue of *Nature Genetics* (Vol. 25, June 2000). Using an analysis of expressed sequence tags (ESTs), B. Ewing and P. Green (p. 232) obtain an estimate of 35,000 genes. Roest Crollius *et al.* (p. 235), using an approach based on comparative genomics employing the pufferfish genome, suggest that humans must possess ~28,000–34,000 genes. These estimates are considerably lower than the oft-quoted figures of 50,000–90,000, which have appeared in the literature over the years. For those whose hearts sink as gene estimates are lowered, there is still hope. F. Liang *et al.* (p. 239) from the Institute of Genomic Research use ESTs and come to the conclusion that the number of human genes is ~1,20,000. Undoubtedly, identifying genes definitively will provide the real answer, but for now we must live with delicious uncertainty. The tasks of eventually assigning functions to all the genes in genomes and monitoring their expression in different biological contexts will be major preoccupations in the future. The gene annotation area is already attracting many theoreticians and computer scientists, but may also be advanced by those whose computational skills are also backed by sound biochemical intuition. The sequences of chromosomes 21 and 22 are only the tip of the human genome iceberg. Already, there are signs that there are enormous non-coding regions which may have some bearing on our evolutionary past. On chromosome 21, there is a 7 million base pair segment that contains a single gene; this region is larger than the entire genome of *E. coli*, the bacterium with which we share much of our basic biochemistry.

The Indian contribution to the genome analysis program has been negligible. Even as the first step towards joining a Japanese-led consortium to sequence the rice genome has been taken, Monsanto has announced the first draft of this genome. It may, in fact, be worthwhile to worry if local efforts can contribute to understanding sequenced genomes, rather than compete head to head on important targets. The example of the Brazilian effort, which deciphered the 2.7 million base-pair genome of *Xylella fastidiosa*, a bacterial pathogen that causes citrus variegated chlorosis and threatens the citrus industry in São Paulo (*Science*, 2000, **288**, 800), may provide instructive lessons for Third World efforts in genome sequencing.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Post-genome blues

Will 26 June 2000 be remembered as one of the defining moments in the history of science? After all, this was the day that the formal announcement of the completion of the rough draft of the human genome was made. On the morning after, the newspapers were full of headlines screaming of the benefits of the project to humanity. In the days that followed we were treated to extraordinary visions of cures for every conceivable disease, whose cause lies in our genes. Among the more fanciful headlines to which we were treated was one that read—'People can live for 1200 years'; a view that appeared to be copyrighted by the Times Newspapers emanating from London, quoting the apparently unimpeachable source of one of the British governments top scientists. Another headline that labelled the genome announcement as the 'biological equivalent of the moon landing', inevitably brought back memories of the almost forgotten days of NASA's missions to the moon. Would the declaration that the human genome sequence was almost at hand, compare in impact with the moment when Neil Armstrong's voice drifted eerily across space, immortalizing the phrase—'one small step for man, one giant leap for mankind'? Would it compare with the moment when Robert Oppenheimer and his troops watched the mushroom cloud develop over the New Mexico desert, as they confirmed with a definitive experiment the destructive power of the atom? This comparison with the moon landing or the first atomic explosion might in fact serve to temper some of the mindless optimism that has found its place in the media after 26 June. In the late 1960s it did appear that a new era of rapid exploration was almost around the corner. A manned Mars landing was considered a definite possibility by the turn of the 20th century. What has happened is too well known to recount here; with recent technical difficulties with Mars probes reminding us that 'there's many a slip between the cup and the lip'.

Unlike the moon landing or the decisive and terrible conclusion of the Manhattan project, the timing of the official announcement of the 'rough draft' of the human genome appears to be a carefully orchestrated, public relations exercise, to counter the increasingly acrimonious race to complete the genome sequence. The competitors were a publicly funded consortium spearheaded by the US National Institutes of Health and the Sanger Centre in Britain on one side and a private company, Celera Genomics on the other. The long arm of the US presidency appears to have brokered a peace, leading to the timing of the announcement on 26 June. The cover of the 3 July issue of *Time* features the generals, Craig Venter of Celera and Francis Collins of NIH. More famous truces, Henry Kissinger and Le Duc Tho of Vietnam fame or Anwar Sadat and Menachem Begin of the Middle East Wars, have been blessed with Nobel prizes, albeit for peace. It is not inconceivable that this will happen again, only it will be for medicine. While the sequencing of the approximately 3.2 billion bases, which constitute the human genome is a major scientific achievement, a technological *tour de force* that will be unmatched for sometime in biology, the circumstances of its public declaration of success, suggest that the political and commercial implications of the project may take a long time to be fully appreciated. From a purely scientific standpoint, biology research stands to benefit enormously from the availability of the genome sequences of diverse organisms. The work of annotation and the task of establishing connections between genotype and phenotype will keep large numbers of scientists busy for many years to come. The most worrisome aspect of the genome sequencing program is that overriding commercial interests will soon ensure that the entire area of genomics and its applications will soon get mired in a sea of patent problems. The Americans have a characteristically pithy statement that summarizes the problem: 'There's no such thing as a free lunch'.

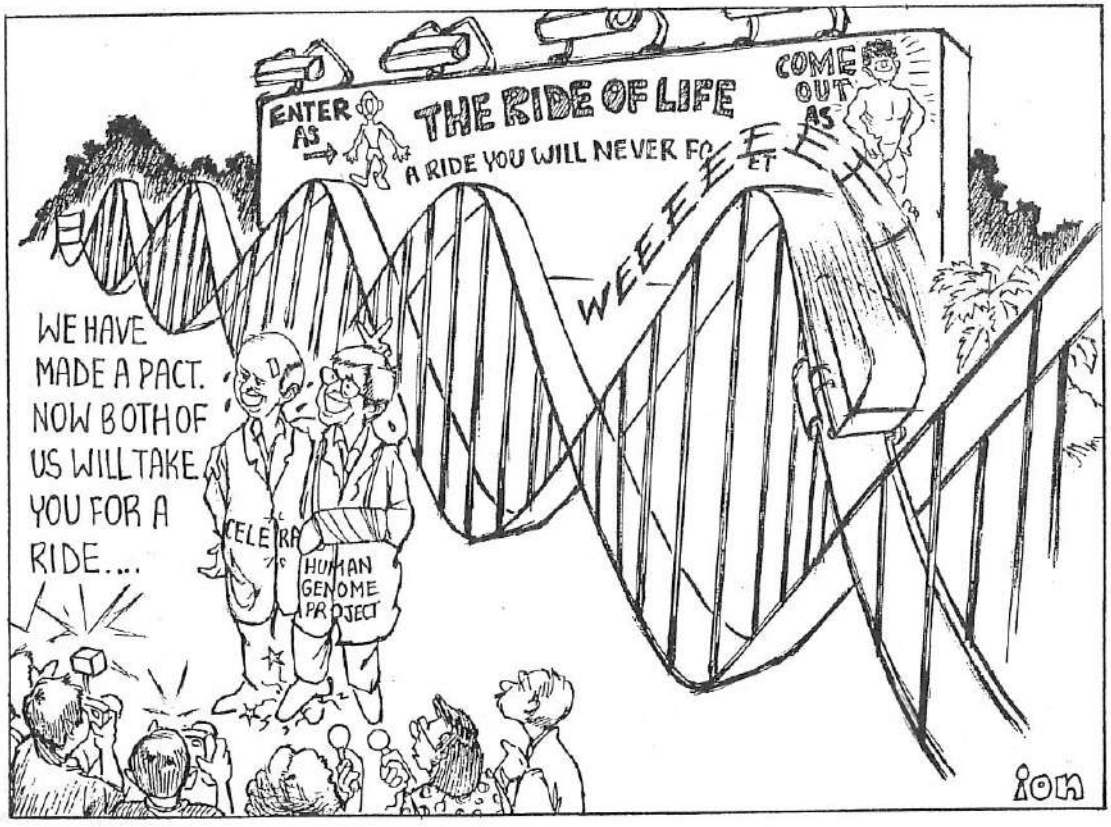
While our newspapers and TV programs have featured some of our more visible and voluble scientists talking about the broad implications of the human genome sequencing effort, few have wondered about the consequences for biological and biomedical research in this country. India has not been a partner in the international sequencing venture which has involved laboratories in France, Japan, Germany and China in addition to the major role of UK and US laboratories. How well organized are our laboratories to make use of the enormous

body of sequence data that is being generated, not only human but also pathogen genomes? Here, we usually take recourse to trumpeting the debatable claim that we have an enormous reservoir of trained computer whiz kids, who will see in genomes what no one else has seen before. The reality, of course, is that even the task of annotation is one that has been anticipated elsewhere, years ago when the sequencing programs were in their infancy. The result is that we are likely to be very far behind even in the task of building future research on the emerging genome data. A most distressing feature that has come to the fore is the facility with which we tout our large (almost impossibly so) population and ethnic diversity as a major resource in biological research in the area of genes and genomes. Even in scientific circles there is a worrying tendency to be self-congratulatory about our achievements in biological research (other areas have similar problems, although the magnitude of unsustainable claims appears to be correlated to the money spent). In order to compete effectively in the 'big science' of genomics we need to have a hard headed and unsentimental assessment of our strengths and weaknesses. The almost complete absence of high quality medical research, that uses the techniques of modern molecular biology is a matter of serious concern. In many areas we have been spectators to technological advances that have revolutionized whole areas of human activity. It is not often recognized that our repeated drum beating on information technology (IT) rests on the fragile fabric of a non-existent hardware base. What is true of IT today, will undoubtedly be true of biotechnology tomorrow.

But these depressing thoughts may be part of the 'morning after syndrome'. I should end these musings by recalling that even the big science of genomics had humble beginnings. The heart of the genome project is the procedure for reading the sequences of the letters of the DNA alphabet, the 'dideoxy procedure' developed by Frederick Sanger at Cambridge in the 1970s; later modified and automated to produce the machines that according to James Watson, could be 'run by monkeys'. Sanger himself, the recipient of two Nobel prizes (the first for sequencing proteins in 1958 and the second for sequencing nucleic acids in 1980), remained characteristically modest. In a marvellously understated autobiographical essay ('Sequences, sequences, and sequences', Sanger, F., *Annu. Rev. Biochem.*, 1988, **57**, 1) he says: 'Of the three main activities involved in scientific research, thinking, talking and doing, I much prefer the last and am probably best at it. I am all right at the thinking, but not much good at the talking. "Doing" for a scientist implies experiment.' One would hope that there are many unsung Sangers in our midst waiting to bloom as genomics flowers in the future.

P. Balaram

Ayan Guha



CURRENT SCIENCE

Volume 79 Number 2 25 July 2000

EDITORIAL

Sunsets and sunrises

Committee meetings can sometimes be educational. At one such meeting to discuss the inevitably mundane issues of academic life, I heard an intriguing phrase – 'the sunset clause'. The speaker extolled the virtues of sunsets and went on to argue that sunsets are as welcome as sunrises; the less harried amongst us still hopefully, have the time to watch the spectacle. He was, of course, metaphorically suggesting that research projects, institutions and organizations (and undoubtedly individuals) must prepare for dramatic changes, necessarily demanded by the imperatives of the moment. In simple terms, a sunset clause would translate to an enlightened 'exit policy', that would allow termination of a program or project, when there is a clear perception that the guillotine may bring about a quick, merciful conclusion. But, as we all know, the task of prematurely terminating any activity once started, is neither easy nor pleasant. Sentiment, accepted mores of functioning and the general feeling that a project once started must continue forever (at least until its protagonists tire), always stand in the way of precipitous decisions that favour quick closure. The larger the investment, the more difficult it is to decide when to stop. If a large sum of money has been spent on a project that appears to be going nowhere, few have the courage to suggest that it is time to cut losses and retreat. The normal tendency is to pump in more money, in the hope that this will resuscitate an already moribund project. This philosophy has been the guiding principle in determining the course that public sector organizations have taken over the years; it is also a style of operation that has found comfortable acceptance in other spheres, including the areas of higher education and scientific research. Should there not be a clearly defined 'sunset clause', with the precise time of sunset being clearly evident, well before mega projects reach an 'intensive care' stage? Many strategic research projects, shrouded in secrecy, survive long past the stage where euthanasia is warranted, both on technical and conceptual grounds. (The recent, publicized failure of the US antimissile project is reassuring; sunset clauses are apparently not easy to apply even in the country which, presumably, is the birthplace of the idea.) Others, in more visible areas are so large that indefinite continuance is guaranteed by their sheer size. Sometimes, the successful completion of a project promises so much, that even normally hard-headed critics are compelled to remain silent, for fear of being branded as obstacles in the path of progress.

But research projects are not the only area of concern to science, where the sun must occasionally set. What about institutions, programs and areas of activity? In Germany, the Max Planck Institutes operate under the remarkable (and apparently brutal) system, where the area of focus is determined by the research interests of the Director. When the head of an Institute is replaced, the entire focus of activity shifts; it is fairly common to change the field of research of an entire institute by this somewhat unique procedure of building organizations completely centred around a single individual. The Americans have a more subtle approach, where the 'tenure system' ensures an acceptable level of scientific accomplishment before confirmation of the state of permanence of a faculty member's position in a University. The 'sun sets' in the US most often on individuals and not on institutions. In India, however, matters are different. The sun rises on many new programs and institutions and almost never sets anywhere. Programs once created, rarely change direction; learning from experience is not common. Most institutions reach comfortable middle age and settle down to a long interminable twilight. Programs and individuals, to an alarmingly high degree, follow the same course. Rarely is a program terminated. New programs are generated without unduly disturbing existing activity; a consequence is that many institutions are overstaffed and overpopulated, bursting at the seams with a basket of committments that cannot be easily met. We have funding agencies which have taken on a frighteningly broad range of tasks, that were hardly envisaged in their original mandate; overseeing research institutions, distributing research funding, building common projects

with industry, promoting science education at all levels and developing international collaborations – multiple responsibilities that overload an already fragile administrative fabric.

Inside academic institutions new courses and research programs are often started in a sunrise phase, the first flush of enthusiasm reminding one of Wordsworth's memorable line – 'bliss was it in that dawn to be alive'; but few worry if the sunset clause must operate anywhere else in the organization. The laws of conservation, so beloved in the physical sciences, appear to be strangely missing in discussions on growth and development within scientific institutions. At an individual level, the absence of any operable 'sunset clause', other than retirement, has led to the increasing load of 'deadwood' in most laboratories and academic institutions. The public sector undertakings (PSUs), some years ago, spoke wistfully of a 'Voluntary Retirement Scheme (VRS)', a device which might have permitted both 'sunrise' and 'sunset' to occur synchronously. In practice, the VRS schemes only saw the more employable individuals (presumably the more competent) leave for the private sector, resulting in a further deterioration of the organization. The new attempts at disinvestment and privatization are an inevitable consequence of a system that never had any built in 'sunset clauses'. In research departments at academic institutions across the country, the process of decay is offset to a considerable extent by the steady influx and efflux of students, who work towards Ph D degrees. The 'sunset clause' is implicit in Ph D programs; although some students prolong their periods of stay, there is often a clearly spelt out upper limit. It is this group which, in a subtle manner, changes the direction of research in a laboratory, by dropping unproductive areas and following fertile trails. Unfortunately, much of this change of direction occurs in open-ended, low budget, basic research, which is generally of limited consequence. The process of change and reevaluation is almost missing at the higher levels of organization in our institutions. In the Universities, the sheer size of decision-making bodies and the compelling need for consensus on all issues, effectively ensures that change rarely happens. In national laboratories there are usually sufficiently large vested interests in maintaining the status quo. We therefore, seem to live in a twilight zone, where the sun rarely rises, but certainly never sets.

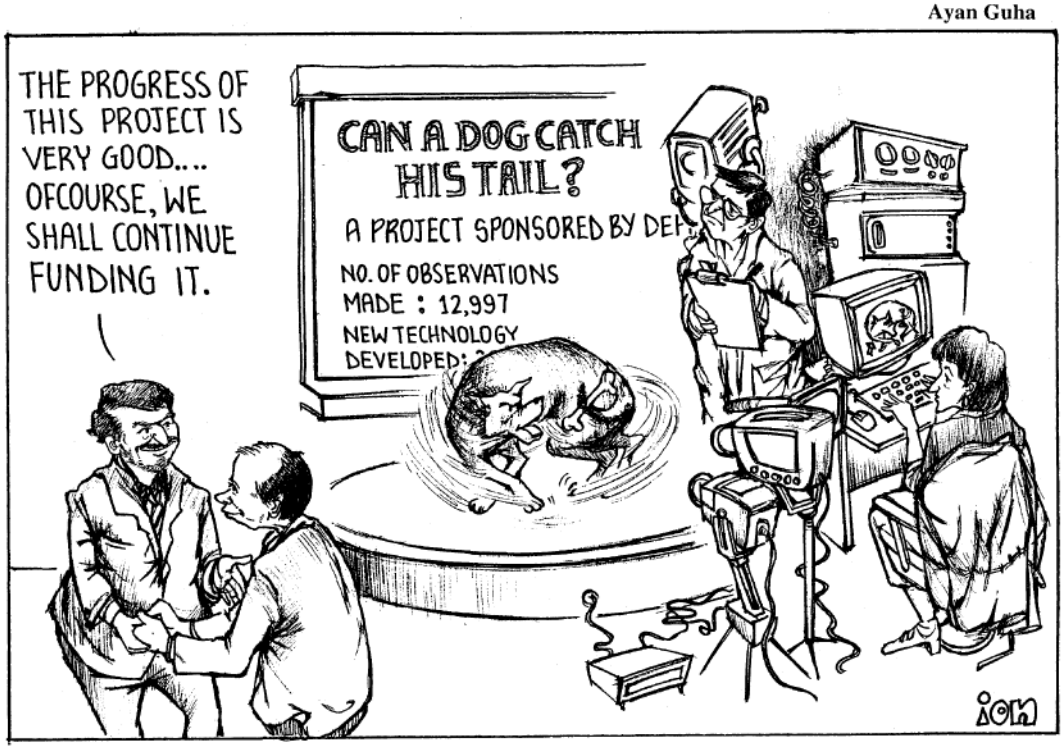
Sunrises and sunsets have been immortalized in verse; there is a certain sense of melancholy in contemplating the end of the day. Thomas Gray's sombre reflections in the *Elegy* may be worth recalling:

'The curfew tolls the knell of parting day,
The lowing herd winds slowly o'er the lea,
The ploughman homeward plods his weary way,
And leaves the world to darkness and to me'.

But, there is a case for sunsets if we want sunrises.

P. Balaram

Ayan Guha



CURRENT SCIENCE

Volume 79 Number 3 10 August 2000

EDITORIAL

Teaching research students

This is the season that a fresh academic year commences at universities across the country; indeed, in the period stretching from August to October a fresh cycle begins at academic institutions spread over the globe. There is the annual festival of student selection, a process marked by entrance tests, qualifying examinations and interminable interviews as sizable groups of students seek entry to Ph D programs, which will eventually certify their entry into a scientific career. Students working towards Ph D degrees (graduate students in American parlance) form the core of academic science. It is these students, in search of original and interesting results to report in their doctoral theses, who will eventually produce a substantial amount of the publishable work that finds its way into scientific journals. Ph D students often work long hours, supported by stipends that hover around subsistence level (despite periodic revision), in a scientific apprenticeship that can last for as many as five to six years (and in some unfortunate cases even longer). Several years ago many students entered research careers, driven by some intrinsic interest in the practice of science. Today, the pressure of increasing student numbers and the absence of suitable career opportunities result in a Ph D program being a logical extension of a Master's course; a device which permits a new lease of subsidized student life, postponing career decisions to a later date. For students with good academic records and superior scores in American qualifying examinations, like the GRE, the West beckons. For the others, the choices lie in the many institutions across the country that provide a haven for those in search of a Ph D degree. But, in the last few years there has been a growing feeling that the quality of students entering for research degrees is declining and that even the size of the pool from which Indian institutions make their selections is diminishing. In major disciplines of science like physics and chemistry the number of top quality students available for selection appears to be sharply declining. In mathematics (often touted as one of our strengths), the Ph D scholar is rapidly acquiring the status of an endangered species. Biology appears to be better served by numbers, driven by the relatively new fad of biotechnology; to many parents and students the suffix 'technology' seems to promise lucrative positions in industry. The decline in the input to Ph D programs is not an Indian phenomenon alone; in fact, the fall in interest in research degrees is even more marked in the West. However, Europe and America have the advantage of being able to fill their classrooms and laboratories by simply fishing elsewhere in the world; inevitably, Chinese, Indian and Korean students flood US graduate schools. It is a common sight today to find Western Universities ready for 'collaborative tie-ups' with counterparts worldwide, a thinly disguised hunt for quality scientific manpower.

But in a country with such large numbers of students in almost any discipline, should we really be complaining about limited numbers of 'good students'. Why are the vast majority of applicants found so unsuitable for a research degree? Presumably, the answer is that most students, barring a few from exceptional institutions, are poorly trained and woefully unprepared to face interview panels which often are completely out of touch with the ground realities of science teaching (or the lack of it) at most places. Our top institutions look for students, who appear to be instantly ready to plunge into research and have little time for those who may be motivated and capable, but lack the preparation to qualify. With very, very few exceptions, Indian institutions do not have an adequate pre-Ph D training program, which is an essential feature of the extremely robust American graduate school system. The importance of courses and qualifying examinations covering the broad discipline which the student has chosen, has never been recognized. The Ph D program is most generally viewed as an almost private contract sealed between the student and the research supervisor; departments and institutions often playing only a negligible academic role. In many universities 'registered guides' can enroll students under them with complete disregard for any academic standards. At present, the CSIR–UGC National Entrance Test stands as the sole line of defence; students who would like to be paid while they do research, must at least get a reasonably high rank in this examination.

Over the entire spectrum of academic institutions ranging from the modern research laboratories to the most ill-funded of universities there is a common feeling that research students are a commodity of some utility. In the former, students are the medium by which research is carried out, presumably to fulfill the laboratories mandates. In the latter, the number of students registered for a Ph D in itself, becomes an index of a supervisor's academic importance. Should not these institutions be paying more attention to the training of research students? Why is it that most academic departments in universities are bereft of courses and seminars for Ph D scholars? Why is it that in the highly specialized cocoons of the national laboratories, Ph D students are exposed to very little outside their immediate areas of interest? Clearly, this situation is a consequence of the inability of the university faculties to view the 'teaching' component within Ph D programs, with any degree of seriousness. The tendency to shirk work, so widespread in our midst, percolates to academia too. It may be time to begin thinking of common standards for regulating entry to Ph D programs, which must at least satisfy generally accepted minimum requirements. Enhancing the training component may allow poorly prepared but intrinsically capable students to break into a research career. Teaching students to do research and to enjoy science is often hard work; wonderfully rewarding when successful. Unfortunately, hard work is not in fashion.

P. Balaram

CURRENT SCIENCE

Volume 79 Number 4 25 August 2000

EDITORIAL

A foundation for science

Scientific research in its most fundamental form is practiced in Universities and academic institutions. At its best, research in academia is unfettered by the need to demonstrate instant utility, allowing scientists to follow their instincts in choosing problems for attack at the frontiers of their disciplines. At its worst, research in Universities is unpurposeful, undirected and largely driven by the need of students to acquire Ph D degrees, working ostensibly under 'guides' who show little interest in their progress. Academic science covers an extraordinarily wide spectrum of activity ranging from work at the cutting edge of contemporary research to purely routine, pedestrian activity, which nevertheless, serves the useful function of acting as the training ground for the scientists of tomorrow. Academic science forms the foundation of the scientific enterprise, indeed no 'useful, new science' will emerge without a wide base of basic science. Few will deny the importance of fostering the practice of science in academia. But, it appears that science departments in academic institutions across the country are facing a crisis, with both financial resources and quality manpower declining at a steady rate. Research, today, requires a substantial infrastructure, with most areas of science requiring access to sophisticated instrumentation, well endowed laboratories and significant budget for consumables. Most importantly, the pace of progress has been so fast that expensive instrumentation needs to be frequently upgraded. Maintenance costs are also mounting at an alarming rate. It is no longer easy or even possible to practice meaningful science on shoestring budgets.

Looking back at the structure for funding scientific activity in India, it comes as a surprise to many to realize that major initiatives for funding science at Universities are only a quarter of a century old. The Department of Science and Technology (DST) provided the first major thrust for funding individual investigator driven grants in the mid-1970s and raised the level of its activity very substantially in the 1980s. The Science and Engineering Research Council (SERC) of the DST served as the focus for supporting academic science. Much of the heightened activity at academic institutions (and national laboratories) across the country in the 1980s and 1990s was the consequence of the enthusiasm for science at the agencies and the infusion of funds by the DST, the Department of Biotechnology (DBT) and the Council of Scientific and Industrial Research (CSIR). But, in the last few years there has been a decisive change in the mood at the agencies. Building infrastructure for academic science and promoting individual driven projects are no longer a high priority. The torch has passed to those who champion the cause of 'applied science'; the projects that find favour today are usually large coordinated exercises, which invariably promise some remarkable outcome. Genomics, smart materials, vaccines and even a mission to the moon are talked about; it is fashionable to be mindlessly optimistic about the possibilities for success in technologically advanced areas. The commercial successes in the 'information technology' area appear to have convinced many that miracles are around the corner in the other fields of science and technology in India. The pessimists, largely silent, are being relegated to the fringe; not many policy makers today, like to be told that the fabric of fundamental science in India is dangerously stretched and needs attention.

Even at the best of our academic institutions, research facilities are meager as compared to Western laboratories, with which we often imagine that we compete. Even as a training ground for scientists, our Universities are poorly equipped in terms of equipment, facilities and people. These laboratories can hardly provide an adequate introduction to the many areas of advanced research, in which trained persons need to be generated for many other sectors of our science and technology establishment. Clearly recognizing the importance of shoring up academic science, countries like Canada, China, Japan and the UK have introduced new, innovative schemes for refurbishing and restaffing the institu-

tions charged with the mandate for developing basic science. In India however, no worthwhile initiative that will make a substantial difference is even being contemplated. In new programs for infrastructure building like FIST, DST's newest venture, the financial inputs are meagre enough to ensure that the effort will only be a token of our commitment to basic science. The major funding agencies have become enmeshed in an ever-widening circle of activities that have steadily diluted their commitment to funding 'small science'. Unfortunately, even 'small science' today, needs appreciable resource inputs.

Is there any hope of separating the needs of basic academic science from the requirements of clearly applicable, target driven science? One possibility is to resurrect an old idea that it may be worthwhile to establish a National Science Board (or Foundation) along the lines of the US National Science Foundation. Such a body would be clearly charged with the responsibility of fostering science in academia, without being shy of supporting apparently 'inapplicable research'. In principle, a foundation could operate by generating a corpus from both Government and private sources. An autonomous body, with full financial flexibility, operating outside a ministerial structure (and possibly away from Delhi, in a manner reminiscent of the Atomic Energy and Space Commissions) may provide a much needed transfusion of funds and enthusiasm to our academic institutions. There are also many lessons to be learnt from the growing influence of private foundations on the conduct of academic science in the West. For those who like to think in concrete terms, a reasonable difference may be made only if sums of about Rs 100–200 crores per year are additionally directed into the academic science sector. Only then will access to instrumentation and good laboratory facilities increase in a substantial manner across the country. Where will this money come from? Those who pore over the annual Central budget figures may soon realize that this may not be an impossibly large figure. Unfortunately, basic science does not have many lobbyists in the corridors of power. Private initiatives have also been completely lacking.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Watching science watchers

Can the output of science and scientists be quantitatively measured? Can open-ended scientific research be subjected to 'measurements' that provide reliable indices of the status of scientists and their institutions, including the journals in which the output of science is published? These must have been questions which preyed on the minds of many scientists a few decades ago. Today, the answer is clearly, yes; scientometrics is an established field, concerned with 'the study of the measurement of scientific and technological progress' (Brusilovsky, B. Y. A., *Technol. Forecast. Soc. Change*, 1978, **12**, 193–200; Garfield, E., *Curr. Contents*, 1979, **46**, 313–318). One of the major tools of scientometrics is the Science Citation Index (SCI), which was conceived by Eugene Garfield in the 1950s. Readers of the journal *Science* in the mid-1950s may have paid little attention to an article entitled 'Citation Indexes for Science: A New Dimension in Documentation through Association of Ideas' (Garfield, E., *Science*, 1955, **122**, 108–111). The paper begins with a quote, which is well worth reproducing: 'The uncritical citation of disputed data by a writer, whether it be deliberate or not, is a serious matter. Of course, knowingly propagandizing unsubstantiated claims is particularly abhorrent, but just as many naive students may be swayed by unfounded assertions presented by a writer who is unaware of the criticisms. Buried in scholarly journals, critical notes are increasingly likely to be overlooked with the passage of time, while the studies to which they pertain, having been reported more widely, are apt to be rediscovered' (Thomasson, P. and Stanley, J. C., *Science*, 1955, **121**, 610). Garfield then goes on to say: '. . . I propose a bibliographic system for science literature that can eliminate the uncritical citation of fraudulent, incomplete, or obsolete data by making it possible for the conscientious scholar to be aware of criticisms of earlier papers. It is too much to expect a research worker to spend an inordinate amount of time searching for the bibliographic descendants of antecedent papers. It would not be excessive to demand that the thorough scholar check all papers that have cited or criticized such papers, if they could be located quickly. The citation index makes this check practicable. Even if there were no other use for a citation index than that of minimizing the citation of poor data, the index would be well worth the effort required to compile it.' The importance of citation systems was in fact first recognized by lawyers, whose preparations for arguments in court were vastly aided by Shepard's *Citations*, a tool published since 1873, as pointed out by Garfield. In science, as in law, much is based on precedent. Garfield's case for a citation index was compelling and he concluded his 1955 essay with a prophetic comment: 'The new bibliographic tool, like others that already exist, is just a starting point in literature research. It will help in many ways, but one should not expect it to solve all our problems'.

In the early days, few could have anticipated the enormous impact of the SCI. The dramatic advances in data storage, transmission and retrieval have allowed Garfield's creation, the Institute for Scientific Information, to create quite literally a 'Web of Science' whose reach is truly worldwide. It is now possible (and sometimes unfortunately so) for almost anyone to do citation analysis; most often, for purposes quite unintended by the originators of the SCI. The citation index can provide remarkable insights into the lineage of scientific ideas; but science watchers must remember that scientometric analyses require a degree of scholarship that is sometimes no less than that required for the science that is being studied. In his columns in *Current Contents*, Garfield repeatedly demonstrated that highly cited scientists are indeed the pace setters in their chosen fields. His classification of scientists as 'Nobel class', was a provocative reminder to the scientific community that the Royal Swedish Academy cannot always honour all those whose work deserves to be celebrated. But, in looking back on this facet of citation counting, readers might go back to a list of 50 names published over a quarter of a century ago, representing the most cited authors during the period 1961–72 (Garfield, E., *Curr. Contents*, 1973, **40**, 487–488). Fourteen names on the list had already been recognized by Nobel awards; in the ensuing years half a dozen more have been honoured. Almost all others are pre-eminent in their fields, a clear indication that 'high end' citation frequencies are a reliable index of scientific achievement. Both critics and

promoters of citation counting in India often forget that analyses of 'low end' citation frequency data must be approached with care. The most vehement critics of scientometrics are often those who do not wish to learn lessons from hard facts, reminding us of the Bourbons who 'learnt nothing and forgot nothing'. Garfield clearly recognized that citation counting was but one parameter, in a complex equation that governs our judgements about science: 'The numbers used by researchers in the science of science include but are not limited to: the number of people receiving scientific degrees, the number of patents granted to scientists, the number of scientific articles published, the number of scientists who publish papers, the number of references appearing in papers, the number of citations to each paper, the amount of grant money awarded to scientists, and the amount of money budgeted by research agencies for scientific activities. The various numbers may be used in science policy or programme evaluation studies to measure the scientific "strength" of various countries, regions, or a particular university. One can follow the growth or decline of various fields or identify "where the action is". In short, scientometrics is concerned with the demographics of the worldwide scientific community'. (Garfield, E., *Curr. Contents*, 1979, **46**, 313–318).

In India, scientometrics has much to contribute in analysing our strengths and weaknesses. In a country with limited financial resources for science, policies may be better guided by scholarly analysis of scientific output, with the citation index providing only one of the tools of measurement. The strongest critics amongst our scientists of the discipline of scientometrics would have us believe that old-fashioned 'personal prejudice' is a better guide to assessing scientific activity. Unfortunately, scientometrics is itself a field that is in its infancy in India. Few science watchers in India bring rigour and scholarship to their studies. Most, use statistics, culled in an amateurish way from the SCI, to draw sweeping and often unwarranted conclusions. It is clear that 'science watching' is an important activity, but the 'watchers' must be trained and professional.

In highlighting scientometrics in a special section, this issue of *Current Science* focuses on an area outside traditional science, which will nevertheless dominate our perceptions about scientific activity in the years ahead.

P. Balaram

CURRENT SCIENCE

SUBMISSION IN ELECTRONIC FORM

Authors who have been informed of acceptance of their manuscripts may send the final version in electronic form on floppy diskette (3.5" preferred; IBM PC format only, *not* Mackintosh). The text of the manuscript only should be supplied as a plain ASCII file with no formatting other than line and paragraph breaks. (WordStar 5.5 or 7.0 and Microsoft Word for Windows 6.0 are acceptable, but ASCII is preferred.) A hard copy of the text, with all typesetting information (italics, bold, mathematical type, superscripts, subscripts, etc.) must accompany the electronic copy. Tables and figures must be supplied only as hard copy. The diskette must be labeled clearly with the following: manuscript number, file name, file information (ASCII or WordStar, version number, etc.).

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We expect that electronic submission will result in quicker processing for publication.

CURRENT SCIENCE

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EDITORIAL

Journals

This is an exciting and testing time for traditional journals. On the one hand there has been a remarkable increase in the number of new journals advertised by commercial publishers; on the other there has been the mounting pressure to go electronic, exploiting the new and powerful technologies of the information age. A few years ago the possibilities of electronically submitting a manuscript, which is then transmitted with almost no delay to referees and the wonderful feature of reading accepted papers that are yet to appear in print, were almost unheard of. Open archives of peer-reviewed, 'postprints', together with citation linking promise to revolutionize the manner in which scientists will navigate through the increasingly dense maze of scholarly literature (Harnad, S. and Carr, L., *Curr. Sci.*, 2000, **79**, 629). The preprint archive or the 'bulletin board', where authors 'electronically post' unpublished papers was restricted to the limited domain of particle physics, which indeed seeded the birth of the Internet. Today, preprint archives are being considered in many fields of science, in a move that is likely to redefine the role of reviewers and editors, as gatekeepers in the dissemination of scientific research results. Commercial publishing conglomerates have also grown larger, with a single company sometimes controlling many major journals in specific fields of science. In some areas, journal prices have spiralled out of control, forcing libraries to cut back on their holdings. The professional societies, particularly in the United States, produce many of the flagship journals of science, which have maintained their pre-eminent position over the last century. However, the ongoing electronic revolution has forced major changes even at venerable journals like the *Proceedings of the National Academy of Sciences, USA (PNAS), Journal of the American Chemical Society, Journal of Biological Chemistry and Physical Review.* In biology, more than in any other field with the possible exception of computer science, the explosion of research has led to a dramatic increase in the number of journals. There was a time when the *Annual Reviews* were the main source of scholarly overviews of important areas of biology. In the last few years, these simply produced (and affordable) volumes have been severely challenged by the appearance of many attractively formatted and colourfully illustrated review journals.

The *Trends and Current Opinion* series have begun to set new standards in providing quick overviews of rapidly changing research areas. *Nature* now has many offspring as the stable under the magazine's umbrella continues to grow; the most recent being a review journal. Printed journals have undergone a remarkable (and agreeable) transformation, with colourful covers and colour illustrations no longer being the sole prerogative of a few chosen journals. The constraints of limited library budgets and space, dictates that the competition for subscriptions, particularly print versions, is becoming brutal; the world of publishing follows decidedly Darwinian principles. In Europe, the coming together of professional societies of different countries, to publish common journals in physics, chemistry and biology (in English, of course), is indeed an historical transition. It is clear that integration of the European society journals offers an effective strategy for countering competition.

At this time of transition in the world of scientific publishing, the scene in India is surprisingly placid. There are occasional seminars and discussions, where the status of Indian scientific journals is considered and the importance of disseminating scientific information is emphasized. In these meetings, which are far removed from the realities of publishing in India, a few suggestions are casually dropped for improving both the content and accessibility of our journals. An oft-noted point is that less than a dozen Indian journals are listed in the portfolio of the Science Citation Index, presently the most widely used 'objective tool' for tracking science. The vast majority of the other journals remain invisible, contributing substantially to the 'Lost Science of the Third World'. The impact of even the 'top ten' Indian journals (*Current Science* included) is decidedly low;

ironically their 'impact' even in Indian scientific circles is modest. Why, then, are so many journals published? Why do journals in a discipline get subdivided into multiple sections, when it is difficult to even sustain journals with a broader scope? For example, in chemistry, a discipline in which India has had a reasonable presence, we have two sections of the *Indian Journal of Chemistry,* the *Journal of the Indian Chemical Society* and the *Proceedings of the Indian Academy of Sciences (Chemical Sciences)*. The profusion of academies and societies has also led to a multiplication of journals, many of which remain unseen and unread. How do journals survive if there are few readers and even fewer subscribers? Like many other activities in India, their existence, if not their regular appearance, is guaranteed by a government grant. At almost any forum where Indian journals are discussed, there is a groundswell of support for moves that seek to integrate journals, as a first step in launching a concerted program for improvement. Unsurprisingly, these ideas are usually discarded as unworkable in the bodies that run journals; the result is, of course, a comfortable status quo. The imperatives of the ongoing technological transition and the need to project a more coherent and positive picture of India's research output, must serve to catalyse fresh thinking on the future of our scientific journals. As in the case of the public sector undertakings, it is quite conceivable that the day is not far when government agencies may consider 'disinvesting' in many of our journals. The time may be ripe to consider strategies that will ensure a rising impact of at least some Indian journals, which in turn will ensure that science publishing in this country has a reasonably secure and successful future.

P. Balaram

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EDITORIAL

Where has chemistry gone?

A review panel recently suggested changes in the way that the National Research Council (NRC) the operating arm of the US National Academy of Sciences (NAS) functions (Lawler, A., *Science*, 2000, **289**, 1443). Since a major function of the NRC is to produce reports, most often for US federal government agencies, the review recommended an administrative reorganization of divisions. The new structure would be based on broad themes: 'education and social matters; physics, astronomy, engineering and energy; food and health; biology, earth sciences and environment policy; and transportations'. According to the news report, such a grouping 'will allow greater synergy among disciplines'. In reading this rather prosaic news item, I was suddenly struck by the fact that 'chemistry' as a discipline was conspicuously missing from the list of distinct 'themes' envisaged for the NRC. Physics, astronomy, engineering, biology and earth sciences find a specific mention; agriculture and medicine appear disguised as 'food and health'. Mathematics is absent and probably, rightly so. But where has chemistry gone? After all, the American Chemical Society is supposedly the professional body with the largest number of members in America. Is the absence of an explicit reference to the chemical sciences as a 'theme' for the NRC, a commentary on the present status of the discipline?

There was a time when 'chemistry' was a well-defined subject; its practitioners clearly marked the boundaries of their sub-disciplines. Organic chemistry confined itself to the study of compounds of carbon. The rest of the periodic table, with the exception of the few elements that bonded to carbon, were hardly of concern. But even here, the boundaries were sharply drawn; natural organic polymers, proteins, nucleic acids and polysaccharides were generally excluded from the ambit of 'organic chemistry'. In its early, developmental stages, life was indeed 'banished' from organic chemistry. Edvard Buchner's epoch-making discovery in 1896, of cell-free fermentation by yeast juices may have marked the beginning of biochemistry; but it drew from Adolf von Baeyer, his mentor and one of the pillars of German chemistry, the scathing comment: '. . . he (Buchner) has no real chemical talent'. The remarkable ability of academic organic chemists to construct high barriers around their discipline, well into the second half of the 20th century, prompted the biochemist Arthur Kornberg (noted for his seminal contributions to the emerging discipline of molecular biology) to note: 'The problem was that organic chemists placed arbitrary boundaries on their science. Although in their pursuit of natural products, they might still eagerly seek the challenge of an Amazonian butterfly pigment, they would not accept nucleic acids, proteins and enzymes as proper natural products.' (Kornberg, A., *Biochemistry*, 1987, **26**, 6888).

Inorganic and physical chemistry, the other main subdivisions of the science were also substantially insular, although the influence of physics was always strong on the latter. For a while, beginning in the 1920s, it appeared that the quantum revolution had breached chemistry's borders, but it was quickly adapted to create a new branch, theoretical chemistry. In recent times in many laboratories computers are more evident than shelves of chemicals. Analytical and polymer chemistry were always poor relatives, important in industry but low down in the academic hierarchy.

While in the public perception the first half of the 20th century belonged to physics, the second was undoubtedly dominated by biology. Chemistry which made steady progress throughout the last one hundred years has always been a Cinderella amongst the sciences, with no fairy godmother in sight. Chemical knowledge forms the basis for much of the improvements in our standard of living; pharmaceuticals, fertilizers, pesticides, polymers and refrigerants are among the many products of chemistry. Nevertheless, chemistry has been closely associated in the public mind, with many of the widely perceived ills of modern life – environmental pollution by chemicals being amongst the most widely discussed public concerns. Chemistry, thus, has an unfortunately poor, public image despite its widely acknowledged utility. Even in academic circles physicists and biologists appear to be asking fundamentally important questions which apparently pose greater intellectual challenges than the more mundane questions of chemistry. Who would not like to be told of quests that seek to probe the origins of the universe or the

origins of life? Chemistry, however, seems to address matters of detail, at a level of complexity that generally appears unappealing.

Chemistry is, undoubtedly, a central science. It borders biology on a very broad front, ranging from molecular biology to behavioural biology. Chemical ecology is rapidly developing as an important field, dependent exclusively on the most sensitive and sophisticated tools of analytical chemistry. The tools for probing the structure of matter, developed by physicists, most notably X-ray diffraction, nuclear magnetic resonance, mass spectrometry and a whole range of spectroscopic and microscopic techniques, have invariably been refined on the more tractable problems of chemistry, before their more glamorous applications in biology or medicine. That chemistry occupied a central position in the 19th century is borne out by Alfred Nobel's will, which ignores biology but awards prizes for physics, chemistry and physiology or medicine. But the first sign of the widening scope of the definition of chemistry was visible, when in a remarkable feat of alchemy, the Royal Swedish Academy of Sciences, transformed Ernest Rutherford into the laureate for chemistry. In more recent times, biologists and physicists have routinely been awarded prizes in chemistry. The frequency with which molecular and structural biologists have been recognized is a clear sign that perceptions about what constitutes chemistry are rapidly changing. Chemistry baiters might enjoy the uncharitable comments of John Maddox, the former editor of *Nature*: 'Chemists have done wonders in losing their identity in the rest of science. . . . the practice of what still passes for chemistry seems to have been largely preempted by outsiders – physicists, quantum theoreticists, computer mavens, statisticians, instrument designers, laser experts, genetic engineers, medical researchers, psychiatrists, astronomers, materials specialists and a host of other species. Truly, the science of chemistry has lost its identity'. (Maddox, J., quoted in, Seebach, D., *Angew. Chem. Int. Ed. Engl.*, 1990, **29**, 1321). But, chemistry has its defenders. In his perceptive essay written a decade ago, Seebach noted: '. . . that discrete boundaries no longer exist between the various natural sciences (mathematics, physics, chemistry, biology, medicine) and especially between related subdisciplies (in this case inorganic, biological, organic and physical chemistry). This has been the case for a long time in the world of applications, and it is just as true along the frontlines of research. Chemistry has not lost its identity: it has instead gained important footholds within the domains of other disciplines – albeit rarely at the initiative of chemists'. There is a distinctive side to chemistry which Seebach emphasizes in a quote attributed to Marcellin Bertholet (1860): 'chemistry creates its own object. This creative power, similar to that of the arts distinguishes it fundamentally from the natural and historical sciences'. In recent times, the flame of chemistry flickered briefly and brightly in the public eye, when the wonderfully symmetric C_{60} molecule, literally a 'celestial sphere', appeared to fall from the heavens.

In reflecting on chemistry, one cannot but note that in India also, the discipline faces a crisis of identity. Hemmed on its frontiers by biology and materials science, chemistry needs to become more broad based. Biology is a religion, that has always welcomed converts. Chemistry departments, on the contrary, have invariably been more puritanical in their outlook. But, in excluding chemistry as a 'theme' the NRC review is not alone. Close to home we have a major institution, the Jawaharlal Nehru University, Delhi which finds place for a spectrum of disciplines ranging from Political Science to Physical Sciences, but chemistry is well disguised, if not absent. In improving the image of chemistry and emphasizing its importance, the discipline needs a new generation of practitioners, who will redefine the borders of the science. The richness of chemistry clearly lies in its role as a central science. Traditional chemists must recognize that disciplinary boundaries are porous. Only then will public perceptions about chemistry improve. Maybe we will then rediscover the science.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Discrimination

Discrimination is a bad word. Discrimination is politically incorrect. Discrimination is a word that is invariably associated with prejudiced judgements based on caste, colour, religion or nationality. But, the dictionary tells us that 'discrimination' also means the process of 'distinguishing accurately'.

In everyday life, we constantly exercise our judgements and discriminate between good and bad; although in many situations decisions are rendered difficult when the choices are not clearly in black and white, but assume indeterminate shades of grey. How important is discrimination in the conduct of science and the management of institutions? The ability to discriminate between good science and bad, performers and non-performers is undoubtedly critical to the health of any organization.

In academic science, the American universities have evolved a system that has served them well. Tenure, or the promise of a permanent position, on a university's faculty is achieved only after demonstrated performance in research, teaching and, in many fields, the ability to generate resources. This process places the newest recruits, in the most prestigous places, under extraordinary pressure to perform at their very best in the first few years of their academic careers. The American system affords the facility of movement, both within academia and to government and industry, for those who do not achieve tenure at their initial places of appointment. The 'bar' on tenure decisions is raised as we move up the totem pole of academic institutions. The top ranked universities have the luxury of not only recruiting the most promising faculty, but also the even greater privilege of exercising tenure judgements, based not on promise but on performance. It is this ability to make judgements, the luxury of 'discrimination', that has provided the American academic system with the means of ensuring that high standards are maintained at the best of institutions.

The concept of 'tenure' is, of course, alien in our midst. The best of our institutions, the Tata Institute of Fundamental Research and the Indian Institute of Science among them, offer initial faculty appointments on 'contract'; but there are almost no instances of contracts not being renewed – the equivalent of not being granted tenure. In most institutions, the initial appointments are to 'permanent' positions (the mandatory 'probationary periods', merely an historical artifact), conjuring up visions of a comfortable sinecure that stretches all the way up to the age of retirement. Permanence is a prize attribute of jobs; the government bureaucracy and the public sector providing the gold standard of job security, irrespective of performance. But, academia is not far behind. In these days of privatization, more people are attracted to jobs in companies, which apparently lack the guaranteed permanence of government employment; but the uncertainties are offset by higher salaries, better perquisites and sometimes even greater job satisfaction. The problem of 'permanence' is that at all levels there are no mechanisms in place to ensure that performance is a condition for continuation.

Almost all our academic institutions, research laboratories and university research departments are populated by four categories of individuals. There are the scientists or faculty, administrative staff who man the offices, 'supporting staff', who range from the technically skilled to the completely unskilled and research students and associates. Of these, only the last group are truly 'temporary'; indeed, they are birds of passage, spending a few years working towards a research degree or gaining research experience. Much, if not all, of the research output can be traced directly to this constantly changing and renewable work force. Permanence confers on all the other categories, the privilege of never having to worry about job security. While, in principle, this is a very desirable state there are no mechanisms to goad the shirkers into working. Promotional avenues are limited and overwhelming importance is given to the length of service. Indeed, even when promotions are withheld, the action is hardly more than a minor rap on the knuckles; efflux of time will guarantee elevation to the next level

in the hierarchy. The 'Peter principle', presumably based on Western organizations, tells us that everyone rises to their level of incompetence; in our institutions sometimes, neither competence nor willingness to work is an important determinant in influencing the upward mobility of a career. Interestingly, some of our better endowed 'autonomous' institutions have moved to a system of contracting out routine tasks like cleaning and security to private agencies; a strategy which permits essential jobs to be carried out, without having to deal with a recalcitrant, 'permanent' work force. This device does not appear feasible at all levels of staff in scientific institutions. Faced with the unshakeable permanence of employees, institutions have assessments as the sole weapon in motivating their staff towards a higher level of performance. We can, of course, ignore as exceptions the 'creamy layer' of the academic community, who may be driven to work by internal desires. For a substantial number, institutional ambience determines the quantum of work output. Given the constraints of our institutional systems, one cannot but help wondering whether we should be more 'discriminating'. Can we not evolve a system, which at least makes an honest attempt to make distinctions between performers and non-performers? Is it necessary that egalitarianism should be carried to an extreme, within the confines of academia? Have we completely abdicated the exercise of judgement at all levels simply to avoid even the semblance of unpleasantness?

Any move towards changing the culture of performance evaluations would be most credible if it were to start at the top. Unfortunately, in India heads of institutions, agencies, departments and academics, who have reached a professorial rank are never assessed. This is, of course, true of other fields of activity also. Salaries are a great leveller; the hallmark of a government institution is that pay packets are completely unrelated to performance. There is, often, no correlation between the status of the position held and the quality of the work produced. While there appear to be some yardsticks by which academic performance may be judged, there seems to be no easy way of evaluating administrative competence. Retirements appear to be the sole mechanism for organizational renewal; with no facility for selectively retaining the efficient and productive workers.

This inability to introduce the fine filter of 'discrimination' at the workplace, may be contrasted with the high resolution discrimination on which our school, college and entrance examination systems are based. Here, thousands of students are evaluated in diverse subjects and fractional marks are used to 'discriminate'. However imperfect this may be, there are really no other viable mechanisms to cope with the numbers involved at this level. We have lived comfortably with this procedure, which sifts goods performers from poor performers and provides an absolute ranking system, despite all its pitfalls. Unfortunately, at the workplace we cannot have an impersonal evaluation system; subjective judgements must necessarily be exercised. If our public institutions are to move to a higher level of performance, we must improve our ability to discriminate.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Creationism, Astrology and Science

'In the Middle Ages and almost up to Darwin's time, the world was believed to be constant and of short duration. But the credibility of this Christian worldview had already been weakened in some quarters by a series of scientific developments. The first of these was the Copernican Revolution which had removed the earth and its human inhabitants from the center of the cosmos and in the process demonstrated that not every statement in the Bible had to be interpreted literally. Second, the researches of geologists had revealed the great age of the earth, and, the discovery of extinct fossil faunas had refuted the theory that the earth's biota was unchanged since the creation . . . . Despite this and much more evidence . . . the more or less biblical worldview still prevailed up to 1859. . . . It may seem strange to us today, but the concept of evolution was alien to the Western world.'

Ernst Mayr in *This is Biology*, Harvard University Press, 1997 (Universities Press, India, 1999, p. 175).

In the almost one and a half centuries that have passed since Charles Darwin penned *The Origin of Species*, the wheel has turned the full circle. Last summer, the Kansas State Board of Education decided to remove references to evolution and cosmology from its curriculum, touching of a fresh round of debate on what constitutes essential science, that must be communicated to students. Kansas, of course, is the quintessential mid-Western state in the United States; a bastion of conservative thought. Soon thereafter, the American Association for the Advancement of Science (AAAS) joined issue, publishing a strong statement urging restoration of the teaching of evolution and cosmology and committed itself to 'work aggressively to oppose measures that could adversely affect the teaching of science, wherever they may occur' (*Science*, 1999, **286**, 1297). The subsequent debate that has been sparked reflects the many tensions that exist between the findings of science and the beliefs of religion (*Science*, 1999, **286**, 681; 2000, **289**, 869).

Science, religion and superstition coexist in our midst. For non-scientists the choices are often clear. For scientists, particularly those familiar with evolutionary biology, faith in creationism appears completely irrational. Darwin's intellectual synthesis has passed many tests and 'common descent' removes humans from the pedestal they seek to occupy. Science appears relentless in its approach to explaining natural phenomena and brooks no interference from unknown and supernatural hands. A telling example is the story of 'vitalism', recounted in a recent essay (Gupta, S., *Nature*, 2000, **407**, 677). The popular belief, that organic substances of natural origin were imbued with a 'vital force', was effectively laid to rest by Friedrich Wohler's immortal synthesis of urea from ammonium cyanate. Wohler's dramatic announcement in a letter to his mentor Jons Jacob Berzelius marks a turning point in science: 'I must tell you that I can make urea without the use of kidneys, either man or dog'. But as Gupta points out in her perceptive essay, Berzelius 'vacillated between stances that are clearly supportive of a mystical vitalist force and others that are more accommodating of an atheistic materialism, which he generally abhorred. It appears that much of his energies as a chemist were engaged in the honest negotiation of a compromise between these two poles'. A century of scientific progress has, however, brought us to a point where one can legitimately ask, as Gupta does, 'whether life is greater than the sum of its parts'. The biological view of the human race has often been at odds with broader (and often, more imaginative) perspectives. A very recent commentary draws attention to Pierre Teilhard de Chardin and his book *The Phenomenon of Man*, which drew the ire of Peter Medawar, one of the most eminent and articulate biologists of the twentieth century. Teilhard, a French palaeontologist, was also a Jesuit priest and his views on the evolution of Man had the distinction of 'falling foul of both the Catholic church and the scientific orthodoxy' (Simmons, R. M., *Nature*, 2000, **407**, 839).

In the background of the ongoing debate on creationism in the West, we have the provocative report that the University Grants Commission (UGC) has decided to promote courses in astrology and palmistry. We would all like to know what the future has in store for us. Even the most rational amongst us cannot resist a peek at the 'Stars

this week' column in the Sunday papers, fondly hoping that some windfall might indeed come our way. But, astrology is not religion; it is a subject that comes cloaked in the garb of a pseudo-science, purporting to make definitive predictions on human affairs, based on planetary conjunctions. Astrology has no basis in planetary studies as practised by scientists. In a devastating critique, one of our most accomplished educationists and promoters of science, H. Narasimhaiah argues that astrology's 'superstructure is built on such wrong assumptions, that can never be considered as science by any stretch of imagination' (*Deccan Herald*, 21 October 2000). In a country where belief in 'auspicious times' is all pervasive, there may not be many takers for Narasimhaiah's viewpoint. However, one may still legitimately ask whether the UGC views astrology as a science and if it considers the teaching of astrology in Universities as an important determinant of this country's progress. Higher education in India is in a dismal state, with crumbling standards and decaying institutions across the country. For the UGC to even imagine that the introduction of astrology would be viewed as a 'new initiative', reflects very poorly on our decision-making bodies. In focusing on astrology at a time when higher education needs sustained and careful attention the UGC appears to have followed the example of the Emperor Nero.

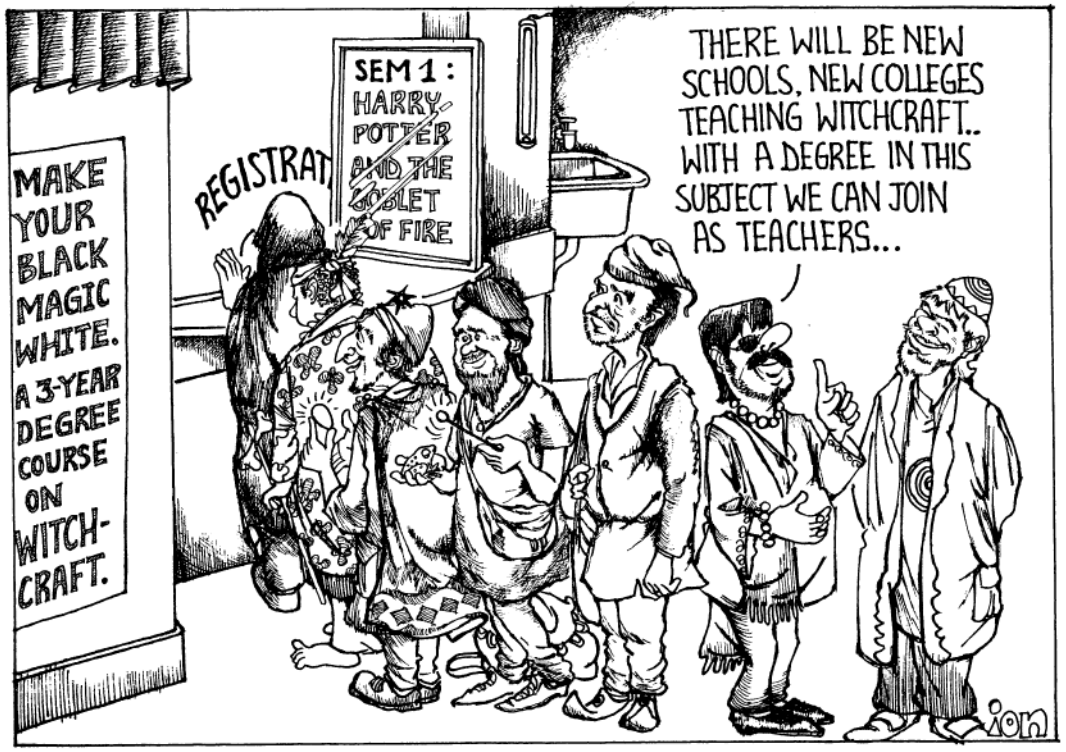
All of us undoubtedly, have an inalienable right to profess any faith and subscribe to any belief; but, superstition is best practised by individuals in private. The government must not provide a licence for the formal teaching of a subject, under the umbrella of our Universities, that only serves to mislead its believers. At times, when events appear to overtake us and we are swept along by circumstances outside our control, we would like to attribute our misfortunes to the planets. We might do well then to recall William Henley's exhortation in his poem *Invictus*:

'It matters not how strait the gate,
How charged with punishments the scroll,
I am the master of my fate,
The captain of my Soul'.

As in the fight against creationism, Indian scientific bodies have a responsibility to express a collective and reasoned view. The Academies and Associations must not adopt the expedient path.

P. Balaram

Ayan Guha



CURRENT SCIENCE

Volume 79 Number 10 25 November 2000

EDITORIAL

New institutions

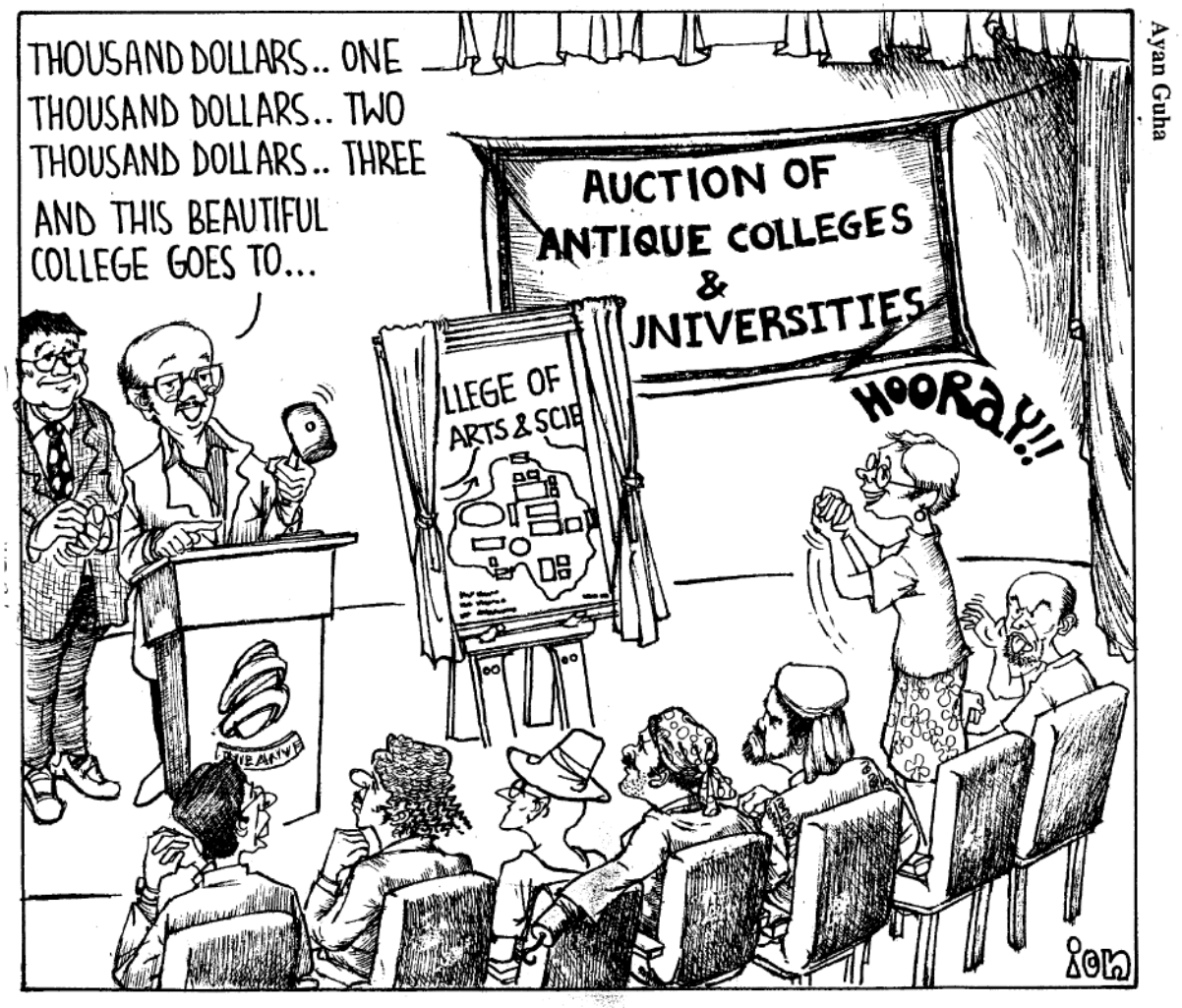
Scientific research and higher education in science are inextricably linked. There was a time when research in science was naturally practiced in Universities; a bygone age when the National laboratories and exclusive research institutions were yet to be conceived. The first signs that research could be fostered in an environment where formal classroom teaching was not an essential feature, came with the establishment of the Indian Association for the Cultivation of Science (IACS) in Calcutta by Mahendra Lal Sircar in 1878 and the founding of the Indian Institute of Science (IISc), a brainchild of Jamsetji Tata in 1909. Although during the second half of Raman's tenure at IACS (1907–1933), he also held the Palit Professorship at Calcutta University, the separation of teaching and research was already underway. The IISc was also conceived as a research institution although in his letters of 1896 Jamsetji Tata referred repeatedly to his scheme for a 'University'. Indeed, in its first half a century of existence IISc did not award its own degrees; the 'deemed University' status being given only in 1958. The Tata Institute of Fundamental Research (TIFR) conceived during the Second World War was born as a pure research institution, which even today relegates degree granting functions to associated Universities. At present, these institutions are largely training grounds for research students working towards Ph D degrees, with little involvement of the faculty in undergraduate science or engineering education. The national laboratories of the government agencies, including the laboratories of the Council of Scientific and Industrial Research (CSIR), are institutions dedicated to research with no real mandate to promote education in science. All these institutions seek to recruit Masters degree holders for Ph D programs and to technical positions. Over the last several years the quality of the input at these levels has been declining, a clear indicator of a looming crisis in science education.

The Indian Institutes of Technology (IITs) conceived in the 1950s and largely born in the 1960s were a remarkably successful experiment in higher education. Set up as 'institutes of national import n- joyed a special status which quickly propelled them to the first rank of Indian institutions. Designed to cater to the needs of undergraduate engineering education, the IITs did develop powerful science departments, active in both teaching and research. Modelled after institutes of technology in the West, the IITs attempted to combine first rate research with the demands of high quality undergraduate teaching. As they approach middle age the IITs are distinguished more by the quality of the graduates they produce, than the impact of the research that is accomplished. Juggling the demands of research and teaching, in an environment where the former is not easy to perform, has proved difficult. But, the alumni of the IITs have proved extraordinarily successful in their chosen fields, the world over, particularly in the area of information technology. Some of the most successful IIT alumni have now turned their attention to the cause of higher education in science and technology. A proposal to transfuse a large sum of money into the IITs, by a group of non-resident Indians, has been discussed in the press for some time. More recently a proposal to set up several new institutions, Global Institutes of Science and Technology (GIST), as a completely privately funded initiative, at an estimated cost of between US $300 million to 1 billion, has been reported. The purpose behind these initiatives appears to be the need to strengthen and replicate the IIT model, with a freshly defined emphasis on the coexistence of both high quality research and teaching in science and engineering. In reading about the GIST idea one cannot escape a feeling of *déjà vu.* These proposals bear a striking similarity to the scheme advanced for setting up a National Science University (NSU), which was widely discussed in the columns of this journal a few years ago (*Curr. Sci.*, 10 October 1994). The NSU was an idea projected by Swadesh Mahajan, a non-resident Indian physicist. Its *raison d'être* was, of course, the rapid erosion of the quality of science practiced in Universities. Mahajan's thesis was an unexceptionable one; high quality research within the

ambience of a teaching institution would be the best formula for attracting bright young minds to science. But the NSU proposal differed from GIST in an important way. In the former, the Government of India would have had to make a very significant financial outlay, amounting to about Rs 200 crores, to kickstart the enterprise. In the case of the latter, in keeping with the times, the institutions will presumably be completely funded by private sources. In the six years since the NSU proposal, the financial muscle of potential investors has immeasurably increased. If no governmental inputs are needed, the GIST proposal is less likely to get mired in political quicksands.

But, in considering all these new initiatives our existing system of Universities and colleges appears to have been pushed far from the public view. For almost fifty years, since the early days of the national laboratories, university science departments have fallen prey to the inexorable tide of decline. With the passage of time the pace of decay has quickened. The difficulties of recruiting good faculty, perilous financial positions, the unchecked growth of Universities and autonomous colleges providing advanced degrees and the failure of most university administrators to appreciate the special needs of science departments, have all contributed to a nationwide problem which appears to have no easy solution. A few Central Universities, particularly those of recent origin, are positioned better. There are no substantial initiatives, either governmental or private, which attempt to transform the older institutions. There is a tacit assumption that resurrecting science in the Universities is not a task that has any reasonable chance of success. It would appear, at least in the short run, a better option to start afresh, creating new institutions. The half life of decay of scientific institutions in India has not been very long in the last century, with only a handful struggling to maintain a steady course. It is to be hoped that the new institutions will have a brighter and stabler future.

P. Balaram



CURRENT SCIENCE

Volume 79 Number 11 10 December 2000

EDITORIAL

Systematic biology in the information age

The fusion of biology and information technology has spawned a new field, bioinformatics. All of a sudden, governments, venture capitalists and science adventurers see a fortune to be made, in a discipline whose origins are so recent, that even its definition is somewhat blurred. Bioinformatics grew in the shadow of the genomics revolution in the 1980s and 1990s, with the dramatic developments in the areas of information storage, retrieval and transfer providing spectacular support. The flowering of the Internet and the successful completion of major genome sequencing projects have happened at almost the same time, propelling bioinformatics to the forefront of scientific disciplines. There is presently, an enormous volume of nucleic acid sequence information pouring into computerized databases, with the raw information requiring considerable refinement and analysis before useful biological interpretations can be formulated. The term 'data mining', which is frequently used at meetings, refers to the fond hope that bioinformaticists, like miners of yore, will pull out nuggets of virtual gold from the enormous rubble of accumulated DNA sequence information. There are other areas of bioinformatics too. Crystallographers, and more recently NMR spectroscopists, have been depositing three-dimensional structural information on biological molecules, in the form of positional coordinates for individual atoms, into public domain data banks. While less extensive than sequence data, even the 3D-databases are growing quickly, facilitating statistical analysis and pattern recognition forays. The newly developing field of structural genomics soon promises to fill computer banks with structures of protein molecules of unknown function; truly turning biochemistry on its head. The remarkable speed with which DNA expression profiling has progressed, signals the arrival of another flood of biological information, this time on differential gene expression in diverse settings. In all these cases, the power of modern computers and the efficiency of global data transfer are central to further developments.

While molecular research has provided the rationale for the marriage of information technology and biology, there are clearly other areas of biological research, which will soon be swept into the digital age. A recent issue of *Science* (29 September 2000), highlights the potential importance of information technology for biodiversity research. Issuing a call for a 'global biodiversity map', Edward O. Wilson, the high priest of sociobiology, who coined the term 'biodiversity', argues that 'to describe and classify all of the surviving species of the world deserves to be one of the great scientific goals of the new century'. Wilson and others guess that the number of species of organisms that inhabit the earth may be of the order of 10 million; with about 1.5–1.8 million having been scientifically identified and named, in two centuries of 'the Linnaean enterprise' (Wilson, E. O., *Science*, 2000, **289**, 2279). Central to this goal of complete classification would be an army of expert taxonomists and systematic biologists, who have unfortunately been marginalized in biology departments across the world as molecular approaches grew from strength to strength. Wilson emphasizes this, when he argues that the 'key choke point . . . is not the tools of informatics, most of which are already at hand. Rather, it is the severely limited capacity of museums and other collections-oriented facilities to collect, prepare and analyse specimens, and the shortage of expert taxonomists to do the job'. In recognizing that taxonomists may indeed be an endangered species, the U.S. National Science Foundation has funded an initiative a few years ago, called the 'Partnerships to Enhancing Expertise in Taxonomy', whose purpose will eventually be to create a new biologist, 'comfortable with molecular and computer tools as well as with the microscope and collecting kit' (Pennisi, E., *Science*, 2000, **289**, 2306).

In India both biodiversity and bioinformatics are buzzwords; cash cows to be milked in the meeting rooms of funding agencies. Bioinformatics, is largely confined to setting up centralized sites for data dissemination, with little by way of original research ('mining'), at centres across the country. Indeed, most bioinformatics units seem content to be 'mine owners', still waiting for the 'miners' to descend into the pits to hunt for gold. Since very little by way of original data is received from local sources, these centres are repositories for international public domain databases. Biodiversity research is also hampered by the gradual disappearance of expert taxono-

mists. Conservation biology focuses on the few visible and publicly appreciated species, which provide a rallying point for activists; the millions of organisms that remain unseen and unidentified are of little concern. While our 'biodiversity resources' are trumpeted at every forum, the magnitude of the task of even finding out what we have is not easily appreciated. Bioinformatics provides powerful tools. Biodiversity research will allow us to realize the richness of our biological resources. But, at present, the prospect of moving purposefully towards the Wilsonian goal of a complete classification in India appears remote. Such projects require a level of organization, commitment and staying power that may be hard to come by in our midst. But, as a first measure we may indeed begin by seeding a 'taxonomic revival', restoring systematic biology to its proper place. Ensuring the integrated teaching of biology in a fashion that draws the connections between systematics and molecular approaches, emphasizing the tools of the information age, may be a good starting point. Finally, in considering the role of 'informatics' in biology, it may be prudent to remember the fine distinctions between information, knowledge and wisdom.

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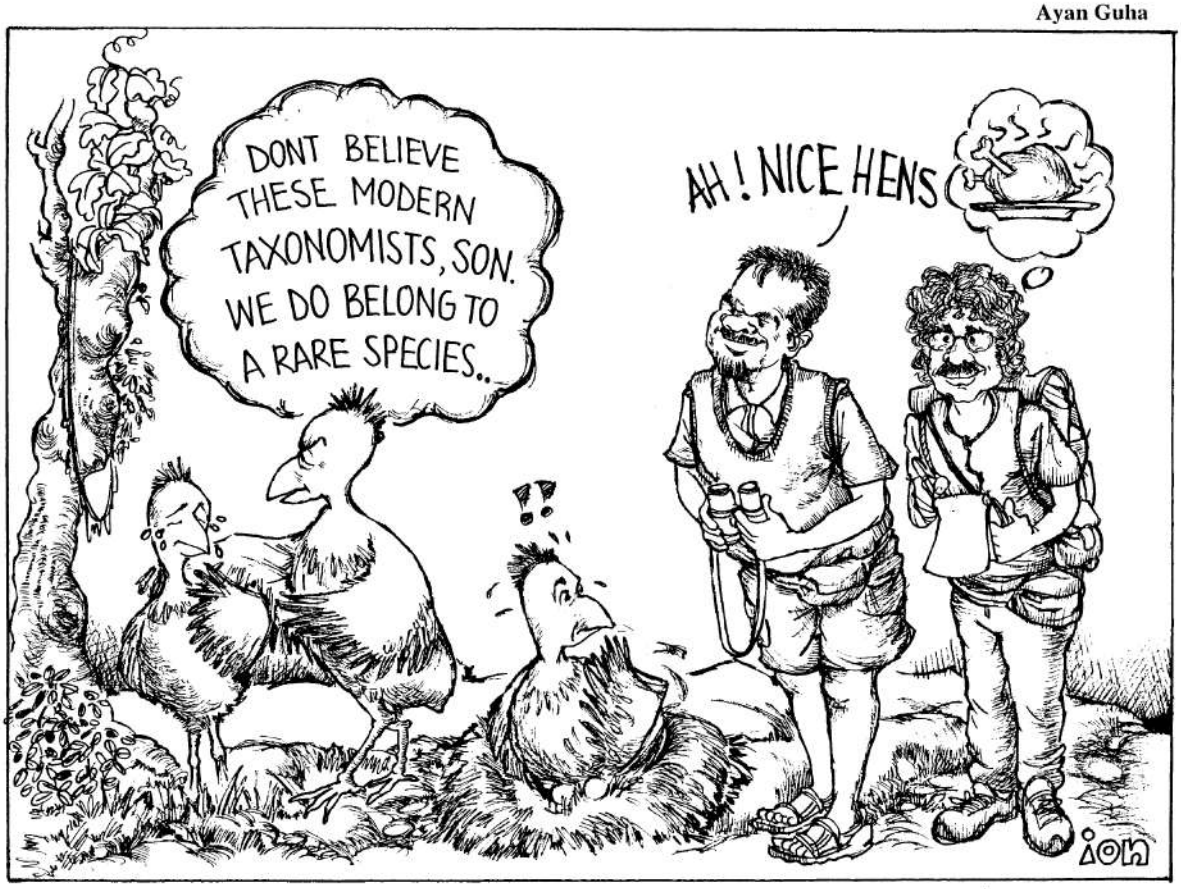
Ayan Guha



CURRENT SCIENCE

Volume 79 Number 12 25 December 2000

EDITORIAL

Reflections on an Apology

One of the indices of minor and inconsequential success, in local scientific circles, is the amount of time that a professor spends at airports across the country and, in the case of some, across the globe. There are always symposia to be attended, lectures to be delivered, examinations to be conducted and invariably, committee meetings to be attended, where advice is often proferred to a bureaucracy, which can hardly be derailed from rolling inexorably down a pre-determined path. But, there is a silver lining. Airport lounges provide a time to read and on occasion, airport bookshops can throw up a surprise offering. In aimlessly browsing at the well-stocked shop at Bangalore's otherwise forgettable airport, I came across a beautifully produced little book; a reissue of G. H. Hardy's *A Mathematician's Apology*, with a long preface by C. P. Snow (Cambridge University Press, 1940; Canto edition 1992, reprinted 2000). This was the kind of book that may have been fashionable to read, or at least be seen with a long time ago. But, modernity extracts an inevitable price. There may be few takers for an introspective view of the motivations for the practice of 'pure mathematics' and indeed creative science; and even fewer for Snow's perceptive and at times, strangely moving analysis of the life and times of Hardy in Edwardian England.

Hardy, of course, may be best known to readers as the discoverer of Srinivasa Ramanujan, in the almost impossibly romantic story of the transplantation of genius, from the inhospitable environs of the Madras Port Trust to the intellectual garden of Trinity College, Cambridge. This tale, chronicled with scholarship in Robert Kanigel's biography of Ramanujan (Scribners, 1991; Rupa & Co, 1992), is a tribute to Hardy's wonderful instincts as a mathematician and a human being.

In *Apology*, Hardy sets out to ask: 'Why is it really worthwhile to make a serious study of mathematics? What is the proper justification for a mathematician's life?' But, the aging Hardy wrestles with himself when he says: 'The function of a mathematician is to do something, to prove new theorems, to add to mathematics, and not to talk about what he or other mathematicians have done. ... there is no scorn more profound or on the whole more justifiable than that of the men who make for the men who explain. Exposition, criticism, appreciation is work for second rate minds'. While, the *Apology* may be viewed by many, and indeed it is, in parts, by Hardy himself, as a defence of the practice of pure mathematics, there is more. Graham Greene, arguably one of the finest novelists of the last century, ranks the *Apology* as the 'best account of what it is like to be a creative artist'. In his preface, C. P. Snow, a long time friend of Hardy and chronicler of the intellectual fare of Cambridge, passes judgement: 'There is something else, though, at which he (Hardy) was clearly superior to Einstein or Rutherford or any other great genius: and that is at turning any work of the intellect, major or minor or sheer play, into a work of art. It was that gift above all, I think which made him, almost without realizing it, purvey such intellectual delight.'

Six decades after it was written, Hardy's essay starkly illustrates the case for pure mathematics: 'A mathematician, like a painter or a poet is a maker of patterns. If his patterns are more permanent than theirs, it is because they are made with ideas'. For Hardy, 'Greek mathematics is 'permanent', more permanent even than Greek literature. Archimides will be remembered when Aeschylus is forgotten, because languages die and mathematical ideas do not'. This quest for immortality is a subtle undercurrent in Hardy's essay; an unstated fear that the worst that can befall men of genius is to be forgotten after their times. Hardy tells the story of Bertrand Russell recalling a horrible dream. 'He was in the top floor of the University Library, about A.D. 2100. A library assistant was going around the shelves carrying an enormous bucket, taking down book after book, glancing at them, restoring them to the shelves or dumping them into the bucket. At last he came to three large volumes, which Russell could recognize as the last surviving copy of *Principia mathematica*. He took down one of the volumes, turned over a few pages, seemed

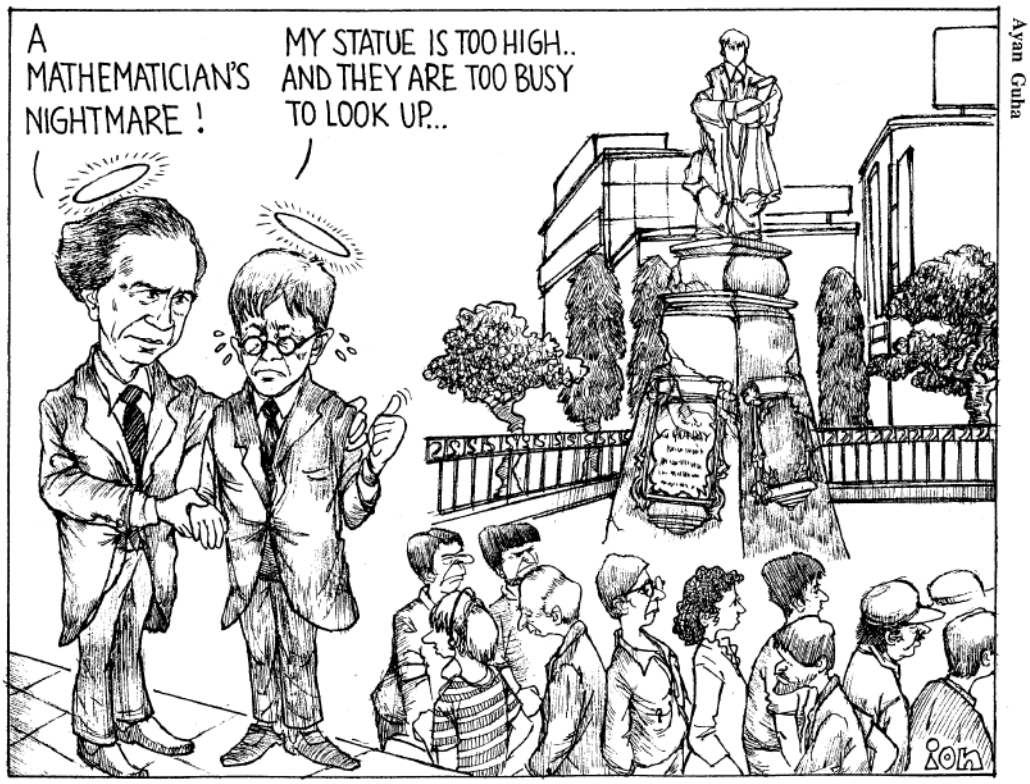
puzzled for a moment by the curious symbolism, closed the volume, balanced it in his hand and hesitated…'.

The *Apology* is a wonderfully melancholic essay. Hardy's assessment of himself should be required reading for all of us; it might help assess ourselves in the sheltered corners of our minds. 'It is plain now that my life, for what it is worth, is finished, and that nothing I can do can perceptibly increase or diminish its value. It is very difficult to be dispassionate, but I count it a "success"; I have had more reward and not less than was due to a man of my particular grade of ability…. I have always had plenty of leisure for the researches which have been the one great permanent happiness of my life…. It seems absurd to suppose that I could have "done better". I have no linguistic or artistic ability, and very little interest in experimental science. I might have been a tolerable philosopher, but not of a very original kind. I think I might have made a good lawyer; but journalism is the only profession outside academic life, in which I should have felt really confident of my chances…. The case for my life, then, or for that of any one else who has been a mathematician in the same sense in which I have been one, is this: that I have added something to knowledge, and helped others to add more; and that these somethings have a value which differs in degree only, and not in kind from that of the creations of the great mathematicians, or of any of the other artists, great or small, who have left some kind of memorial behind them.'

In reading a draft of Hardy's *Apology*, C. P. Snow made the point that, 'even if we grant that "Archimides will be remembered when Aeschylus is forgotten" is not mathematical fame a little too "anonymous" to be wholly satisfying'. Hardy has a ready counter, referring to a friend who asked Hardy, while passing the Nelson column in Trafalgar square: 'If I had a statue on a column in London, would I prefer the column to be so high that the statue was invisible, or low enough for the features to be recognizable? I would choose the first alternative, Dr Snow, presumably the second'.

Analysing creativity in science and defining the environmental factors that nurture creative science, has sometimes occupied sociologists. Hardy's essay might be a good starting point for those who attempt to define creativity. Snow's gentle and flowing prose recreates the Cambridge ambience that Hardy enjoyed; an ambience from which the legend of Ramanujan grew. But, we might remember, as Max Perutz remarked in the preface of his book (*I Wish I'd Made You Angry Earlier*, Oxford University Press, 1998): '… creativity in science, as in the arts cannot be organized. It arises spontaneously from individual talent. Well-run laboratories can foster it, but hierarchical organization, inflexible, bureaucratic rules, and mountains of futile paperwork can kill it. Discoveries cannot be planned; they pop up, like Puck, in unexpected corners.'

P. Balaram



CURRENT SCIENCE

Volume 80 Number 1 10 January 2001

EDITORIAL

Planning and serendipity in science

Planning for the future is a very respectable and essential activity. A recent visit to the Planning Commission, which is housed in a typically forbidding government building in Delhi, turned my thoughts to the role of planning in scientific activity. Meetings are often conducive to letting the mind wander, free and unfettered, sometimes to pose questions which have no simple answers. What has half a century of centralized planning achieved in a complex and populous country like India? How many of our past 'visions', documented in the turgid prose of government committees, have been realized? What indeed, is the prognosis for the many visions of the future that we are now documenting? Is 'centralized planning' a critical activity in all sectors of the liberalized economy? But, these are general questions, which presumably deserve attention in another forum. Of somewhat greater concern to an audience of scientists would be the issue of 'planning for science' and the virtues of 'planning in science'. As an aside, it is worth recalling that scientists of note were associated with the centralized planning process in the early years of the Planning Commission. While, P. C. Mahalanobis is the recognized architect of organized planning for development, another strong votary was Meghnad Saha. Indeed, the story of Saha persuading M. Visveswarayya not to accept the chairmanship of the original planning body, so that its power could be enhanced by having Jawaharlal Nehru at the helm, is recounted in the biography of C. V. Raman by G. Venkataraman (*Journey Into Light*, Indian Academy of Sciences, 1988, p. 517). But Venkataraman also quotes the feelings of Nehru himself, as the Planning Commission grew in size and bureaucratic complexity: 'When we first started it, I definitely thought it should not function as a part of the Government. But now it is just like any other part of Government. . . . the same hierarchy of Secretaries, Under-Secretaries and what not – it is frightening. What was thought of as a close body of people, who think and advise Government has now grown into a huge organization, with all the Departments of Government almost duplicated there and each one sending papers to the other, which is the normal habit of Government.' (*Journey Into Light*, 1988, pp. 463–464). Today the Planning Commission has grown into a monolithic organization, which determines budgetary allocations, at least in theory, to all areas of government. Both science and education are well represented on the Commission; indeed, the requirements of science departments are whetted here, in the complex and sometimes interminable process of decision making.

Planning for science is an activity that is important; usually dealing with the large-scale creation of institutional infrastructure for the practice of science and for the accomplishment of specific technological goals. Bombs, satellites, aircrafts, submarines and the networking of every corner of the country can be planned for. Clean water (or even more simply, water of any quality) and adequate power (nuclear and otherwise) appear harder to plan for. There are 'greyer areas' of desirable technological goals like drugs and vaccines against innumerable diseases, which are even harder to plan for, since the underlying science is still being understood. Then, of course, there are the diverse areas of basic science where 'planning' may not be an important or even desirable activity.

Can one really plan *in* science? Have not some of the most spectacular discoveries of the past appeared almost miraculously from nowhere? Has not apparently esoteric science, suddenly found dramatic application? Magnetic resonance imaging is a spectacular recent example. Is not the unexpected finding the most engaging feature of scientific research? Serendipity, a word associated with many scientific discoveries, is indeed defined as the 'faculty of making fortunate and unexpected discoveries by accident'. Royston Roberts (*Serendipity: Accidental Discoveries in Science*, John Wiley, New York, 1989) catalogs many of the 'happy accidents' in science, beginning with Archimedes. The term serendipity has an interesting origin; Horace Walpole coined it in 1754, influenced by the tale of 'The Three Princes of Serendip' who attracted 'happy accidents'. Serendip or Serendib was the anglicized version of Swarnadweepa, the old name for Sri Lanka. In examining many of science's most famous moments, Newton's discovery of gravitation, Pasteur's resolution of the chiral forms of tartaric acid, Kekule's visualization of the structure of benzene, Roentgen's discovery of X-rays and Fleming's remarkable observation of the activity of penicillin, Roberts traces the history

of serendipitous (or pseudo-serendipitous) discovery. But, he strongly emphasizes that many of the principal beneficiaries of serendipity 'clearly recognized' the difference between an accident and an accidental discovery. Pasteur put it succinctly: 'In the field of observation, chance favours only the prepared mind'. Joseph Henry echoed Pasteur when he said: 'The seeds of great discovery are constantly floating around us, but they only take root in minds well prepared to receive them.'

Planners of all kinds, those who plan *for* science and those who plan *in* science, might occasionally ponder on the many intangible factors that help science advance; sometimes in bursts and at other times as a steady march forward. It is hard to plan for an accidental windfall, but we might plan to be well prepared to quickly recognize and exploit new discoveries. But to return to the mundane; it might be wise for science planners to ensure that the ambience in our institutions favours the survival and proliferation of 'prepared minds'. Chance might then favour us with her bounty.

P. Balaram

Ayan Guha

CURRENT SCIENCE

Volume 80 Number 2 25 January 2001

EDITORIAL

Stem cell research: Biology's new frontier

'All cells come from cells'.
– *Rudolf Virchow*

The provocative title of a recently published paper, 'Turning Blood into Brain' (Mezey, E. *et al.*, *Science*, 2000, **290**, 1779) draws attention, forcefully, to biology's latest battleground – stem cell research. The same issue of the journal carries another report, more circumspectly titled, 'From Marrow to Brain (Brazelton, T. R. *et al.*, *Science*, 2000, **290**, 1775). What these reports, and many others appearing in the current scientific literature, promise is a 'brave new world' of biomedicine, where stem cells may provide the basis for treatment of many types of tissue and organ damage. The possibilities that appear on the anvil are the 'use of cardiac muscle cells for heart problems, liver cells for hepatitis, and neural cells for Parkinson's and Alzheimer's disease' (McLaren, A., *Science*, 2000, **288**, 1775). Cell biology has reached its present state after over one and a half centuries of progress, since Theodor Schwann first propounded the cell theory of biology in 1839. Schwann brought reductionism to biology; complex organisms could be viewed as a collection of cells, whose properties determine an organism's future. The years after Schwann were to take reductionism further; biochemistry, which reduces cells to a complex collection of interacting molecules was born at the end of the 19th century, midwifed into existence by Buchner's experiments on extracts of yeast. The tide of 'ultimate reductionism' that swept 20th century physics and chemistry, a belief that atoms, protons, electrons, quarks and a host of others (and even more recently, 'superstrings'), will provide a complete understanding of matter, presumably both inanimate and animate, left its imprint on biochemistry. The gulf between cell biology and biochemistry has really defined the difference between the living and the non-living. Single-cell organisms, bacteria preeminent among them, replicate with astounding fidelity and multiply rapidly; reproduction is indeed, the most remarkable attribute of life. Despite the power of modern chemistry and biochemistry, the ability to create 'synthetic life' is confined to the scientists featured in comic strips and science fiction.

But, cells and their aggregates can be manipulated. Over a century ago, Hans Driesch showed in a classic set of experiments that each cell of a two or four cell sea urchin embryo developed separately 'into perfectly formed embryos'. Clearly, individual embryonic cells possess a genetic program for development, which is an intrinsic property of the cell. Today's excitement about stem cell research and animal (and possibly, human) cloning experiments derives from the remarkable ability of some cells to multiply and differentiate under appropriate conditions. The pace of research in cell biology, in the three years since the arrival of the world's first 'cloned sheep', Dolly, has been dramatic. The production of ANDi (inserted DNA spelled backward; black humour evident even in the christening), the first genetically engineered rhesus monkey, marks the beginning of a new era, where mammals, even man, may be produced in genetically modified form. The arguments for creating genetically modified ('transgenic') monkeys appear impeccable; after all, primate models for many human diseases do not exist. The hope is that future medical advances will undoubtedly be made on the basis of research with genetically engineered monkeys.

But, we must return to stem cells. The cell biology texts list the 'defining properties' of these cells: (i) They are not 'terminally differentiated'; in simpler terms, the cell's ultimate fate has not been defined as yet, leaving open the possibility that stem cell differentiation can be controlled. (ii) Stem cells may divide without limit (an added caveat, in the 'Molecular Biology of the Cell' by Alberts *et al.* states 'or at least for the lifetime of the animal.'). (iii) When stem cells divide, the daughters appear to have a choice; they can either continue as stem cells, eternally youthful, untouched by the transition to adulthood. Alternatively, they can embark on a course that leads to terminal differentiation, sealing their fates for example, as liver, kidney or brain cells.

Nature appears to have endowed stem cells with many of the properties that we all seek, eternal youth and a whiff of immortality. Stem cells are found quite widely, particularly in tissues with a rapid turnover; the most widely known example being the bone marrow, from where the diverse cells of the blood are derived by differentiation. The recent papers of Mazey *et al.* and Brazelton *et al.* in *Science* demonstrate that adult bone marrow cells when transferred into mice, appear to find

their way to the brain (a not entirely surprising finding) and express the characteristic features of neurons (a remarkable discovery). Translating these findings into practical therapeutic advances will undoubtedly be a formidable task, but the prospects are immense.

This elementary and clearly oversimplified consideration of recent advances in cell biology would be out of place in an editorial column, but for one fact; embryos and fetuses are a rich source of undifferentiated cells, with the spectacular ability to differentiate and integrate into tissues. For human therapies, which may determine future treatment of diseases from Parkinson's and Alzheimer's to diabetes, we must, necessarily, turn to human stem cells. How much research can be carried out on human embryos? The questions regarding human stem cell research, using embryonic cells, are complicated by the fact that the technologies closely border those that may be used in human reproductive cloning. The ethical questions involved must be addressed, eventually, by governments, legislatures and regulatory bodies. Countries like Japan, Germany, Sweden, UK, Australia, the European Union and the United States have already embarked on a detailed consideration of the issues involved (*Science*, 2000, **290**, 1673). In all cases, legislative guidelines on the permissible limits of research in the area are likely to be spelled out. The House of Commons in Britain has just passed amendments to the Human Fertilization and Embryology Act, 1990, to allow the use of human embryos for research. The opponents of these changes argue that the British legislators have taken the first steps down the 'slippery slope towards human cloning'. (Dickson, D., *Nature*, 2001, **409**, 5). The Europeans and others appear to be more conservative, with the European Group on Ethics in Science and New Technologies suggesting, that 'at present the creation of embryos for somatic cell transfer would be premature' (*Nature*, 2000, **408**, 277). But, the proponents of stem cell research argue that distinctions must be made between (i) embryos donated for research by patients undergoing *in vitro* fertilization treatment ('spare embryos'); (2) embryos made for research by fertilizing donated oocytes *in vitro* ('research embryos') and (3) embryos made for research by transfer of somatic nuclei to donated oocytes ('nuclear transfer embryos') (McLaren, A., *Nature*, 2000, **408**, 573). At present the use of human 'research embryos' and 'nuclear transfer embryos' are considered ethically unacceptable, while work with 'spare embryos' appears to cause less discomfort. Biology's advances have posed new ethical dilemmas for which no easy answers appear to be forthcoming.

But, what of Indian research? There is little by way of organized stem cell research that is visible as yet. However, it would not come as a surprise if stem cell research is soon hailed as the new frontier in biotechnology and biomedical research, by our government agencies. When that happens we may have to address ethical issues with a degree of understanding and patience that is not easily found. Our legislators find it hard even to discuss the annual budget in a constructive fashion; complex ethical issues may scarcely merit any attention. Unfortunately, committees of scientists are often no better. The attitude that 'we know best', which has pervaded governmental decision making, almost exclusively dominated by directly interested departments, in areas as diverse as atomic energy and genetically modified crops, does not augur well for an informed debate on the complex issues, which must eventually be faced in the area of human stem cell research.

P. Balaram

CURRENT SCIENCE

Volume 80 Number 3 10 February 2001

EDITORIAL

Earth science, earthquakes and aftershocks

The earthquake that rocked Gujarat on 26 January 2001, was a sharp and apocalyptic reminder of the immense destructive power of natural disasters. Even as seismologists differed on the precise magnitude of the earthquake and rescue teams worked through the rubble to locate survivors and bodies, public discussion brought to the fore the inadequacy of our preparedness to face sudden emergencies and the ineffectiveness of our hazard-mitigation strategies. But, as public consciousness of the disaster slowly fades in the rest of India and the world, for those who experienced it in Gujarat the earthquake will remain one of the most frightening and awesome experiences of their lifetimes. The stability of the earth beneath us is something we take for granted. For the overwhelming majority of the residents on Earth, the planet's surface remains unchangingly placid, reassuringly stable and solid. Indeed, most of us will echo a view of the Earth: 'no vestige of a beginning, – no prospect of an end'. Only those scientists interested in probing the origins of the universe, the stars and planets, would have the courage and imagination to consider the birth of the Earth itself. Only the faithful would precisely mark the date on which the Earth was created; James Usher, a 17th century Irish prelate, did indeed estimate the date of the Genesis as 4004 BC, clearly in line with the Old Testament.

Modern earth science, or geology as it was originally christened, has its origins about a century after Usher, when James Hutton thought up the interesting idea of the 'Earth as a giant machine that had run and would continue to run'. Hutton realized that the erosion of continents, uplift of compacted sedimentary layers in the ocean and igneous activity constituted a geological cycle that was based on dynamic renewal. Jules Verne's 19th century novel, *Journey to the Centre of the Earth* was one of the rare attempts in fiction to imagine the core of the planet. The 20th century has been the setting for two major revolutions in the earth sciences, which have led to 'mobilistic views of solid earth'. Alfred Wegener, a German meteorologist and an outsider to the geological establishment, proposed in a lecture in Frankfurt in 1912, 'that continents had actually moved thousands of miles apart during geological time'. Wegener's idea of 'continental drift' was 'based on palaeontological and geological correspondence' between the continents separated by the Atlantic Ocean. Wegener's idea was a firm and more scientific restatement of Roger Bacon's 17th century view, that the similarity in shape of the Atlantic coasts of Africa and South America constitute 'conformable instances which are not to be neglected'. Wegener's concept of a supercontinent, Pangea, as the precursor of our present day continental distribution was to change the face of the earth sciences forever. His ideas summarized in a book in 1915 were to remain largely unaccepted in his lifetime. Wegener disappeared on 1 November 1930, on his fiftieth birthday, while on an expedition to Greenland. In a 1928 conference, a group of eminent geologists were split down the middle, with half of those present opposing the idea of continental drift. Support for drifting continents was to come only two decades after Wegener's death, when the science of palaeomagnetism was born [*Dictionary of Scientific Biography* (ed. Gillispie, C. G.), Charles Scribner's Sons, New York, 1976, vol. XIV, pp. 214 217]. The second revolution emerged in the 1960s, when the concepts of plate tectonics took root; a modestly titled paper, 'A new class of faults and their bearing on continental drifts' (Wilson, J. T., *Nature*, 1965, **207**, 343), was to signal a paradigm shift. The boundaries between 'plates' are relatively narrow and demarcate the areas of greatest seismic risk, the Rann of Kachchh amongst them. But, unlike other natural disasters like cyclones or even volcanoes, even approximate prediction of earthquakes appears to be a formidable or nearly impossible task (*Current Science*, Special Section on Seismology, 10 November 2000). Diverse warning signs have been probed as potential indicators of an earthquake. Seismic precursors, velocity of seismic waves in fault vicinities, tilt and strain precursors, electromagnetic disturbances, hydrologic phenomena like sudden changes in water tables, chemical monitors like radon emissions and even animal behaviour, in the hope that they respond to 'inaudible acoustic signals' have been, hopefully, investigated. But, the view of the professionals would be that plate tectonics only provides a 'broad view for assessing seismic hazard'. This conclusion necessarily suggests, that at best, we can hope for a 'probabilistic approach to hazard man
knowledge of the statistical risks 'can provide a rational basis for building codes, zoning regulations and the provision of back up systems' for coping with the aftermath of an earthquake (Turcotte, D. L., *Annu. Rev. Earth Planet. Sci.*, 1991, **19**, 263).

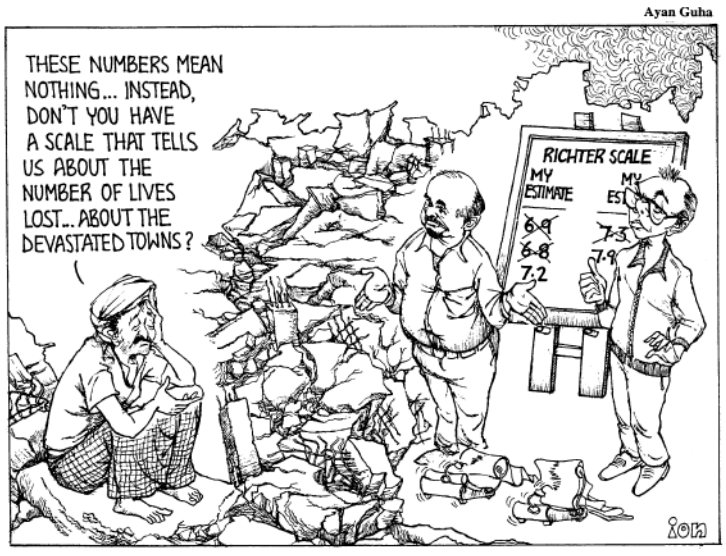
The Gujarat disaster is the latest reminder that India is poorly equipped to implement any scientifically sound

strategy for hazard assessment and disaster management. The Bhopal gas tragedy, the Surat 'plague' and the periodic cyclones that ravage the eastern seaboard have passed into history. At its very best civic administration in our midst is indifferent. At its worst local administration can be a barrier to well-meaning efforts at relief. The concept of 'building codes and zoning regulations' is completely alien; there is little doubt that civil engineering of 'earthquake-resistant' buildings will remain wishful thinking in reports generated by newly constituted committees for hazard management. It is also doubtful if mitigation strategies favoured in the richer countries are applicable in a populous and poorly developed country. Inevitably, like the annual cycle of droughts and floods, earthquakes too recede from the public eye, with the passage of time.

Tragic as they are, earthquakes provide earth scientists with a wealth of data, which will, hopefully, enhance our understanding of the underlying phenomena, that culminate in those few, terrible and catastrophic, moments. As in many other fields, understanding the cause may hardly provide any clues towards prevention; Nature's forces do not easily brook control. However, many curious features that intrigue geophysicists can emerge in the aftermath of earthquakes. The 1994 earthquake in California provided the unusual observation of 'excessive damage' in Santa Monica, located 21 km from the epicentre; raising the possibility that anomalous damage may result from geologic focussing of seismic waves (Davis, P. M. *et al.*, *Science*, 2000, **289**, 1746). How well positioned are earth scientists in India? Geology has been a traditionally classical discipline in the universities; its teaching like many other sciences, descriptive and dull. The CSIR manages a major institution in the area, the National Geophysical Research Institute at Hyderabad. Ocean research is geographically separated, the National Institute of Oceanography being located in Goa. The Geological Society of India and the Geological Survey of India are organizations with a substantial history. But, modern earth science is a melting pot of disciplines; our own institutions are notably insular. At the level of basic research the earth sciences maintain a low profile. In the hierarchy of desirable professions, science itself ranks low; the earth sciences (and this may be a prejudiced view) appear to be even less sought after than sister disciplines. A somewhat forlorn weekly newspaper column, entitled 'Career Quest', highlights career options in various fields. Coincidentally, attention was focussed on 'studying the forces that shape the earth' (Geophysics) in the 30 January edition of *The Hindu*. In the almost obligatory nod towards the computer sciences, the article suggested that 'geologists are refocussing their skills and drawing heavily from satellite and computer technology'. Major institutions like the Indian Institute of Science and the Tata Institute of Fundamental Research do not have focussed groups in the area of earth sciences; in an ironic twist the former has a Centre for Atmospheric and Oceanic Sciences, which appears, at least in a titular sense, to exclude the land mass.

Japan is a country which has spent much effort in coping with earthquakes. In browsing through the library, I came across the reflections of Japanese earth scientist, tempered undoubtedly by personal experience (Wadati, K., *Annu. Rev. Earth Planet. Sci.*, 1989, **17**, 1): 'The environment surrounding us is of both natural and artificial origin, and these are intertwined in a complicated way. Recently the artificial side has become predominant, particularly in densely populated districts. It is not an exaggeration that the Earth is changing its original state and nature will be destroyed if things go as they are going now. Actually, it may be impossible to stop this present trend of gradual worsening of environmental conditions. But we must at least endeavour to create a new ideal Earth, searching for the best way to human happiness, today and in the future. In the quest for this goal, research in geophysics will naturally play a part, and its advance is strongly anticipated. Lastly, I think we should always be modest while carrying out this serious task, as our knowledge about the Earth is not yet enough.' Earthquakes that come upon us without warning emphasize the limits of our ignorance.

P. Balaram



CURRENT SCIENCE

Volume 80 Number 4 25 February 2001

EDITORIAL

Publishing the genome

The human genome holds an extraordinary trove of information about human development, physiology, medicine and evolution.

– International Human Genome Sequencing Consortium

Decoding of the DNA that constitutes the human genome has been widely anticipated for the contribution it will make towards understanding human evolution, the causation of disease, and the interplay between the environment and heredity in defining the human condition.

– J. C. Venter *et al.*

The completion of the 'rough draft' of the human genome sequence has been announced again. The first time this happened was when the commanding generals of the sequencing armies, of the publicly-funded International Human Genome Sequencing Consortium and the private company Celera Genomics, Francis Collins and Craig Venter, appeared at a joint news conference last summer; an event that appeared to have been brokered by the then American President, Bill Clinton. The popular press went wild in the days that followed; some reports extravagantly concluding that as a consequence of genome sequencing all of us would lead disease-free lives extending to 150 years. Now the two genome groups have done it again, this time by publishing their results in scientific journals. But, getting the articles into print simultaneously, in a manner that the credit is equally shared between the two groups, appears to have involved considerable backroom effort. The final outcome is the appearance of two mammoth papers in the 15 February issue of *Nature* (International Human Genome Consortium, *Nature*, 2001, **409**, 860–921) and the 16 February issue of *Science* (J. C. Venter *et al.*, *Science*, 2001, **291**, 1304–1351). While the raw sequence data of the publicly-funded consortium have been flowing into the GenBank database, providing unrestricted access, the sequence data generated by Celera Genomics are available to academic scientists only in a restricted manner. The data-sharing debate has been contentious in the past and is likely to be even more rancourous in the future; ironically the remarkably rapid progress of the Celera group was in considerable measure aided by the large volume of data deposited in public databases. The editors of *Science* have presumably agreed to the access limitations insisted upon by the Celera group, so that the journal does publish the first major results of biology's most visible project. The honours seem to be equally divided between the two competing groups and journals; future historians may indeed reveal some fascinating insights into the processes by which the authors and editors reached a conclusion that would have done King Solomon proud.

But, what of the genome? The two groups have indeed assembled a draft sequence that covers over 90% of the genome; the International Consortium reports about 2.69 billion bases (to most of us, an incomprehensible string of the letters A, T, G and C), while the Celera group registered 2.91 billion bases. There is, quite simply, too much information to be digested easily. In the two major papers and several ancillary analyses that appear in the issues of *Science* and *Nature* there are many observations highlighted that will provide the impetus for new lines of biological research in the coming years. The most dramatic outcome of the first pass at 'annotating' (the term preferred by bioinformatics researchers for deciphering the encoded information) the genome, has been the significantly smaller number of genes that have now been found. In the early days of the sequencing program, gene number estimates fluctuated widely, most estimates varying from 60,000 to 75,000 genes, with a one-time peak of 120,000 genes. But, now that the dust is settling, the numbers are dramatically smaller. The Celera group has found '26,588 protein-encoding transcripts for which there was strong corraborating evidence and an additional ~12,000 computationally-derived genes with mouse matches or other weak supporting evidence'. The International Consortium concludes that 'there appear to be about 30,000–40,000 protein-coding genes in the human genome – only about twice as many as in worm or fly'. In the first phase of reactions that have appeared in the press, there appears to be some measure of discomfort in the fact that humans do not appear to possess a dramatically larger complement of genes than the fruitfly, *Drosophila melanogaster* (13,600), the worm, *Caenorhabditis elegans* (18,500) and the tiny plant *Arabidopsis thaliana* (25,000). Curiously, classical

biology has always lived quite comfortably with the 'C-value paradox', where there is no simple correlation between genome size and apparent organismal complexity. We now appear to be moving towards a refinement of the problem, as the number of protein coding genes does not appear to reconcile easily with our notions of complexity. Indeed, Venter and his colleagues draw attention to J. B. S. Haldane's 1937 speculation 'that a population of organisms might have to pay a price for the number of genes it can possibly carry'. In Haldane's view, large gene complements can reach a stage where 'each zygote carries so many new deleterious mutations that the population simply cannot maintain itself'. Remarkably, early estimates of gene loci in humans by Hermann Muller (1967) and J. F. Crow and M. Kimura (1970), hovered around a figure of 30,000 genes.

As long anticipated, the human genome is an enormously barren stretch of DNA bases that do not code for anything; a veritable 'desert', rarely punctuated by a gene oasis. The Y-chromosome, which is a male attribute, is particularly barren, containing relatively few, albeit important, genes. However, the size of the 'human proteome', the total complement of proteins, quite literally the workhorses of cellular biochemistry, maybe significantly higher; current estimates climb into the neighbourhood of 60,000. This would mean, of course, that the final translation of the 'message', encoded in human genes and genomes, would require considerably greater use, than hitherto anticipated, of 'alternative splicing mechanisms'; a process in which the final messages are constructed by piecing together coding regions in different ways. This molecular sleight of hand, would allow considerably more gene products (proteins) than predicted from the number of identified genes. A considerable part of the difficulty in decoding the information implicit in the human genome is the presence of tracts of non-coding DNA of widely varying length (introns), which separate segments of coding sequences (exons). The human genome sequence reveals large stretches of 'parasitic DNA', which appear to have come about by 'reverse transcription from RNA. In places the genome looks like a sea of reverse-transcribed DNA with a small admixture of genes'. (Baltimore, D., *Nature*, 2001, **409**, 814). These stretches of DNA might provide a link to our evolutionary past, containing an imprint of mankind's continuing interaction with viruses. Surprisingly, the genome sequence also suggests that as many as a couple of hundred human genes may be a result of direct transfer from bacteria, as distinct from evolution from bacterial genes. Clearly, we are all genetically modified ('engineered') organisms, natural transgenics created over eons of evolution. A Nature News Service summary alludes to the ongoing debate on genetically modified (GM) organisms: '... it is perhaps ironic that all humans, including those in the anti-GM lobby are GM organisms'.

In thinking about the impact of the unveiling of the genome, it is useful to consider David Baltimore's assessment: 'I've seen a lot of exciting biology emerge over the past 40 years. But chills still ran down my spine when I first read the paper that describes the outlines of our genome.... Not that many questions are definitively answered. For conceptual impact, it does not hold a candle to Watson and Crick's 1953 paper describing the structure of DNA. Nonetheless, it is a seminal paper launching the era of post-genomic science'. How do the authors of these, massive and at times forbiddingly complex, papers view the future? For Venter *et al.* the issues are clear. Low gene numbers limit the extent of complexity that can be explained purely on the basis of simple interactions between protein components, a cornerstone of reductionist biochemistry. The newer descriptions of 'robust networks', with which the rapidly growing literature of complexity theory abounds, may be called into play in biology, to rationalize the sometimes seemingly incomprehensible relationship between genotype and phenotype. For Venter *et al.* 'the real challenge of human biology, beyond the task of finding out how genes orchestrate the construction and maintenance of the miraculous mechanism of our bodies, will lie ahead as we seek to explain how our minds have come to organize thoughts sufficiently well to investigate our own existence'. The paper by the International Consortium ends (borrowing a memorable phrase from the classic paper of Watson and Crick): 'Finally, it has not escaped our notice that the more we learn about the human genome, the more there is to explore'. For these authors the journey has been long and T. S. Eliot provides inspiration: 'We shall not cease from exploration. And the end of all our exploring will be to arrive where we started, and know the place for the first time'.

The publication of the first papers to emerge from the human genome sequencing project marks a major milestone in biology. The papers represent a technological and organizational *tour de force*, which sets new standards for biological research. The Celera achievement of reaching a preset goal in less than a year is truly remarkable; competition undoubtedly catalysing the final sprint to the finish line. The first year of the 20th century saw the rediscovery of Mendel's Laws of Heredity and Max Planck's formulation of the quantum hypothesis. The opening weeks of the new millennium clearly belong to biology. What lies ahead is hardly predictable, but one certainty has emerged; the practice of biology will never be the same again.

P. Balaram

CURRENT SCIENCE

Volume 80 Number 5 10 March 2001

EDITORIAL

A developing crisis in higher education

Within the sheltered environments of research institutions, national laboratories and the sprawling organization of the strategic R&D establishment, discussions on the future course of science in India are inextricably linked to budgets. The scramble for funds is inevitably tied to the mounting costs of scientific research and the common perception that large grants must signal scientific success. There are occasional discussions on the declining quality of training that is imparted to those seeking to enter these laboratories. Some would go so far as to categorically state that there is a discernable and pronounced decline in the quality of those who seek careers in science. But, even a casual visit to many of the countless colleges (and Universities) that grant Bachelor's and Master's degrees across the country will reveal that a problem of alarming dimensions is slowly in the making. In some states, Karnataka which is home to this journal is among them, there are few takers for degree courses in traditional disciplines like chemistry, physics and mathematics. Biology is marginally more attractive, especially if the course is repackaged as 'biotechnology' or 'microbi
still appears hope, that those who ride the crest of the biotechnology wave will one day reap rich rewards. Classical disciplines like botany and zoology are best hidden, notwithstanding the growing crescendo of concern about our biodiversity resources. Geology, despite the earthquakes, has a very low profile in undergraduate institutions. In Karnataka, we have the remarkable spectacle of 'vanishing classes', in the once, moderately popular physics chemistry–mathematics (PCM) combination. A few weeks after the commencement of the academic year, supplementary admissions to the myriad 'engineering colleges' across the state are announced; prompting an exodus, leaving the classes, in many disciplines, of the degree colleges empty. Since many teachers then have little work, the Government, in turn, would like to downsize grants, understandably prompting a flurry of protest. Interestingly, in the same colleges, courses in commerce and the humanities do not suffer this phenomenon of sudden migration; indeed, while students in the former look forward to bright prospects in the liberalized economy, those in the latter appear to have fewer choices. For students the die is cast as they near the end of their schooling.

But, today the various disciplines of science in our colleges face an automatic regression, as students flock elsewhere; the onslaught of 'information technology' (IT) has been relentless. Few, parents or students, can resist the lure of this hugely successful area of business, which promises to revolutionize our lives. The governments in some states have contributed to this frenzy by opening innumerable institutes for information technology. It is not clear, however (at least to this writer), whether teaching in these institutions conforms to minimum standards and whether qualified teachers are readily available. The contours of the discipline of 'information technology' are also not clearly drawn. Most recently, in the wake of the genome euphoria, 'bioinformatics' (whatever it may mean) has become a favoured child for state support. While the IT courses draw into their fold students from almost all disciplines of science and engineering, the flight from the latter is most pronounced. The professional training imparted to these students in various specialized disciplines is largely wasted as they take up positions in the 'software sector'.

The hardest hit departments are those that teach pure sciences in the colleges. For managements in many institutions, particularly those that are privately funded, the way to survival appears to be to start new courses, business administration and catering technology amongst others. But, as the science departments slowly decay we may ask: 'Do we need science courses at all? If there are no takers, should these not be wound up? After all, in the brutal aftermath of liberalization, should not the market rule?' Ironically, science teaching is at its lowest ebb in India at a time when science appears to be poised for another major leap forward. The remarkable technological advances of the last two decades, computers amongst them, have brought many areas of science to a critical point where major beneficial advances appear to be around the corner. While biology has been at the forefront of recent discussions, in the wake of the spectacular successes on many fronts, it is clear that modern biology is not a homogeneous discipline; rather it is a conglomeration

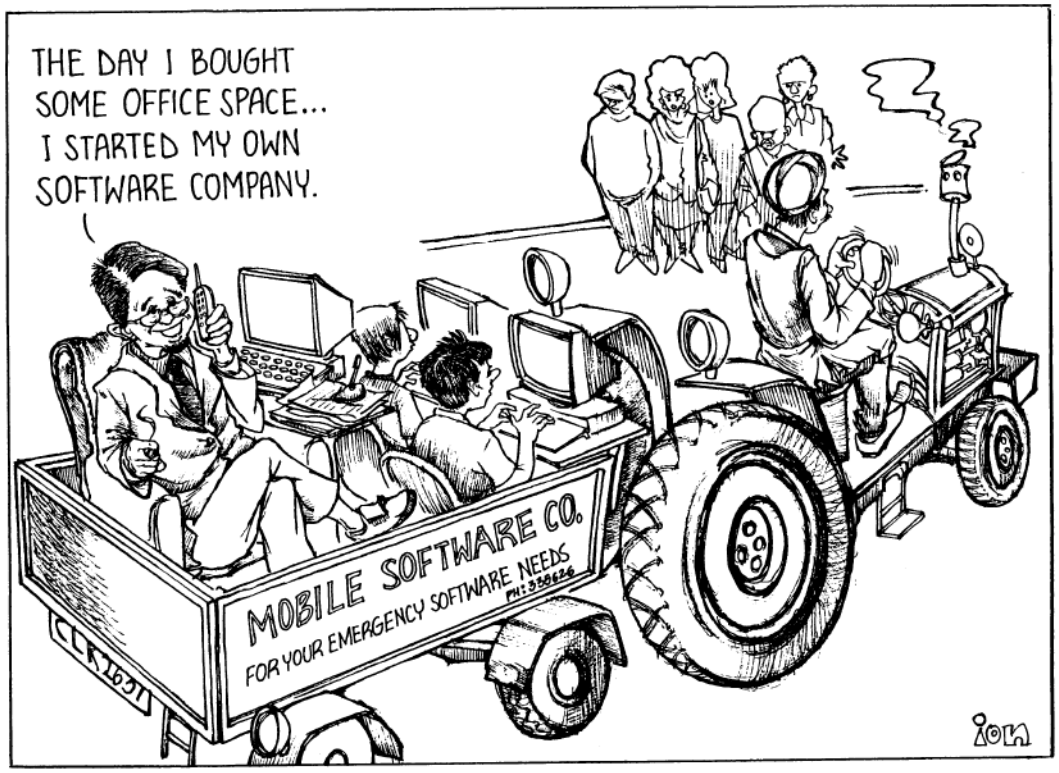
of almost every discipline of science. It is therefore critical to reexamine the issue of undergraduate science institutions and the teaching of science, if Indian research laboratories hope to participate in the inevitable flood of science that lies ahead.

Research institutions face stiff competition for a limited number of well qualified science students, as every major university in the world attempts to attract the best of graduate students. Major institutions overseas now conduct vigorous recruiting programs for students from India; few can resist the lures of foreign universities. Our own institutions do not often appear sufficiently attractive and are also ponderously slow in their methods for admitting beginning researchers. Recruitment of fresh talent, both at the level of students and faculty, is the lifeblood of a research institution. On both fronts the numbers are diminishing, a phenomenon that does not augur well for the future.

Many years ago, the problems of teaching science in colleges and universities would have rightfully been under the purview of the Ministry of Education. But, even this body has had its name changed a long time ago to the Ministry of Human Resources Development (MHRD). There is indeed a world of difference between the problems of education and the more defined task of 'developing human resources'. Today, we are indeed producing human resources in plenty, for the IT sector and the many areas of commercial activity, marketing and business administration, amongst them. But several other areas of higher education appear to be haemorrhaging severely and may be in need of intensive care. Unfortunately, the education sector has been divided into the groups of primary education and higher education. While there is undoubted need for great emphasis on the former, the needs of the latter must not be completely ignored. It is time that the University Grants Commission worried seriously about the steady decline of science education. With the cooperation of the many Ministries and departments that are directly concerned with the fruits of science, it may still be possible to halt the decay of the departments which teach science.

P. Balaram

Ayan Guha



CURRENT SCIENCE

Volume 80 Number 6 25 March 2001

EDITORIAL

The Bradman class

Don Bradman, the Australian batsman, who died in February at the age of 93, was a cricketing legend. Although he played his last Test innings over half a century ago in England, an occasion on which he was dramatically dismissed by the second ball he faced, Bradman's exploits on the cricket field were to remain forever imprinted on followers of the game. There have been many spectacular achievements in sporting arenas. Jesse Owens' four athletic gold medals in the 1936 Berlin Olympics, obtained before a stadium packed with adherents of Adolf Hitler's theories of racial superority, Muhammed Ali's remarkable run in boxing rings around the globe; Rod Laver's Grand slams achieved on tennis courts of three continents; Pele's magical runs across the soccer fields of successive World Cups, are all amazing examples of achievement. But, in the years that followed, successive generations of performers would set new standards. Carl Lewis was to have a dream run in the Olympics matching Owens. Bjorn Borg and Pete Sampras have contributed to tennis folklore. Diego Maradona, despite his many troubles, for a while wove a magic on the soccer fields that matched Pele's. But, in cricket, the Bradman legend only grew as time passed after his retirement; in a game dominated by statistics, it soon became clear that his batting averages, a tribute to his amazingly consistent, high level of performance, are unlikely to be matched in the forseeable future. In a career that spanned the period 1928–1948, interrupted by the long years of the Second World War, Bradman set new standards of performance on the cricket field. In reading the deluge of obituaries and essays that followed Bradman's passing, I was reminded that the mathematician G. H. Hardy, a diehard cricket enthusiast, was fond of categorizing his colleagues. For a while in the 1920s, Hardy's highest praise for pure mathematicians (he had a healthy dislike for applications), was to describe them as in the 'Hobbs class'. Here the allusion was to the English batsman Jack Hobbs, who for a while was acknowledged as the finest exponent of his craft. But, that was before Bradman first appeared on the playing fields of England. C. P. Snow recollects a postcard that he received from Hardy in 1938: 'Bradman is a whole class above any batsman who has ever lived: if Archimedes, Newton and Gauss remain in the Hobbs class, I have to admit the possibility of a class above them, which I find difficult to imagine. They had better be moved from now on into the Bradman class.' (C. P. Snow, *Variety of Men*, Penguin, 1969, p. 37). Ramanujan, who according to Hardy, ranked with Gauss and Euler for natural mathematical genius, was clearly in the Bradman class.

In many fields, the hindsight of history allows us to recognize those who have scaled the ultimate heights of achievement. From the 18th and 19th centuries Newton, Faraday, Darwin and Pasteur would undoubtedly win Hardy's unquestioned approbation. But, Science's roll call of honour is long and the standard for elevation to the Bradman class must be extraordinarily stringent. The 20th century (indeed the final decades of the 19th century too) presents a formidable task for the assessors of science. The explosion of scientific activity and the unbridled expansion of science makes it extraordinarily difficult to draw up lists of accomplishment. How can we choose members of the Bradman class? In some fields there are scientists who have achieved iconic status. Albert Einstein, whose remarkable year in 1905 is unlikely to be matched for a long time to come (for cricket aficionados this is comparable to Bradman's march through England in 1930), would be at the top of everyone's list. Who, then, would we choose for our physics list from the remarkable cast of characters who engineered the quantum revolution and ushered in the atomic age; Rutherford, Bohr, Schrödinger, Heisenberg, Dirac, Fermi? Where would we place Richard Feynmann? What of Subrahmanyam Chandrasekhar and Stephen Hawking? Physics in many ways is close to mathematics; connoiseurs may be able to judge and rank practitioners, using the combined yardsticks of intellectual ability and achievement.

In many other fields, most notably those where experimental success depends considerably on managerial and organizational skills, judgements are more difficult. Many areas of modern science are so dependent on group activity that individual contributions are well hidden. There are also major discoveries made by scientists whose intellectual influence on their fields is limited. The polymerase chain reaction (PCR) won for its discoverer, Kary Mullis, a Nobel prize in record time and is probably the most widely used technique in modern biology. But, one would hardly raise Mullis to the

Bradman class. While the DNA double helix will undoubtedly remain one of the most enduring images of 20th century science, would James Watson and Francis Crick qualify easily for the Bradman class? Nobel prizes and major discoveries may be necessary but not sufficient requirements for scientific immortality in the case of 20th century scientists.

Chemistry is an area sandwiched between the more glamorous fields of physics and biology. The former held sway till the 1970s, but the latter has a definite edge at present. Chemists, in search of fame and fortune, have often moved towards their sister subjects; interdisciplinary areas after providing a happy hunting ground for discovery. In assessing chemists (and chemistry) I was struck by a device adopted by the magazine *Chemical and Engineering News*, published by the American Chemical Society. In celebrating the 75th year of its publication in 1998, the journal decided to conduct a poll of its readers, who were asked to send in a list of 20 names of 'chemists', whom they considered had contributed maximally to the discipline, in the years of the journal's existence. Democracy was at work in classifying scientists. The results showed that four chemists outran the rest of the field by a substantial margin; Linus Pauling, Robert Woodward, Glenn Seaborg and Wallace Carothers (*Chem. Engg News*, 12 January 1998). Seaborg, one of the codiscoverers of plutonium and nine other transuranic elements was a major figure in the US atomic energy programs. Carothers, less widely known, was the discoverer of neoprene and nylon at Du Pont. But, what struck me most was that this survey clearly identified chemistry's contribution to the Bradman class, Pauling and Woodward.

The centenary of Linus Pauling's birth was celebrated a few weeks ago. In a career that spanned almost seven decades, Pauling introduced quantum mechanics into chemistry, transformed forever chemical concepts of bonding in molecules, laid the foundations of molecular and structural biology and over fifty years ago introduced the idea of 'molecular disease' in a seminal paper on sickle cell anaemia. In an adulatory 'millennium essay', Gautam Desiraju ranks Pauling 'with Galileo, DaVinci, Shakespeare, Newton, Bach, Faraday, Freud and Einstein as one of the great thinkers and visionaries of the millennium' (*Nature*, 2000, **408**, 407). Desiraju asks provocatively: 'Does chemistry lack the high drama of physics and the glamour of biology because they were appropriated by one larger than life individual?' While Pauling hunted comfortably on the borders of physics and biology, Woodward straddled the heart of chemistry, chemical synthesis. The primary purpose of chemistry for over a century and a half has been the synthesis of molecules. No practitioner of the art of synthesis had greater influence than Woodward, who brought formidable intellectual skills and a deep understanding of chemical theory to an area that was experimentally, intensely demanding. In the span of three decades, from the mid-1940s to the mid-1970s, Woodward completely transformed a field; an intellectual synthesis that will remain far more important than the many molecules he conquered. What marks both Pauling and Woodward is the fact that they became legends in their own lifetimes. This, indeed, is a true indicator of the Bradman class.

But, returning to my original theme; polls may indeed be a good way of identifying the Bradmans amongst scientists. While assessing the impact of science and scientists is a difficult task, at the highest level there may be a fair degree of consensus. Finally, even as Bradman passes into the pages of sporting history, his name will forever be associated with a level of excellence, that the greatest achievers strive for in their chosen fields.

P. Balaram

CURRENT SCIENCE

Volume 80 Number 7 10 April 2001

EDITORIAL

Redirecting migrations: Reversing the brain drain

A recent press release issued by the Committee on Science and Technology in Developing Countries (COSTED), based in Chennai, carries the formidable (and somewhat forbidding) title – 'Redirecting Migrations'. The release accompanied a questionnaire that arrived on my desk; a quick glance reassured me that this was merely yet another attempt to address the issue of the flight of trained manpower from the developing countries to the more developed countries of the West. For most, this subject is a very familiar discussion topic of yesteryear – 'the brain drain'. For a long time in India, science students with post-graduate (M Sc) and doctoral degrees obtained in institutions across the country, inevitably looked to the West for opportunities for acquiring Ph D degrees and post-doctoral research experience. In the area of engineering, the success of the Indian Institutes of Technology (IITs) as an experiment in technical education, triggered a remarkable phenomenon beginning in the 1960s; the sustained mass migration of generations of young, trained engineers with Bachelor's degrees, to the United States. The IITs provided a unique atmosphere, where a subtle combination of training and peer pressure ensured that migration was the most important agenda of each succeeding graduating class. The West, particularly the United States, and to a lesser extent Canada and Europe were the favoured destinations. Since the costs of training students at IITs were (and are) heavily subsidised by public funds, increasing concern about the consequences of mass migration of scientists and technologists, crystallized in the 1970s into the many, and generally futile, debates on the brain drain. As a corollary, there were many strategies advanced to stem the tide of migration of trained manpower. Predictably, nothing much happened. Migrations continued and governmental agencies periodically devised schemes for 'attracting talent' to return from overseas; notable among these, was the CSIR 'Pool Officers' scheme. More recently, the ill-conceived Swarnajayanti Fellowships scheme of the Department of Science and Technology, was apparently born as a result of a moment of ministerial inspiration, that was directed towards ensnaring the talent of Indians working overseas. But, generally the worries over the flight of talent have become muted; globalization is changing not only the rules of the game, but is creating new problems and opportunities, which appear of more immediate concern. The IITs are now preoccupied in generating funds from their many, enormously successful alumni who have scaled great heights in the West; indeed a new relationship between the migrants and their institutions is developing, which augurs well for the future. Even new institutions that have as their primary focus, the training of students for the global ventures of the future, are on the anvil. In this context the COSTED note appeared a ghost from a distant past when it states: '. . . Migration is not a new phenomenon but migration of trained S&T manpower from developing countries . . . has been causing great concern to the developing countries because these highly educated and skilled are the very ones whom the less developed countries cannot afford to lose. Since the great majority of these migrants move on a permanent basis, this perverse brain drain not only represents a loss of valuable human resources but could prove to be a serious constraint on the future economic progress of Third World nations.'

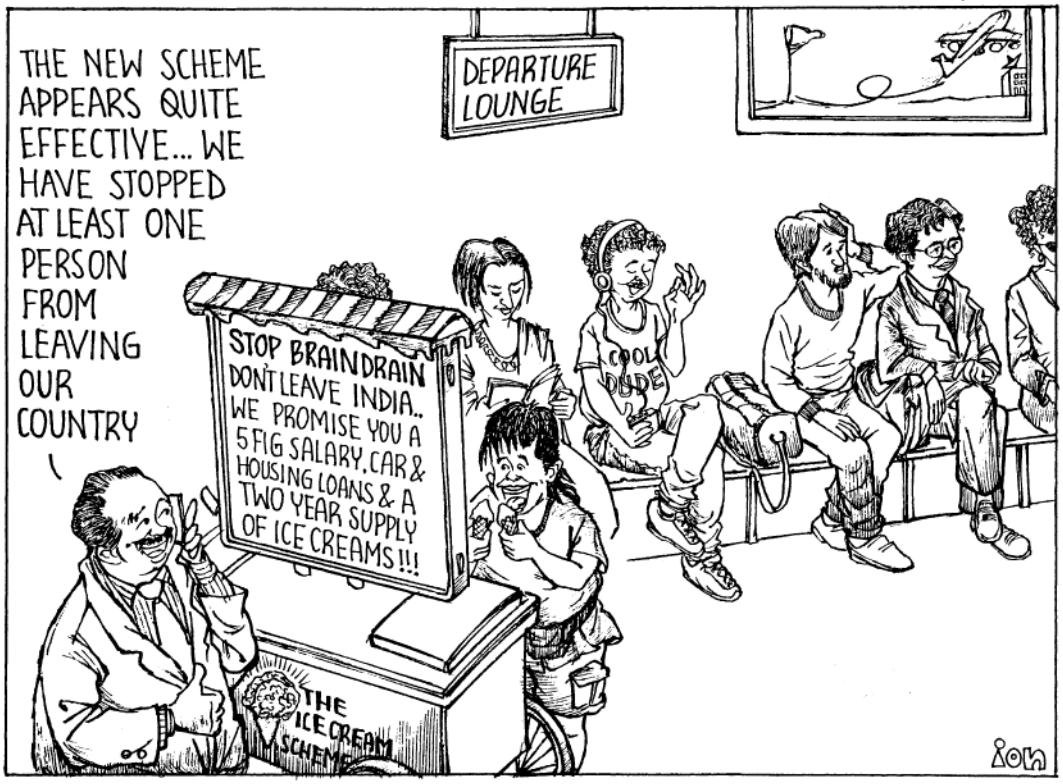
Is the premise, on which the COSTED press release based, accurate? It is quite possible that in some countries of the Third World, notably Africa, the loss of trained manpower may be critically important. Is it important in India? For decades, the output from our institutes of higher education has been large and of a very high quality. Our ability to absorb the talent has been limited and in many areas of science, Ph Ds have little option but to go overseas. In the last 20 years recruitment at our Universities has been limited by local political constraints and our national laboratories and research institutions have had few openings. The steady migration of scientists has been inevitable. Years of restricted recruitment of highly trained manpower, have led to the perception that opportunities in India are limited and that jobs in research institutions are not easy to come by. We would hardly be in a position to optimally employ the best of our compatriots working overseas, were they to decide to return. However, Government agencies periodically decide that special 'material' incentives must be offered to entice

scientists living overseas to return. Invariably, these schemes flounder, because many scientists, who are keen to return, are really looking for institutions, with the necessary academic ambience and research facilities, which will allow them to effectively pursue their chosen areas of research. Few of our institutions appear attractive to those settled overseas. Indeed, some Indian companies interested in setting up state-of-the-art R&D facilities have found it worthwhile to consider setting up laboratories in the West – the forerunner of truly made-in-India multinationals. While the COSTED questionnaire inevitably leans towards new ideas for 'incen redirecting migration, it may be a better idea in the long run to devise financial and management packages to resurrect some of our slowly dying academic institutions and national laboratories. This revival must be fuelled by major new initiatives to create new positions in important areas of science across the country; a strategy that is being pursued in many countries, notably China. Greater attention must also be paid to schemes which enhance the internal mobility of scientists. But, in the end, more worrying than the flight of scientists and technologists overseas, is the internal haemorrhage, where fewer and fewer students opt for science as career and the vast majority of engineering graduates yearn for a career in marketing, management, finance or 'information technology'.

The Declaration on Science adopted by the World Conference on Science, organized by the United Nations Educational, Scientific and Cultural Organization (UNESCO) and the International Council of Scientific Unions (ICSU), held at Budapest in June 1999, quoted by the COSTED note, states that 'favourable conditions should be created that will tend to reduce or reverse brain drain and that any measures should not restrict the free circulation of scientists'. Despite the best of intentions, redirecting migrations is not going to be easy. Whether efforts to do so, by offering selective incentives, are desirable, must be a matter of debate.

P. Balaram

Ayan Guha



CURRENT SCIENCE

Volume 80 Number 8 25 April 2001

EDITORIAL

G. N. Ramachandran

Stereochemistry was born in Paris in the mid-19th century when Pasteur serendipitously observed the spontaneous resolution of racemic tartaric acid. In a spectacular leap of intuition, Pasteur recognized, a quarter of a century before Vant Hoff and Le Bel, that molecular structure and optical activity were related and that common forms like helices and irregular tetrahedra were intrinsically handed. Our present-day understanding of the shapes of molecules and the connections between three dimensionality and chemical function rests to a large extent on the many insights that Linus Pauling had between the 1930s and 1950s. The first triumph of stereochemical thinking in biology was Pauling's realization in 1948, that the non-integral α-helix provided a spectacular solution to the search for folded polypeptide structures. The Watson–Crick double helical structure of DNA appeared in 1953; clearly the pre-eminent example of a three-dimensional model that instantly reveals the basis for biological function. In 1954, the structure of collagen, the most abundant protein of connective tissue was described by G. N. Ramachandran and G. Kartha, working at the University of Madras. The structure, an intertwined three chain triple helix, elegantly accounted for the limited experimental data and provided a compelling rationale for the unique amino acid composition of collagen. It also marked the emergence of a new star, in the fledgling field of structural molecular biology, G. N. Ramachandran.

When G. N. Ramachandran died at Chennai (formerly, Madras) on 7 April 2001 the curtain was rung on one of the most remarkable chapters of modern science in India. Ramachandran began his journey in science when he joined the Indian Institute of Science, Bangalore in 1942, as a student in the Electrical Engineering Department. He realized very quickly that his interests lay in physics, a subject then overwhelmingly dominated at Bangalore by the presence of the legendary C. V. Raman. In retrospect, it appears almost inevitable that Ramachandran would desert electrical engineering and embrace physics; an event that appears to have been accompanied by Raman's soothing words to the Professor of electrical engineering: 'I am admitting Ramachandran into my department as he is a bit too bright to be in yours'. Ramachandran was to eventually become the most distinguished of Raman's students. At Bangalore he first submitted a thesis entitled 'Optics of Heterogeneous Media' for an M Sc degree of the University of Madras and later a doctoral thesis in 1947, which contained some of the earliest applications of X-ray diffraction to the study of crystal perfection. He spent two years in Cambridge, obtained a Ph D degree working with W. A. Wooster and returned to Bangalore in 1949 to begin an independent career as an assistant professor of physics, working in the X-ray diffraction laboratory that he was instrumental in building as a student. He did not stay long. Madras University beckoned with a Professorship and the responsibility to head the Department of Physics. Ramachandran moved to Madras when he was just 30, to begin an extraordinary burst of scientific activity. At Madras, he flourished under the benign and supportive influence of an enlightened Vice-Chancellor, A. Lakshmanaswamy Mudaliar; a relationship reminiscent of that between Asutosh Mukherjee and C. V. Raman in Calcutta. J. D. Bernal visited Madras in 1952 and in a casual conversation suggested that the structural proposals for collagen were unsatisfactory. The Central Leather Research Institute was next door to Ramachandran's laboratory. He soon had a sample and X-ray diffraction photographs. The triple helix emerged in two papers in *Nature* in 1954 and 1955, introducing the coiled coil concept, a fundamental advance in the understanding of polypeptide structures. But, in a sharp critique, Francis Crick, fresh from his DNA success, together with Alexander Rich argued that the Madras structure was 'stereochemically unsatisfactory'. Out of the brewing collagen controversy was to emerge what is undoubtedly Ramachandran's finest contribution to structural biology. Spurred by the criticism of unacceptably short interatomic contacts in his collagen structure, Ramachandran set out to investigate the criteria for describing stereochemically acceptable structures for polypeptide chains. Using the simple hard sphere model for atoms and driven by the insight that each residue in a polypeptide chain is allowed only two degrees of torsional freedom, Ramachandran together with his colleagues C. Ramakrishnan and V. Sasisekharan laid the foundations for the conformational analysis of polypeptide chains. Their seminal paper published in the *Journal of Molecular Biology* in 1963, circumspectly titled 'Stereochemistry of polypeptide chain configurations', introduced the famous two-dimensional map, which was to eventually bear Ramachandran's name. Today, for beginning students of biochemistry, protein structures

are introduced with a discussion of the Ramachandran map, which also forms the cornerstone for many discussions of protein folding. The simple concept of reducing the 'structure space' of protein chains to two-dimensions with dihedral (torsional) angles serving as the variables had a profound impact on stereochemistry and structural biology. In the late 1960s and early 1970s a flood of important papers appeared from Ramachandran's laboratory, which addressed key issues in the structural chemistry of proteins and peptides; chain reversals, *cis*-peptide bonds, hydrogen bonding, non-planar amide distortions and novel helices in polypeptides with alternating L and D-residues.

While polypeptide stereochemistry was a dominant theme in Ramachandran's career between the 1950s and 1970s, his sharp incisive mind turned often to his first love, X-ray crystallography. Fourier theory fascinated him and he authored, with his colleague R. Srinivasan, an influential book on Fourier Methods in Crystallography. Ramachandran's paper in this journal on a 'A new method for the structure analysis of non-centrosymmetric crystals' was influential in promoting the use of anomalous scattering for solving the crystallographic phase problem. In 1971, Ramachandran together with A. V. Lakshminarayanan published a key paper on three-dimensional image reconstruction, which was to have important applications in Computer Assisted Tomography. Ramachandran had a remarkable ability to cut through unnecessary details and go straight to the heart of a problem. This quality coupled with formidable physical insights and mathematical skills allowed him to make many important contributions in biophysics and crystallography.

Ramachandran was the editor of *Current Science* between 1950 and 1957. This was the only time that the editorial office moved out of Bangalore in the 69 years of this journal's existence. He published several important papers, notably his work on the X-ray topographs of diamond and anomalous dispersion in this journal. In 1990, *Current Science* highlighted his contributions to polypeptide stereochemistry in a special issue. Ramachandran was an excellent teacher and lecturer. His clear grasp of the fundamentals of a problem allowed him to convey to listeners the key elements of a scientific issue, shorn of all the complicating details.

Ramachandran was widely honoured for his work in India and abroad. In recognizing Ramachandran's work most agencies in India honoured themselves and conferred a new lustre on the awards they instituted. A dispassionate analysis of his life and work will reveal that he did not, in large measure, get his due. The Government of India, undoubtedly advised by the scientific establishment, never found it fit to include him in the annual Republic Day honours list. This act of omission in no way diminished his stature; instead it forever dimmed the lustre of these awards. The Royal Society belatedly recognized Ramachandran in 1977, almost towards the end of his active scientific career and over two decades after his remarkable work on collagen. Ramachandran was clearly a 'Nobel class' scientist, to borrow a phrase from Eugene Garfield. But, his active career was all too brief by modern day standards. For the last twenty years Ramachandran was not really visible internationally, reminding us of one of the ironies of modern science; achievement alone is not enough, packaging and marketing play an important role. In India, where administrative positions are often considered a mark of scientific success, Ramachandran was essentially an 'outsider' to the establishment. We have yet to learn that idiosyncratic personalities often make the most original contributions to our science. Ramachandran did all his work in India, following in the footsteps of his mentor, C. V. Raman. In the 1960s and 1970s he did travel regularly to the United States, to the University of Chicago where he held a visiting professorship. At Madras, Ramachandran's work brought an unprecedented level of recognition to the University. The two international conferences he organized in 1963 and 1968, brought to Madras some of the most famous names in molecular biology and biophysics; Linus Pauling, Severo Ochoa, Maurice Wilkins, Stanford Moore, David Phillips, Ephraim Katchalski, Harold Scheraga, Paul Flory, Elkan Blout and John Schellman.

Ramachandran returned to Bangalore to set up the Molecular Biophysics Unit at the Indian Institute of Science in 1971. His move from Madras was catalysed by the deteriorating academic atmosphere of the University. Indeed, Ramachandran's two decades at Madras University clearly showed that the highest levels of research could be practiced within our University system. His departure signaled an impending change. We have all watched, with varying degrees of helplessness, the steady decay of University science in India, over the last three decades. At Bangalore in the period between 1971 and 1979, Ramachandran fashioned a new department, which has grown into a major center of structural biology. Like many extraordinarily gifted individuals, Ramachandran often had an uneasy relationship with his surroundings. It was not easy for him to come to terms with mediocrity. Elevated to the formidable position of a head of department at 30, he grew to be isolated from his colleagues, rarely establishing the easy academic relationships that make science a pleasure. But even at the height of his career Ramachandran most enjoyed scientific discussion; unfortunately his surroundings could rarely rise to the levels he demanded. His last years were troubled. A stroke and the steady onset of Parkinsonism diminished his movements and activities. The loss of his wife Rajalakshmi (Rajam), after 53 years of marriage, in 1998, was a great blow. In many ways, when the end came it was indeed time to go. But, Ramachandran has left behind a rich scientific legacy. His achievements will serve as a source of inspiration for generations to come. Ramachandran was undoubtedly one of the most outstanding scientists of post-Independence India and truly, a jewel in the crown of India's science. For us, it was a very special privilege to have known him.

P. Balaram
S. Ramaseshan

CURRENT SCIENCE

Volume 80 Number 9 10 May 2001

EDITORIAL

The astrology fallout

The generally placid academic science community in India has been stirred into unprecedented ferment. The catalyst for this transformation is a letter that emanates from the University Grants Commission (UGC). A missive, dated 23 February 2001, conveys to India's numerous universities, the decision that 'there is an urgent need to rejuvenate the science of Vedic astrology in India'. The UGC, which has considered a proposal to set up departments of Vedic astrology since last summer, feels that such a move would allow 'all this scientific knowledge to reach society at large' and might even 'provide opportunities to get this important science exported to the world'. The Commission approved the plan to set up a few departments of Vedic astrology in Indian universities, which 'would provide exclusive teaching and training in the subject leading to certificate, diploma, undergraduate, post-graduate and Ph D degrees'. As is customary, an 'expert committee' was formed to formulate the guidelines; the UGC then approved the new 'Jyotir Vigyan' courses.

Universities are to bid for starting these new courses and the UGC has decided that 'the syllabus for the courses shall be sent to the selected universities'. The new departments will have Professors, Readers, Lecturers, Library Attendants and Computer Operators; the last clearly indicating that there will be additional employment opportunities in academia for the products of the innumerable 'computer schools' that abound in our cities. The UGC has also suggested a possible 'non-recurring' budget of Rs 6 lakhs for an 'observatory' and Rs 5 lakhs for a 'computer lab and horoscope bank', in these carefully chosen departments.

The UGC's objectives are clearly and succinctly stated (although at times, inelegantly): *'Vedic astrology is not only one of the main subjects of our traditional and classical knowledge, but this is the discipline which lets us know the events happening in human life and in the universe on time scale. The distinguishing feature of this subject is that it makes us familiar with time, its nature and feature and its effects on human life and other events and that way it helps us to manage and make optimal utilization of time. It is a common feature that despite best methods adopted for estimation, the events happen in different ways and add to worries, tensions and frustration in life. Here, Vedic astrology can help us see the unforeseen, it being the subject dealing with the time. Starting of the courses in Vedic astrology in universities will not only impart the knowledge of this subject to the people, but will also add a new dimension for research in the fields of Hindu-Mathematics, Vaastushastra, Meteorological Studies, Agriculture Science, Space Science, etc.'* In addition to training new generations of astrologers (although this is not explicitly stated), the UGC hopes that *'beneficiaries of these courses would be students, teachers, professionals from modern streams like doctors, architects, marketing, financial, economic and political analysts'*. Undoubtedly, after careful reflection, the UGC has recognized the importance of predicting the future, accurately, and identified the professions most likely to benefit from the advance of astrology.

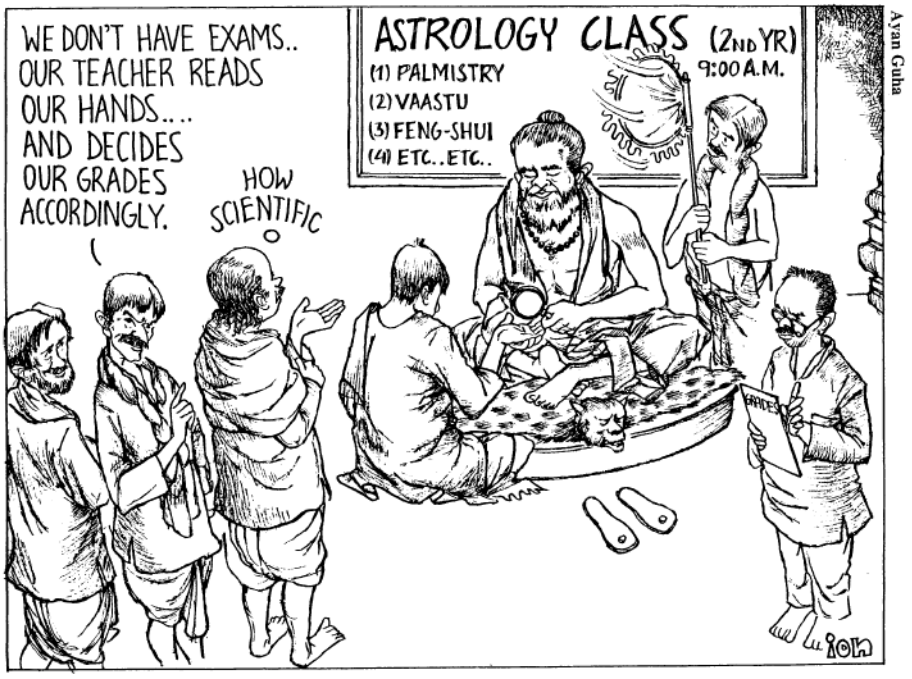
The UGC's assertion that astrology, specifically, 'Vedic astrology', is a science has stirred a hornets' nest. When I did refer, somewhat unkindly, to the impending plan of the UGC, in these columns a few months ago (*Curr. Sci.*, 2000, **79**, 1139–1140), there was little by way of encouraging response. I was sharply (and justifiably) reminded that my views were a result of ignorance (Sarma, V. V. S., *Curr. Sci.*, 2001, **80**, 115–116). The opponents of Vedic astrology courses can easily be confounded with the charge: 'But you know nothing about the *Vedas* – you do not even know Sanskrit'; an almost unchallengeable appeal to an apparently glorious past. Another spirited attack suggested that the 'initiative by the UGC and the government of India to introduce new courses clearly represents an open-minded attitude for the perusal of the unexplored domains of a body of knowledge, accumulated at a time when the present practice of the science had not taken its roots' (Ganeshaiah, K. N., *Curr. Sci.*, 2001, **80**, 719–720). But, the UGC's unambiguously worded circular appears to have acted as a wake-up call. The newspapers, magazines and even our Correspondence columns are now full of the astrology controversy. The central issue is whether astrology courses, Vedic or otherwise, should be taught in the science faculty of our universities. Parenthetically, it is worth noting that some institutions, Madurai Kamaraj University amongst them, already have courses on astrology. It is quite possible that if astrology were introduced in the arts and humanities faculties, there may have been less of an outcry. Scientists of diverse persuasions are convinced that astrology has no scientific basis. Indeed, faculty from the Inter-University Centre for Astronomy and Astrophysics has suggested that 'if Vedic astro-

logy is to be promoted as a subject relevant to our heritage, it could be included as part of ancient Indian studies, rather than projecting it as Vigyan (science)'.

One of the redeeming features of the astrology debate is that it appears to have brought widely disparate groups of scientists together; even the pro- and anti-nuclear physicists seem united in their opposition to the UGC's move. Academies have, of course, been notably silent; but for years now they have struck a Faustian bargain with government agencies, seemingly unable to even elect Presidents, who do not hold high administrative office. Despite the groundswell of protest from the scientific community, the UGC circular reflects the Commission's view that astrology is, in fact, a science. After all, the funds for an 'observatory' imply that astrology faculty will vie with their astronomy colleagues for a glimpse of the planets and the stars. The linking of 'computer labs' and 'horoscope banks' is an unmistakable sign that the Commission is convinced that robust algorithms exist, which link our times of birth to our futures. The Andhra Pradesh High Court, while dismissing a writ petition against the UGC's decision is reported to have felt that 'the Courts, ill-equipped as regards such matters, must adopt the doctrine of self-restraint and leave such matters in the hands of the expert committee' (*The Hindu*, 5 May 2001, p. 11).

Were there no members of the UGC who disagreed with the stated objectives of the astrology programme? Is the UGC so devoid of inputs from the scientific community that such a circular could be issued? Or is it that silence is the expedient path in such committees? The discussions in the popular press have degenerated into a slanging match between the vocal adherents of astrology on the one hand and science on the other. It is, however, an inescapable fact of life in India that astrologers of all hues have a substantial following. Superstition extends to every walk of life and governments often bend to the dictates of astrologers and soothsayers. Astrology has been here for centuries. Despite the overwhelming scientific evidence that the positions of planets and the time of birth do not dictate the course of human affairs, astrology will be with us in the foreseeable future. The present battle is not between astrology and science. Rather, it is between the UGC, which is charged with the responsibility of fostering higher education and the scientific community. Even amongst scientists there are those who adopt an idealistic stance – the defenders of a policy of 'openness', which makes any subject a worthwhile topic of investigation. These advocates must remember that astrology as a subject of research, its history and social consequences as topics for study, can still be legitimately pursued within the portals of our universities. The real question is, must we train a new generation of astrologers in a formal sense, legitimizing the discipline as a science and even contemplate 'export'? Will these new departments be staffed by scholars who pore through ancient texts or will they be designed to produce 'diploma holders', who are now licensed to predict our futures and earn a living? The UGC's circular clearly suggests the latter. The UGC's view that astrology needs to be promoted as a science in our universities is an unnecessary and pointless 'initiative', which will further erode the credibility of our institutions. It is possible to rewrite history; it may even be possible to rewrite the history of science; but it may be well nigh impossible to completely rewrite science.

P. Balaram



CURRENT SCIENCE

Volume 80 Number 10 25 May 2001

EDITORIAL

The importance of being impolite

'I have already come to one conclusion as to why science in India is developing with disappointing slowness. It is not because Indians are stupid or lazy. It is because they are too polite.'

J. B. S. Haldane

Some months ago a thoughtful colleague in Calcutta sent me a little book entitled 'Science in Indian Culture', authored by J. B. S. Haldane. This collection of essays, written shortly after Haldane's arrival in India in the late 1950s, has been reprinted by the New Age Publishers Pvt Ltd, Calcutta in 1991. Haldane, a biochemist, physiologist, statistician and geneticist, one of the most colourful figures of 20th century science, came to Calcutta to work at the Indian Statistical Institute. Haldane was the product of a liberal education at Oxford, at a time when the sun never set on the British Empire. His 'only academic degree obtained by examination was in Littarae humaniores, commonly called Greats at Oxford. This examination is based on the study of philosophical and historical works in the ancient Greek and Latin languages'. He had no formal degrees in the subjects on which he left an indelible impression. Haldane was a confirmed Marxist in the 1930s; disillusioned only when Soviet genetics came under the spell of Trofim Lysenko. Haldane brought mathematical rigour to genetics and together with R. A. Fisher and Sewall Wright is considered as 'one of the founders of modern population genetics theory'. Haldane described himself as 'a man of violence by temperament and training'; in the trenches of World War I under enemy fire, he 'found the experience intensely enjoyable'. In the evening of his career Haldane was to come to India, a socialist convinced of the virtues of non-violence, with an abiding interest in Indian philosophy and culture. In his assessment, 'India has made many contributions to world culture. Perhaps, the greatest is the ideal of non-violence. Europe's greatest contribution is the scientific method. If these can be married, their offspring may raise mankind to a new level'. Despite advancing years and a major interest in Indian philosophy, Haldane arrived in India with a clear purpose: 'to escape responsibility for appointments and other administrative work, in order to do research and teaching'. Unhappy with Britain's drift after World War II, Haldane became an Indian citizen and worked in Calcutta and Orissa, until his death in 1964. His training in the classics drew him to Vedic literature and Indian mythology. His definition of the classics bears repetition: 'I mean documents in languages no longer used by all members of a population, documents which are judged to be worth reading for their own sake, and not merely because they serve to determine a historical fact.... I would be inclined to expand this definition and say, documents which are, at least in part, worth learning by heart for reasons other than the conciliation of supernatural beings'. Haldane was not an unabashed Indophile; retreating from the rough and tumble of Western Science, to the seclusion of newly independent India. This country drew him for many reasons, but he came with the intentions of doing science and promoting its cause. He brought with him a robust commonsense and a clarity of thought, which has served Western science so well over the last century. In his essays Haldane proved to be a sharp, if somewhat critical observer of the way science was (and still is) done in India.

Politeness is not a characteristic that one associates with our institutions. Rarely, does one encounter courteous behaviour in our government offices. Nevertheless, to Haldane the Indian characteristic of 'politeness' was a major impediment to the advance of science. In his words: '...again at scientific meetings and usually in ordinary discussion my Indian colleagues are polite about one another's work. In Europe we are usually polite about the work of juniors and highly critical of that of men and women of established reputation. At a recent international meeting on genetics, an American got up after a paper by my wife and said that he will not let her misleading views pass without criticism. She felt that she had reached the status where one is criticized without mercy. She and I at once formed a friendship with the critic. We had something to talk about. In my opinion only a few branches of Indian science have reached the stage of maturity where this is possible.

I may criticize some of my colleagues as I would criticize British colleagues and hurt their feelings severely. Once again I am up against the choice between politeness and efficiency. I do not know how I shall resolve this dilemma. I hope that as Indian science grows up it will become less acute'.

Over four decades after Haldane's musings, it is clear that Indian science has not 'grown up'. Indeed, constructive criticism and debate on science are largely absent in many fields. We now have 'experts' in almost every area; supposed super-specialists, who can rarely be questioned even when they spout the most improbable nonsense. The elevation to the status of an 'expert' is usually by administrative decree; knowledge, accomplishment and most importantly, commonsense, are rarely prerequisites. Haldane's view that 'politeness' must be discarded in debates with senior, established scientists is sacrilege. It is still possible to critically evaluate a research students results in a local seminar; almost heresy to raise doubts in presentations by 'senior scientists'. In universities and national laboratories, administrative seniority is often associated with scientific infallibility, rendering many of our institutions intellectually sterile. But, 'politeness' claims the greatest toll in the decision-making committees of funding agencies. Here again, it is easy to question and contradict junior and unestablished investigators; it is nearly impossible to stem the tide of scientifically questionable projects that emanate from well established individuals and institutions. The growth of 'big science' in India has spawned a host of inter-institutional 'coordinated projects', with a limited scientific base and dubious scientific utility. We can think of mega projects, with limited chances of success and restricted impact, without undue fear of criticism in the decision-making bodies of government; any doubts are always raised only by those on the fringe. In planning for the future, the most outrageous claims can be sustained with little more than a murmur of dissent. This state of affairs prevails in almost all fields, carefully nurtured by our culture of 'politeness' and encouraged by those who have advanced mindless optimism as the key ingredient in scientific progress.

Haldane loved India and his essays are peppered with many perceptive observations on the conduct of our science and scientists. He would probably be disappointed at our continuing 'politeness', were he here today. In an essay entitled 'The Scandal of the Science Congress', describing a meeting held at Mumbai, Haldane could very well be talking about present-day Congresses. He describes a meeting marked by poor arrangements for lectures, an ever changing academic program, the absence of lectures describing original science, but good arrangements for tours and food. In Haldane's words, in one of the lectures, 'the lantern was almost absolutely useless. I was in the second row of the lecture on elementary particles by Professor Abdus Salam, a Pakistani who has recently been given a chair in London. Many of his slides could not be read... '. He goes on to say: 'It is time that responsible persons in India realized that the invitation of foreigners to such Congresses lowers the prestige of Indian science considerably. So do the tours arranged for them later. They are too polite to express their feeling to their hosts but not always too polite to express them to me'. Many years on, I could not but help feeling that Haldane would be surprised at how little things have changed. But, he did have a prescription. In talking about the Science Congress he felt, 'there would be little difficulty in making it useful. This would involve discourtesy to some influential people. But in science efficiency is more important than courtesy'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Third Culture

A little over forty years ago, the English novelist, one-time scientist, technocrat and administrator, C. P. Snow addressed the issue of the intellectual polarization of western society into two groups; literary intellectuals and humanists on one side and scientists on the other. Snow's analysis, undoubtedly a result of years of conversation at the High Tables in Cambridge, highlighted the growing rift between the sciences and the humanities, in universities. To describe this widening schism, Snow entitled his 1959 Rede Lecture at Cambridge as 'The Two Cultures'. When he had finished, 'he had launched a phrase, perhaps even a concept on an unstoppably successful international career; he had formulated a question (or, as it turned out several questions), which any reflective observer of modern societies needs to address; and he had started a controversy which was to be remarkable for its scope, its duration, and, at least at times, its intensity' (Collini, S., in the Introduction to *The Two Cultures*, Snow, C. P., Cambridge University Press, Canto edition, 1993, p. vii). Snow by his own description was a scientist by training, a writer by vocation, fortunate to have a ringside view of the dramatic growth of science, particularly physics, in the period between the 1920s and 1950s. In setting the stage for his famous discussion, Snow emphasized the 'gulf of incomprehension' between 'literary intellectuals at one pole – at the other, and as the most representative the physical scientists'. According to Snow: 'Non-scientists tend to think of scientists as brash and boastful. They hear T. S. Eliot, who just for these illustrations we can take as an archetypal figure, saying about his attempts to revive verse drama that we can hope for very little, but that he would feel content if he and his co-workers could prepare the ground for a new Kyd or a new Greene. That is the tone, restricted and constrained, with which literary intellectuals are at home: it is the subdued voice of their culture. Then they hear a much louder voice, that of another archetypal figure, Rutherford trumpeting: "This is the heroic age of science! This is the Elizabethan age!" Many of us heard that, and a good many other statements beside which that was mild; and we weren't left in any doubt whom Rutherford was casting for the role of Shakespeare. What is hard for the literary intellectuals to understand, imaginatively or intellectually, is that he was absolutely right.'

While Snow touched on the limited feeling that most scientists of the time had for traditional literary culture, he seemed more concerned about the 'pole of total incomprehension of science', which 'radiates its influence on all the rest'. Snow came down harshly on the traditionalists: 'The total incomprehension gives, much more pervasively than we realise, living in it, an unscientific flavour to the whole "traditional" culture, and that unscientific flavour is often, much more than we admit, on the point of turning antiscientific . . . . If the scientists have the future in their bones, then the traditional culture responds by wishing the future did not exist. It is the traditional culture, to an extent remarkably little diminished by the emergence of the scientific one, which manages the western world. This polarisation is a sheer loss to us all.' Snow then worried about the need to rethink education, the need to limit specialisation, but rather despondently concluded: 'All the lessons of our educational history suggest that we are only capable of increasing specialisation, not decreasing it.' In persuasively overstating his case, Snow characterized the literary intellectuals 'who raised doubts about the human cost of the industrial revolution' as 'natural Luddites'; a charge that was to provoke a furious counter-attack by F. R. Leavis, a University Reader in English at Cambridge and a controversial literary critic. The *Spectator*, which published Leavis' polemic criticizing Snow, also carried an editorial that took issue with Snow's apparent suggestion 'that science provided sufficient light by which to steer the world'. In annotating the reissue of Snow's essay, over thirty years after its original appearance, Stefan Collini draws attention to a *Spectator* editorial, which mischievously quoted William James: 'Of all the insufficient authorities as to the total nature of reality, give me the "scientists" . . . . Their interests are most incomplete and their professional conceit and bigotry immense. I know of no narrower sect or club, in spite of their excellent authority in the line of fact they have explored, and their splendid achievements there.'

Snow's purpose was to highlight the need to close the gap between what he perceived as two distinct cultures; to

steer a new course towards progress. Snow's vision included the tensions that he foresaw as material progress, fuelled by scientific and technological revolutions, increased the gap between the rich countries and the poorer nations and, indeed between the rich and poor, everywhere. Although Snow viewed science largely as a single 'culture', he was acutely aware that his own experience had led him to identify most closely with the physical scientists; physics, as Collini remarks, was viewed by Snow as 'a gold standard against which weaker or debased forms of science could be measured'. But, in returning to this theme in 1963, four years after his Rede lecture, to take 'a second look', Snow was to point out that 'there is subdivision after subdivision within, say, the scientific culture. Theoretical physicists tend to talk to each other, and, like so many Cabots to God. Either in scientific politics or open politics organic chemists much more often than not turn out to be conservative: the reverse is true of biochemists'. By the early 1960s Snow was prescient enough to 'put forward a branch of science which ought to be requisite in the common culture, certainly for anyone now at school. This branch of science at present goes by the name of molecular biology. This branch of science is likely to affect the way in which men think of themselves more profoundly than any scientific advance since Darwin's – and probably more so than Darwin's. That seems a sufficient reason why the next generation must learn about it. The Church recognises invincible ignorance: but here the ignorance is not, or need not be, invincible. This study could be grafted into any of our educational systems, at high school or college levels, without artificiality and without strain'. It is indeed remarkable that Snow was speaking at a time when cloned genes (and organisms) and sequenced genomes were decades into the future.

But, why have I turned to Snow? The evocative phrase, 'the two cultures', immediately provokes the question: 'Are there not more cultures?' Snow raised this point himself: 'The number 2 is a very dangerous number: that is why the dialectic is a dangerous process. Attempts to divide anything into two ought to be regarded with much suspicion'. Were Snow alive today, he might have clearly seen the contours of a 'third culture' emerging; born out of the ferment of the information and commercial revolution that envelops us. In our academic institutions both the humanities and the sciences are in full retreat, swept away by the high tide of management, commerce and 'information technology' courses, to which students and teachers flock. Even bookshops, the last refuge of the traditional academic, are awash with books on management and computer languages, promising to turn 'invincible ignorance' into commercial success. Informatics courses have sprung up everywhere, tempting students with career prospects after minimal computer training; converting a whole generation into mindless extensions of computer hardware. The providers and organizers of 'information' have little to do with its generation or analysis. The marriage of information technology and the exploding field of genomics has given birth to 'bioinformatics'; an area whose commercial possibilities are already seducing the original information technologists, while its growth promises to wean away the diminishing number of new entrants to the vast field of experimental biology.

We live in an age dominated as never before, by commerce. The last two centuries of science have brought forth unprecedented technological progress; but in this time science has also contributed enormously to our understanding of nature and ourselves. It is indeed one of the great ironies of our times that science (and of course, the more traditional academic disciplines) has become a victim of its own success. Snow would have been appalled by the emergence of this third culture, dominated by the technologies of communication and driven solely by the mindless consumerism of the marketplace. Nowhere is the emergence of this third culture more manifest than in India, where the headlong rush of students to management, informatics and commerce courses, threatens to completely impoverish the academic life of our colleges and universities. The third culture may be more threatening to our intellectual life, than Snow's two culture schism ever was.

P. Balaram

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EDITORIAL

Starting out in research

The practice of science is a highly professional activity today. New entrants to research can be confronted with bewildering choices at the beginning of their careers; difficult decisions need to be made about the fields of activity they wish to pursue. Most often, a choice casually made at the entry to a Ph D program remains the dominant steering force during a research scientist's lifetime. At this time of the year most of our research departments and institutions are in the process of admitting fresh students to Ph D programs; the choices to be made by successful entrants are determined by a multitude of factors, many of which lie outside the students control. While opportunities do exist for changing fields and techniques, these are not always easy to come by. The course of a Ph D program merges almost continuously with postdoctoral research; for students from many institutions in India this is almost inevitably a stint of several years in a Western laboratory. On an average, most aspiring scientists spend almost a decade acquiring Ph D degrees and gaining appropriate postdoctoral experience, before they are in a position to look for permanent placement and embark on an independent scientific career. Since the median age of entry to our Ph D programs lies in the early twenties, most of our potential scientists are in their early thirties before they are in a position to chart their own course. It is at this point that the real trouble begins. The window of opportunity to obtain a position on the faculty of our institutions is narrow; too little postdoctoral experience can be a disadvantage, while too much can also be a disqualification, since applicants on the wrong side of 35 are not enthusiastically considered by selection committees. Many applicants who are overseas find it almost impossible to squeeze through this slit of opportunity. The fortunate few, who manage to manoeuvre themselves into permanent positions, are now ready to embark on their own careers; independence is a prized attribute in the profession of science. For the first time these new recruits are in a position to follow their own instincts in research; there is no mentor (or 'guide') breathing down their necks, clearly marking out the paths to be taken. I am talking here primarily about academic science as practised in many universities and central institutions, including several of our national laboratories. A somewhat different situation obtains in industry and the strategic laboratories, where areas of research and project goals are more clearly defined and a chain of command and responsibility is well laid out.

Scientific independence can be, and often is, a wonderful blessing. But at the start of a career many new entrants may feel as though they are traveling in a boat on a treacherous sea, without a guiding hand on the rudder. The problems of 'getting started' in India can be compounded by the absence of adequate 'start up' support, physical difficulties in creating functional laboratories and a generally unsupportive and disconcertingly, disinterested environment. But beyond the problems of limited resources and minimal infrastructure looms an even more difficult issue: what problems should an independent researcher tackle at the start of a career? The most famous, recent attempt to give 'Advice to a Young Scientist' was, of course, Peter Medawar's extended essay (Harper and Row, 1979). Medawar in his preface says that he was trying to write a book that 'I myself should have liked to read when I began research, before most of my readers were born – that is not a patronizing comment but a straightforward recognition of the fact that most scientists are young in years and that no one actively engaged in research thinks of himself as old'. Medawar's book, written inevitably for a scientist starting out in the West, contains many nuggets, but I particularly liked his description of the 'art of research as the art of the soluble'; an adaptation of Bismarck's dictum that 'politics is the art of the possible'. Medawar is firm with his prescription for those at the starting line of an independent career: 'After graduate students have taken their Ph Ds, they must on *no account* continue with their Ph D work for the remainder of their lives, easy and tempting though it is to tie up loose ends and wander down attractive byways. Many successful scientists try their hands at a great many things before they settle down upon a main line of investigation....' Medawar's caution might well be extended to beginning faculty at our institutions, who return after years of postdoctoral toil, often from the West, carrying with them an intellectual baggage which is not easy to jetti-

son. Many scientists also become prisoners of techniques they have specialized in; the scientific problem frequently takes a backseat, while specialized (and expensive) methodologies chase questions that need answers. Theoreticians, of all hues, are more fortunate than their experimentalist counterparts; the travails of organizing a laboratory, finding financial support and acquiring a group of coworkers are either unnecessary or on a scale that is substantially smaller. Not infrequently, many new recruits in India continue to work on 'imported' research problems, sometimes with the tacit support of foreign mentors. The entry of international agencies (and national ones, too) for funding overseas collaborations acts as an incentive for continuing to work on problems, which can be appendages to a major project in well-known laboratories. Occasionally, a starting researcher misjudges his or her own contribution to an important and successful project in a mentor's laboratory and begins a competitive line of investigation. This is an approach that almost guarantees failure. It is normally foolhardy and mildly improper to enter into such direct conflict with mentors.

For beginning researchers in India, the choice of problems and areas of research are critically important. The temptation to continue postdoctoral problems is overwhelming. Frequent trips to old haunts in the West, maintain an umbilical cord that is not easily cut. Well meaning international agencies underwrite the costs of these expeditions and perpetuate projects in which Indian laboratories play a minor, supportive role. When a Western connection is absent there can be a strong internal factor which is again detrimental to independent development of young scientists. In many Universities and research institutions new recruits to the faculty are old students, who have sometimes developed no independent research interests of their own. This kind of inbreeding, a compelling demonstration of the power and short-sightedness of senior faculty, has had a debilitating effect on major science departments across India.

But, for starting scientists, while fertile research problems are important, money to run laboratories is also crucial. Funding for research comes slowly; all our major funding agencies have succumbed to a bureaucratic stranglehold, from which they seem unlikely to recover. The individual investigator-driven basic research project is accorded the lowest priority for processing, leaving scientists running from pillar to post to get sanctioned funds released. It is, therefore, easy to be seduced by the promise of funds for research in areas that government committees have decreed as potentially 'applicable'. Once the bait is taken, many new entrants embark on a ride to nowhere – many 'young' biologists, for example, chase mythical vaccines, therapies and diagnostics, within the precincts of institutions, hardly designed for such 'focussed' activities. There is an unfortunate tendency in India to underestimate the complexity of many scientific problems; an alarming technical innocence has begun to spread contagiously. To paraphrase Medawar, self-deception is the first step in deceiving others. There are few takers for quiet and scholarly work in areas far removed from the limelight; indeed, fishing in the backwaters is an unfashionable activity. But, new entrants to research might in fact, be well advised to consider carving out a niche area, even if this involves swimming against a local tide. Fashions are notoriously fickle and patient plodders do sometimes find that they have created a pleasant, research garden of their own. Diversions from research, and our institutions provide many, can sometimes tempt a beginning researcher from the chosen path. There is however, one activity that complements and enhances research, and that is, teaching. Unfortunately, teaching even at the pre-Ph D level is an activity scorned in our research institutions. In places where teaching is essential, the conduct of classes becomes a convenient excuse for diminished research performance. To many of the new members of our institutions, teaching appears an impediment, detracting from their research functions. Here they forget that for the newly independent researcher, teaching becomes a vital process of learning; enlarging the base of facts so essential for everyday science, sometimes clarifying concepts and methods that have been clouded by misunderstanding and confusion over the long years of studentship.

But, eventually the ship does set sail and most new researchers are off on their own. Sometimes, they chase with quixotic relentlessness their pet hypotheses. Here, Medawar has a stern admonition – 'I cannot give any scientist of any age better advice than this: the intensity of the conviction that a hypothesis is true has no bearing on whether it is true or not'. Others, worry their favourite problems with a ferocious intensity. Still others, a pragmatic majority, view science as a sedate profession, behaving much as wage earners do in any area of activity. A very small minority become the torchbearers of a new generation. What are the factors that make for success in science? No easy answers are forthcoming. But, two ordinary qualities are useful – persistence and common sense. Medawar believed that there are no 'important national differences in the ability to "do" science', presumably upholding 'Descartes contention that common sense is the most equitably distributed of all human gifts'.

P. Balaram

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EDITORIAL

Origins of life

The origins of life are buried in the murky past of evolutionary history. Biology's reigning paradigm, Darwinian selection, is the cornerstone of our understanding of biological evolution, providing a rationale for the extraordinary diversity of form and function that characterizes life on earth. The influence of evolutionary theory on our views of biology is succinctly captured in Theodosius Dobzhansky's famous saying: 'Nothing in biology makes sense except in the light of evolution'. Philosophers and poets have worried about life's purpose and Longfellow said it well:

> Life is real, life is earnest
> And the grave is not its goal
> Dust thou art to dust returnest
> Was not spoken of the soul.

But, while life's purpose must clearly lie outside the ambit of science, life's origins might indeed, be a legitimate subject of scientific enquiry. In an intriguingly titled book, *The End of Science*, John Horgan (Broadway Books, New York, 1996; Addison-Wesley, 1996) says: 'If I were a creationist, I would cease attacking the theory of evolution – which is so well supported by the fossil record – and focus instead on the origin of life. This is by far the weakest strut of the chassis of modern biology. The origin of life is a science writer's dream. It abounds with exotic scientists and exotic theories, which are never entirely abandoned or accepted but merely go in and out of fashion.' Darwin himself was remarkably circumspect, titling his treatise *On the Origin of Species*, avoiding speculating on life's origins. In Darwin's world, life had already been created; his concern was really with adaptation and evolution, his purpose to understand the imperatives that give rise to the spectacular diversity of life forms on earth. Richard Dawkins, possibly the most vigourous proponent of Darwinism, argues that the 'theory of evolution by natural selection is the only workable explanation that has ever been proposed for the remarkable fact of our existence, indeed the existence of all life wherever it may turn up in the universe. It is the only known explanation for the rich diversity of animals, plants, fungi and bacteria; not just leopards, kangaroos, Komodo dragons, dragonflies, corncrakes, coast redwood trees, whales, bats, albatrosses, mushrooms and bacilli that share our time, but the countless others – tyrannosaurs, ichthyosaurs, pterodactyls, armour-plated fishes, trilobites and giant sea scorpions – that we know only from fossils but which in their own aeons, filled every cranny of land and sea. Natural selection is the only workable explanation for the beautiful and compelling illusion of "design" that pervades every living body and every organ' (Dawkins, R., in the foreword to *The Theory of Evolution*, Maynard Smith, J., Cambridge University Press, Canto edition, 1993).

Biological evolution is a slow process and organisms have had plenty of time. The earth itself is a little less than 5 billion years old. The earliest life forms, precursors of the simplest bacteria, may have originated 4 billion years ago. Eukaryotes, nucleated cells, are estimated to have appeared only a billion years ago. Present-day biological theories provide little light on what might have happened in the long years of prebiotic evolution. The major stumbling block is the vast body of biochemistry that is inseparable from life. The simplest attributes of life, reproduction and the transmission of hereditary information involve formidably complex biochemical processes. Life involves sophisticated processes of energy transduction and cellular communication, which is central to the functioning of complex organisms, requires an exquisitely controlled cascade of biochemical reactions. Precise molecular organization, breathtaking spatial and temporal control of cellular chemistry and a remarkable fidelity of replication are among the hallmarks of the simplest of life forms. The evolution of life in the seething ferment of a prebiotic soup, however unlikely it may seem, appears to be a favoured view. Given the complexity and fluidity of molecular organization in biological systems, the spontaneous generation of a prototype primordial cell can only be seen as a happy 'chemical fluke'. The famous Urey Miller experiment of 1953, in which a spark discharge was passed through a gaseous mixture of methane, water, ammonia and hydrogen, conditions which purportedly mimic an 'early earth atmosphere', yielded a mixture of amino acids. It is this

experiment that raised the hope that the precursor molecules of biochemistry might indeed be produced under primordial conditions. In the almost 50 years that have passed since the Urey–Miller experiment, there has been little progress; the limitations of such experiments becoming more apparent with each passing decade. Origin of lifers have had their moments of excitement, most often contrived, as in the hoopla about an 'RNA world', in the wake of the discovery of the catalytic power of ribonucleic acid (RNA). John Horgan quotes Lynn Margulis in raising doubts: 'Biochemical systems are effectively historical accumulations. So I don't think there is ever going to be a packaged recipe for life: add water and mix and get life. It's not a single step process. It's a cumulative process that involves a lot of changes. The smallest bacterium is so much more like people than Stanley Miller's mixtures of chemicals, because it already has these system properties. So to go from a bacterium to people is less of a step than to go from a mixture of amino acids to that bacterium'. Among others who have entered the origin of life arena are a diverse group of theoreticians, spouting the new buzzwords of chaos, complexity, self-organized criticality and 'toy models'. But, computer simulations and apparent mathematical sophistication of models for the analysis of complex systems have left most biologists profoundly disinterested.

Despite the debate on its origins, life has firmly taken root on Earth. Does life exist elsewhere in the Universe? Could the 'primordial soup' (a term attributed to J. B. S. Haldane) experiment be in progress elsewhere? Indeed, there have always been adherents of the theories of panspermia, where life on earth was seeded from outer space. Lord Kelvin and Svante Arrhenius were among the early proponents, but the most vigorous campaigners for 'the case for life as a cosmic phenomenon' have been Fred Hoyle and Chandra Wickramsinghe (cf. *Nature*, 1987, **322**, 509). In an 'Opinion' piece these authors argued for an apparent correlation between sunspots and influenza epidemics, raising the picturesque possibility of strains of viruses arriving on earth, borne by solar winds, travelling across the barren reaches of space (*Curr. Sci.*, 2000, **78**, 1057). For a long time scientists, who worried about life's origins or the existence of life elsewhere in the Universe, have worked on the fringes of science; generating hypotheses with little hard data, toiling on the borders of fact and fiction.

While many assessments of origins of life research are pessimistic, those who search for life beyond Earth may take heart from the special feature produced recently by the *Proceedings of the US National Academy of Sciences* (*PNAS*, January 30, 2001). An introductory note defines the field – 'Astrobiology is not an autonomous or self-sustaining discipline. Rather it is a hybrid subject emerging at the crossroads of astronomy, geology, paleontology, physics and biology' (Coughlin, B. C., *PNAS*, 2001, **98**, 796). Notably, chemistry, which is central to the evolution of molecules, cells and organisms is conspicuous by its exclusion; presumably a Freudian slip to which even editors are prone. In focussing attention on the search 'for an alien haven in the heavens', this issue of *PNAS* also records famously dismissive views of astrobiology. Jacques Monod magisterially declared that the 'unfeeling immensity of the Universe' must necessarily lead to the conclusion that biological organization emerged alone on earth. Many evolutionary biologists, Ernst Mayr amongst them, have felt that the search for extraterrestrial life, astrobiology, is an activity popular with some groups of physical scientists; the conglomeration of circumstances that led to life on earth being a highly improbable event. But, the recent focus in *PNAS* on the search for extraterrestrial life highlights many important aspects of the problem; the universal nature of biochemistry and state-of-the-art instruments for detecting extraterrestrial molecules and by extension, signs of life elsewhere in the solar system. In analysing the physical limits of life and the constraints of organic and biochemical processes, Norman Pace concludes: 'In principle, life, regardless of where it arose, could have survived interplanetary transport and seeded the solar system wherever conditions occur that are permissible to life'.

Origins of life research has had a chequered history and has a long way to go; but it is a welcome sign that the field still appears to be stirring.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Big science, small science

The shake-up at the Unit Trust of India (UTI), this country's largest mutual fund organically tied to government, has highlighted the sharp divide between the interests of numerous small investors and the power of a few big investors. In the developing storm cloud over yet another financial scam, there have been rumours in the press about the unwise investments of the UTI in some of the giants of Indian industry; there have also been murmurs that several business houses had advance warning of the impending freeze on trading, allowing them to pull out their investments before the day of judgement. The entire story appeared as a classic case of the 'big' versus the 'small'; the latter, of course, generally ignored and unlamented. Clearly, in the harsh and pitiless world of the financial markets, big fish feed in an unabashedly predatory manner on small fish. But then, this is also true of most aspects of our daily existence. How is it in the world of science? Are there hidden tensions between the realms of 'big science' and 'small science'? Do mammoth organizations and projects elbow out the little individuals in science? Is there at least minimal space for 'small science' anymore?

The definitions of 'big science' and 'small science' are necessarily loose and arbitrary. All of 19th century science, and of course everything that went before, was small science; amateur scientists flirting with the joys of discovery. There was little distinction between scientists, inventors and tinkerers; Faraday, Darwin, Franklin and Edison are among the icons of this early phase. Eccentricity and individuality were among the most striking qualities of many of the remarkable achievers of modern science's early period. Organized science probably took root in early 20th century Germany. The organic chemistry laboratories were forging links with chemical industry by the end of the 19th century; the fruits of basic science were beginning to ripen. Research groups in the best of laboratories were growing in size; the directorial Professor, marshalling his forces much in the manner of an army general, was increasingly evident in early 20th century chemistry. But 'big science' was really formally born in the 1940s, in the midst of World War II, when the Manhattan project provided a striking demonstration of the power of well organized and formally directed science. America learnt its lessons well; in the half century that followed, 'big science' has made remarkable contributions to every area of science and technology, its most visible recent success was celebrated when the human genome sequencing programs neared the finish line. Big science is characterized by well-defined goals, very large financial outlays, ostensibly well thought out strategies and an unmistakably hierarchical administrative structure; large numbers of scientists are deployed at the work bench, reporting to superiors in an, often, pyramidal model reminiscent of big industry. Small science, in its most extreme form as we know it, is practiced in Universities and academic institutions. Invariably, a member of the faculty works with a small group of students; quite often, each individual works on a separate research problem, the head of the group really acts as an academic mentor. Small science projects generally have limited goals, with the ambitions of the mentor and the students rarely rising beyond the publication of their results in journals they judge as superior. But, it is the hundreds, indeed thousands, of 'small science' laboratories across the world that train the scientists of tomorrow, many of whom will, in fact, graduate to the world of big and applicable science. Small science, where investigators can stray from the mainstream, is critical for the process of discovery; the lack of a rigid organizational structure is essential for many truly original individuals to be productive.

The purpose of my musings is really to worry about 'big science' and 'small science' in India. Well organized, governmental funding of science, particularly basic science, is only about a quarter century old. The science establishment has grown dramatically during this period; there are multiple funding agencies and an ever-increasing number of claimants for a limited pool of resources. The political perception of science has also changed; there is much less visible sympathy for the view that basic science needs to be nurtured steadily, in the long term national interest. Small science is clearly out of favour, despite the lip service that is sometimes paid to the importance of sustaining academic research and strengthening academic institutions.

Instead, there is a growing appreciation of the apparent virtues of 'big organized science'. The Department of Atomic Energy, the far flung laboratories of the Defence Research and Development Organization and the Indian Space Research Organization are the best examples of well-structured institutions capable of practicing big science. Their strategic objectives have, apparently, always been clearly defined and their financial requirements, generally, favourably viewed. But, big science is slowly beginning to creep into other institutions, which aim to tap the resources that seem earmarked for highly visible science. This drift towards the world of big money and even bigger promises is happening in many areas of science. For example, the change has been most evident in biology over the past few years. Several laboratories have jumped, with the greatest alacrity, on the genomics bandwagon. Suddenly, highly organized, structured research programs with well-formulated goals and highly desirable (but often unreachable) objectives have become fashionable, even in institutions, which in simpler times were once the preserve of small science. This uncomfortable transformation can, at times, lead to a form of institutional schizophrenia. The thrust for this drive to organize comes from the funding agencies; in biology the flagship is the Department of Biotechnology. There have also been other mechanisms for enforced coordination, like the New Millennium Fund program managed by the Council of Scientific and Industrial Research. Here, large sums of money are promised to conglomerates of researchers, often drawn from academia, government laboratories and industry. This fondness for big science may only reflect an evolution in our funding mechanisms; a drive by the agencies to harness the power of well directed organized research. But, in an age of limited resources and increasing demands it clearly represents a step away from traditional small science.

Does the growth of big science affect the practice of small science? There is an almost clear and evident prejudice against basic research, nowadays. Structured, applicable science promises so much that government agencies are often carried away by prospects rather than performance. In an interesting commentary Gregory Petsko remarks that it seems 'that most scientists are prone to believing – or perhaps hoping is a better word – that two and two can, sometimes, with the aid of the right technology, make five'. He cites three topical examples of this 'misguided faith': 'stealth technology' that allows aircraft to evade radar detection, 'encryption and authentication methods' to build totally secure computer systems and structural genomics, which hopes to obtain *useful* (and that is the important word) structural models of all proteins using a 'combination of experimental structure determination and comparative model building' (*Genome Biology*, 2001, **2**, 1009.1–1009.2). On paper, funding research that is directed towards solving all our most important problems appears politically attractive; even Parliamentary committees cannot fail to be impressed by a litany of good intentions. But, many 'big science' projects often attract a brand of managerial scientists, for whom grant money is a badge of honour; often grants accumulate upon grants, institutions buy equipment they do not need and little attention is paid to the problem of locating bench scientists to do the real work necessary to address chosen problems. Big science confers on its practitioners almost instant recognition; attracting money nowadays is far more important than taking a modest step forward in science. The result is that the votaries of big science soon spread like an unstoppable infection through the committee rooms of funding agencies; trampling the little that remains of our small science of academia. The agencies, enamoured by the prospects of quick technological fixes, promised by big science, have begun to downgrade individual investigator-driven research, by the quiet and effective process of administrative neglect.

Is 'small science' really small? Ironically, in many areas of science, the vision of individualistic science practiced in isolated laboratories as cheap and inexpensive is no longer really true. Even the most modest of our academic institutions require investments of the order of a few crores of rupees to provide the range of equipment that is necessary for modern research in physics, chemistry, biology and a host of other sciences. In the 1970s and 1980s the first steps in shoring up the academic research infrastructure were taken. But, in more recent times the focus has shifted. Replacing aging and obsolete instruments for basic research is hardly a priority; setting up centres for shared instrumentation is unpopular and talking about research without an immediate application is unfashionable. In this environment a new breed has appeared; the scientific buccaneers who harness the limited resources of government to build 'big science' laboratories. Much of this happens at the expense of 'small science', the traditional individual investigator-driven project which is the staple of academic research. The tensions between big science and small science are palpable; they are an inevitable product of our times. But it may help if government agencies adopted a more thoughtful attitude and worried about the relative merits of looking after the interests of its 'small science' constituents, as opposed to focussing only on 'big science'. Like the UTI they might eventually learn that, in the long run, it pays to nurture small investors and investments.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Lost innocence

'We may say that we are living on an island of guncotton. But, thank God, we have not yet found a match that will ignite it.'

– Walther Nernst (1921) quoted in *Brighter Than a Thousand Suns*, Robert Jungk, Penguin Books, 1960, p. 19

Every year in mid-July the Indian Academy of Sciences holds its mid-year meeting at Bangalore, invariably in the sylvan and peaceful campus of the Indian Institute of Science. At these meetings, there are several talks intended to disseminate science on a broad front, and there are others by newly elected Fellows and Associates, which give an opportunity to the greying members of the Academy to have a first-hand look at the new generation. Traditionally, the meetings are low-key, sedate affairs, with most lectures providing a level of scholarly detail that usually leaves the non-specialist mildly bewildered. The Academy, founded in 1934 by C. V. Raman, clearly laid out its charter in a 'memorandum of association'; its foremost objective being *'to promote the progress and uphold the cause of science, both in pure and applied branches'*. Some four decades after its founding and not long after the death of its founder, the Academy re-examined its role. In a clearly written statement the Academy noted: *'Science has emerged today as the most revolutionary co-operative activity of mankind. In the scientific method lies a validity and universality which transcends any other principle devised by man. Science seeks and discovers, cutting across sectarian, national and ideological frontiers. It fosters cooperation and generates a value system which nurtures man's highest capabilities and aspirations'*. Recognizing that in India a situation exists, *'where almost every facet of life needs to be revitalized, retaining a clear perspective for the nurturing of the scientific temper is of the utmost importance'*. The Academy committee also noted: *'In a country beset with prejudices, rules and bureaucratic formalisms, it is of the greatest value that working scientists, specially Fellows of the Academy, promote by personal as well as collective example, the principle of rational thought . . . . By applying rigorous standards of scientific criticism at all levels in a constructive sense, the Academy considers that the scientific community has a unique contribution to make not only to the flowering of science in India but also to national character.'*

Like all of India's growing number of science academies, the Indian Academy of Sciences too has generally maintained a low profile, rarely seeking to express a collective opinion on any issue of public consequence. Most often, this sphinx-like silence is the only sensible course of action, as opinions amongst its fellowship are, undoubtedly, widely disparate. The Academy's focus on science and its dissemination has served it well; the publication and educational activities ensuring that it is not hampered by the image of an exclusive country club.

This year the mid-year gathering began with an opening lecture intriguingly titled, 'The Scientific Objectives of Pokhran II'. Delivered with clarity and aplomb by the former Chairman of the Atomic Energy Commission, R. Chidambaram, the lecture was intended to describe the science and technology behind the nuclear blasts. The speaker has the rare distinction of being a key participant in both the Pokhran I and II experiments, separated by almost a quarter of a century; a period in which we have made a transition from the idea of a 'peaceful' nuclear explosion to a more pragmatic appreciation of the values of nuclear deterrence. A major issue that has developed in the aftermath of the events of May 1998, has been the estimation of the magnitudes of the explosions. There appear to be two sides to this apparently technical issue. The scientists from the BARC would like to place the nuclear explosion yields as close to that originally planned, as proof of the success of a long designed experiment. External observers, primarily Western, would like to estimate significantly lower yields, presumably to minimize the apparent 'success' of these experiments. Beyond the breast beating, there is also the problem of the Comprehensive Test Ban Treaty (CTBT) looming in the background; technology to detect small nuclear tests is desirable for some, while technology to facilitate underestimation of yields by distant observers may be desirable for others. Chidambaram's lecture outlined many of the pitfalls in yield estimates and provided a spirited defence against contradictory estimates published in the literature, including a recent article in this journal (Douglas *et al.*, *Curr. Sci.*, 2001, **81**, 72). Seismology and geology appeared to be the fields clouding the precision of the yield estimates. There were interesting sidelights presented by the speaker, including his description of the tendency of both the Americans and Soviets to over-estimate the yields of the other's tests at the height of the Cold War. This strategy provided an opportunity to claim that the opposing side was cheating on the provisions of

the 'Test Ban' treaties. But, in large part, there were frequent references in the lecture, to the kilotonnage of the blasts, with some comparisons to the magnitudes of the explosions with those that laid waste to Hiroshima and Nagasaki, in the cataclysmic, dying days of the Second World War. In the discussion that followed there was even some banter on the yield estimates; after all even 5 kilotons would leave the 'job' well done.

The lecture left me uneasy. It was undoubtedly well delivered, with a definitive air of technical authority, which held the attention of a full audience in the Faculty Hall of the Indian Institute of Science; a hall where I have over the years heard many wonderful expositions of science. The question which bothered me, however, was this: 'Did not the Indian Academy of Sciences find the topic incongruous for its gathering?' There is, presumably, little new physics to be gleaned from nuclear blasts and even if there were, this was not undoubtedly the purpose of the explosions. Clearly, the Pokhran blasts had many political and strategic motivations; their successful conclusion demonstrating that the enabling technologies work well in the hands of the establishment entrusted with the task. Science and its advancement are not even peripheral motivations and they need not be. Why then does an academy of science seek to clothe an experiment of great significance in the real and important world of geopolitics in the garb of science? Even in cases of controversies that lie in the domain of science or its borders, our Academies (probably, rightfully so) maintain a studied and pragmatic silence. Was it then necessary to suddenly move to a proactive stance and provide a public forum for a dispassionate overview of the power of an immensely destructive weapon and to rejoice in the presentation as an example of an indigenous scientific triumph? Would it not have been better for the Academy to limit its domain to conventional academic science and avoid straying into the difficult waters of strategic science and technology.

There has always been in the worldwide community of scientists a tendency to adopt 'an ostrich-like policy' on most politically sensitive matters. As early as 1947 James Franck analysed this failing: *'It is a custom in science – and perhaps a principle – to select from the infinite reservoir of unsolved problems only those simple ones the solution of which seem possible in terms of available knowledge and skills. We are trained to subject our results to the most severe criticism. Adherence to these two principles results in our knowing very little, but on the other hand being very certain that we know this very little. We scientists seem to be unable to apply these principles to the immensely complex problems of the political world and its social order. In general we are cautious and therefore tolerant and disinclined to accept total solutions. Our very objectivity prevents us from taking a strong stand in political differences, in which the right is never on one side. So we took the easiest way out and hid in our ivory tower. We felt that neither the good nor the evil applications were our responsibility.'* (quoted by Robert Jungk, *Brighter Than a Thousand Suns*, Penguin Books, 1960, p. 41). Robert Oppenheimer, the Hamlet of the nuclear age was troubled by the 'success' of the first experiments that he orchestrated, when he said in 1945: *'I think it is for us to accept it as a very grave crisis, to realize that these atomic weapons which we have started to make are very terrible, that they involve a change, that they are not just a slight modification: to accept this, and to accept with it the necessity for those transformations in the world which will make it possible to integrate those developments into human life.'* [Robert Oppenheimer, *Letters and Recollections* (eds Smith, A. K. and Weiner, C.), Harvard University Press, 1980, p. 318]. Even the end of the Cold War has not dimmed the intensity with which American arms programs are pursued, the nuclear missile defence systems and the space bomber projects illustrating the firm hold that the military–industrial complex has on governance. Ironically, in the first flush of excitement after Pokhran II, there were many reports on the importance of linking defence requirement with industrial development, even in India. There seems to be a more subdued tone now.

In this context the Academy's decision to celebrate Pokhran II and its scientific objectives, in an opening lecture at its recent meeting, is curiously inappropriate. The increasing association of science in India with government functionaries and the complete sidelining of academic science may not have found favour with the Academy's founding fathers. In reflecting on nuclear weapons and our collective fascination with them, I was reminded of Robert Southey's poem *After Blenheim*, which commemorates the carnage in the battle won by the Duke of Marlborough in 1704. Old Kasper describes to his grandchildren the famous victory:

'They say it was a shocking sight
After the field was won;
For many thousand bodies here
Lay rotting in the sun;
But things like that, you know, must be
After a famous victory.'

'Great praise the Duke of Marlboro won
And our good Prince Eugene.'
'Why, 'twas a very wicked thing!'
Said little Wilhelmine;
'Nay . . . nay . . . my little girl', quoth he,
'It was a famous victory.'

'And everybody praised the Duke
Who this great fight did win'.
'But what good came of it last?'
Quoth little Peterkin:-
'Why that I cannot tell', said he,
'But 'twas a famous victory'.

The Academy, like Wilhelmine and Peterkin, appears to be losing its innocence.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The ethical minefields of biomedical research

When Edward Jenner introduced the small pox vaccine he did not have to contend with an elaborate regulatory establishment. Pasteur's rabies vaccine and Ehrlich's chemotherapeutics also arrived in an era of limited controls. The exigencies of wartime saw penicillin reach the battlefield clinics in a remarkably short period. But over the years, particularly after the thalidomide tragedies of the 1960s, regulatory mechanisms for introducing new drugs for human use have become elaborate and expensive. In the United States, the Food and Drug Administration (FDA) acts as an alert watchdog, supervising the introduction of new pharmaceuticals after careful scrutiny of clinical trials, which are carried out in several phases. Indeed, the procedure by which a new drug wins approval for public use is long, arduous and costly. But for researchers in pharmaceutical laboratories the slow process of establishing clinical efficacy of a new drug can be a trying time. The growing controversy over the clinical trials carried out at the Regional Cancer Centre (RCC), Thiruvananthapuram, Kerala, of anticancer drugs, nor-dihydroguaiaretic acid (NDGA) and its tetra-*O*-methyl derivative (M4N) illustrates the cavalier attitude that prevails on the issues of international collaborations in biomedical research and the disregard for procedures when testing in human beings is undertaken. In this instance, a faculty member at Johns Hopkins University, Ru Chih Huang, appears to have made her own arrangements for clinical testing in India, without an ethical clearance from her University's Institutional Review Board. Johns Hopkins cannot, however, plead ignorance because the University has made payments for the trials to RCC. In India it appears that trials may have begun even before the formal approval of the Drugs Controller General's office. With the patients enrolled in the study coming from diverse educational and economic backgrounds, 'informed consent' for participating in the trial would hardly be meaningful. To further muddy the waters, the entire funding for the project in the US appears to have come from private sources; the involvement of a commercial organization invariably raises questions in such cases. While an enquiry has been instituted, it is clear that this incident should serve as a wake-up call to our biomedical establishment.

The real dangers involved in research with human subjects have been dramatically brought to the fore by the death of an apparently healthy volunteer, Ellen Roche, in a clinical study of asthma. This tragic incident, coincidentally at Johns Hopkins University, occurred following inhalation of hexamethonium bromide, a chemical that affects the nervous system, lowers blood pressure and relaxes airways. In the recent past a death has also occurred in a gene therapy trial intended to provide a means of treatment for an ornithine transcarbamylase deficiency. An editorial in *Lancet* (2001, **357**, 2067) points out that 'the whole idea of clinical trials is to ensure the safety of patients in general by confirming efficacy and safety of a treatment and by protecting them from exposure to unproven therapy'. In recognizing the dangers of bureaucratic overregulation, the *Lancet* essay concludes that 'fostering a culture of respect for the ethical conduct of trials should thus be part of any safety system'.

It is absolutely essential that a transparent, efficient and careful review mechanism for human trials be put in place by institutions, before clearances are sought from central authorities. At present, like most regulatory bodies, the Ethics committees in individual institutions and at ICMR lack credibility. They are also burdened by an inevitable sluggishness characteristic of most government bodies. Recent reports on trials of other 'cancer cures' on desperate patients, suggest that public perception of our ethics committees is far from encouraging. There is also an almost unrealistic eagerness for developing a clinically useful drug in many publicly funded laboratories, with only a limited appreciation of the long process of research and testing that is involved. Clinical trials in India pose another major ethical hazard. Very often the group of patients recruited for a study are hardly in a position to understand the uncertainties and risks of experimental treatments. It is therefore unsurprising that collaborative arrangements for human testing in India, of drugs and vaccines developed in the West are viewed with suspicion. It is

hard to avoid the legitimate fear, that poor and illiterate populations may be easily used to further foreign commercial interests. This fear is particularly justified when human clinical trials are not carried out in the country of origin of the drug, although this may not always be possible for diseases endemic only in the Third World. In these collaborations, the benefits, if any, may accrue largely to scientists and institutions. The biomedical research community therefore has a large public responsibility to ensure that it polices itself. At present the preoccupation of the Ministry of Social Justice and Welfare has been almost exclusively with regulation of experimentation on animals. Unfortunately, humans seem to have a lower priority.

But drug trials in humans is not the only ethical issue before the biomedical community. The dramatic advances that are taking place in the areas of stem cell research, xenotransplantation and gene therapies promise to throw up many new and difficult ethical questions. The rapidly transforming face of human genetics research, driven by the enormously powerful tools of genomics will pose several ethical conundrums. The treatment of many diseases of old age, Parkinson's disease amongst them, may benefit from embryonic neuron transplants. Clinical benefits have already been noted in patients, who are relatively young (Freed, C. R. *et al.*, *New Engl. J. Med.*, 2001, **344**, 710). Eventually, the use of human embryonic stem cells may power the next great advance in enhancing human health. But, the use of 'spare embryos' generated in the course of *in vitro* fertilization procedures finds many opponents, the world over. The growing crescendo of controversy over the issues of reproductive and therapeutic cloning is not likely to die down very soon. Clearly, the fruits of the breathtaking advances in modern biology can be savoured only after we have negotiated the ethical minefields of biomedical research.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Awards and rewards

Sometimes the past suddenly intrudes upon us. So it was, when the controversy over the Arjuna award to Milkha Singh reminded me of another day and another age. The late 1950s and the first couple of years of the decade of the sixties were a time of hope and optimism. Independent India was still young and as a boy I was thrilled to hear of Milkha Singh's glorious quarter mile run across Rome's Olympic Stadium in 1960. It was a time of the radio and the newsreel that preceded a film in the cinema theatres. It was a time when we still thought of the hockey gold in the Olympics and Ramanathan Krishnan could even finesse his way into a Wimbledon singles semi-final. Cricket was not the all-consuming force that it is today. Gavaskar and Kapil Dev were still to appear on the horizon; Tendulkar was not born. It was a time when a new world order appeared possible; John Kennedy's assumption of the US presidency promised a future that was to turn into a mirage. The disastrous border war with China lay ahead and the demoralizing food shortages of the mid-1960s were yet to be anticipated. The green revolution and Mrs Gandhi had not yet entered our lives. It was in this ambience that Milkha Singh raised the bar of sporting achievement by sprinting to within a tenth of a second of an Olympic medal, breaking the existing record in the course of his spectacular run. Why did such a man not receive this country's highest national award for achievement in sport, even when it was first instituted? Why did the committees in the Sports Ministry suddenly rediscover Milkha Singh? In the long years since the Rome Olympics there have been many sportsmen and women honoured for far lesser achievements. Milkha Singh's instinctive reaction of rejecting the award, highlighted the infirmities of the entire selection process, sparking a debate on the mechanisms by which we acknowledge men and women of distinction in sport. Are awards important? Undoubtedly they are; particularly in a directly competitive sphere of activity like sport. Was Milkha Singh a stray omission? Obviously not, since there have been many plaintive cries in the past few weeks from once famous names.

Inevitably, pondering on the issue of awards and recognition, I drifted to thinking about science. This is the right season to worry about awards. The annual Bhatnagar awards, instituted in the late 1950s by the Council of Scientific and Industrial Research (CSIR) will be given away even before this is read; this year's selections will be announced by month's end. These prizes are intended to recognize achievement in all branches of science, engineering and medicine, with the important proviso that the awards are restricted only to those researchers who have yet to reach the age of 45. Instituted in 1958 by CSIR, to honour the memory of its founder Shanti Swarup Bhatnagar, the awards were unfettered by age in the early years. In the inaugural year, K. S. Krishnan, then a mature 60 years, was a recipient, but in a few years, the Bhatnagar prizes were strictly limited to relatively young or middle-aged achievers; clearly-late bloomers were sharply excluded from the circle of recognition. The roll call of honour in the early years was impressive, some of the most famous names in Indian science amongst them. Interestingly, age has always been an important parameter in deciding limitations on awards, an issue that has been previously discussed in these columns (*Curr. Sci.*, 1999, **76**, 1059). Bodies which institute awards, often, intend to use recognition as a means of encouragement; in the hope that celebration of promise may pay more dividends in the long run, than acknowledgement of performance. Thus, there are a plethora of 'young scientist' awards with varying age limits; 32 for the Indian National Science Academy (INSA), 35 for CSIR (which is restricted to 'in-house' scientists) and 40 for the B.M. Birla Foundation. By definition, the young scientist awards are 'talent spotting' exercises, with relatively few high achievers recognized for actual performance. But, there is often, a cascading effect, with 'promise' being persistently recognized, before fading into middle-aged oblivion. Early awards can also provide a route up an administrative ladder. Late entrants to the playing fields of science and quiet plodders, who suddenly bloom late, are completely left out of the awards network. There are a few prizes which are given without explicit age restrictions; but selection committees are unlikely to be swayed by a candidate who has been previously unrecognized. Truly, one award begets another.

In recent times, awards have proliferated. Many private bodies have sprung up which honour scientists. Government departments vie with one another in instituting high profile awards. The millennium's beginning (or end) and the fiftieth anniversary of our independence provided ample excuse. The Swarnajayanthi Fellowships instituted four years ago targetted the scientist population below the age of 40. This initiative attracted considerable attention largely because a handsome research grant was supplemented by an even handsomer monthly fellowship of Rs 25,000 as a bonus on top of a regular salary, making it the most lucrative of our awards. Understandably, the scheme met with a fair share of criticism, even as the search for 'world class' performers slowly began to peter out. There has been little input into the Indian scientific scene in recent years. Institutions hire relatively few people and many are already in their mid to late 30s. Even before they settle down they are in fact out of the 'awards net'. But, in keeping with the mood of the times the Department of Biotechnology also introduced bioscience awards carrying a cash prize aimed at a similar age group. In the inter-agency competition the value of awards has steadily been enhanced, with the Bhatnagar awards escalating by about an order of magnitude over a decade, reaching a respectable (and desirable) figure of Rs 2 lakhs.

While cash awards are welcomed by the scientists who receive them, peer recognition is also conferred by Academies, and we have many of them. Science, in its broadest sense, is represented by as many as three academies based in Delhi, Bangalore and Allahabad. Any external observer is bewildered by this surfeit of academies; but, history prevails over common sense as each of these bodies grows in strength. The annual election of 'fellows' is an elaborate process and those who are outside the academy are often desirous of getting in. Here too, for an unfathomable reason, age is an important criterion. There are again two distinct schools of thought; those who believe recognition is an important tool in encouraging scientists to raise performance levels and a more conservative school which holds that elections must be a consequence of achievement. There is also a fond hope in the Councils of these bodies that reducing the average age of their fellowships might invigorate their organizations. Even at the academies, the late bloomers are ignored; the dominant prejudice is that success in science is unlikely after middle age.

But, for those who pick up their share of honours, careers end poses another challenge; a scramble for the limited number of emeritus scientist and professorship schemes, which allow a life in science past the mandatory age of retirement. Here the competition stiffens because of a diminishing number of opportunities. The conversion of the Indian National Science Academy's emeritus scientist program, from one which paid an honorarium to one which is purely honorary has made it unattractive for many; a clear case of the importance of the 'cash award'.

The most contentious issue with awards of all types is, of course, the decision making. Judgements must necessarily be subjective. In the case of science, research publications are often, the only available parameter. The rise of the insidious 'journal impact factor' has introduced a seductive, but misleading quantitative element, into the process of judgement. Scientists now highlight 'average impact factors' on their lists of publications, little realizing that their papers in high impact journals may in fact be cited much less than the journal average. Even within a broad field of science like biology, the impact factors of (and perceptions about) journals vary widely with subdisciplines. It is hardly possible to compare journals in plant physiology with those in neurobiology. Some disciplines remain Cinderellas waiting for a fairy godmother. While most selection committees struggle under the burden of a difficult task, correct judgements hardly cause a ripple, glaring acts of omission or commission can raise a storm. Patronage, prejudice and pressures are inevitably a part of the process. The awards and rewards system in science is determined by complex dynamics. Our Milkha Singhs may one day suddenly appear on the scene.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The seeds of terror

'Civilization is a stream with banks. The stream is sometimes filled with blood from people killing, stealing, shooting and doing the things which historians usually record, while on the banks, unnoticed, people build homes, make love, raise children, sing songs, write poetry, and even whittle statues. The story of civilization is the story of what happened on the banks. Historians are pessimistic because they ignore the banks for the river'. This definition of civilization by Will Durant, who coauthored with his wife Ariel the remarkable *Story of Civilization*, is particularly pertinent at present. When the history of the first years of the new century is written, the carnage in New York and Washington and its inevitable aftermath, which we still await, will undoubtedly appear on the pages of history books. In the space of just over an hour we have witnessed a spectacular leap in the 'technology of terror'. By the simple device of infiltrating suicidal pilots into commercial airliners, the familiar act of hijacking, has transformed a plane full of passengers (and fuel) into a guided missile capable of wreaking death and destruction even in the heart of a country, whose airspace is jealously guarded. Every recent advance of communications technology, the Internet and the cellphone among them, have quickly been adapted to facilitate the spread of terrorism. Many of the technologies that have now become a part of civilian life were indeed developed in a military context, with either offence or defence in mind. Unsurprisingly, the enhanced technological sophistication of our everyday life has been mirrored by the growing escalation of the scale and reach of terrorist attacks. While history, religion, nationalism and politics lay the seeds of terrorism, technology (and, inevitably, science) provides access to the means of death and destruction.

The 'fruits' of modern science, poison gases and high explosives, first appeared on the battlefields of the early 20th century. Germany under the Nazis raised the level of application of science in the act of mass murder to a new high, by systematically using hydrogen cyanide (generated from the infamous Zyklon B capsule) to exterminate millions of Jews in concentration camps across central Europe. The recent, belated apology by the German scientific establishment for the ghastly excesses of scientists under the Nazis is an acknowledgement that scientists of the era participated, sometimes enthusiastically so, in the madness of the times. The Second World War came to an abrupt and immeasurably bloody end, when the discovery of nuclear fission was quickly transformed into an atomic bomb. Hiroshima and Nagasaki were instantly erased and immortalized in the history books. In the years since, the battlegrounds of Vietnam proved a testing ground for many terrible technologies. Most notably, there was the extensive use of Agent Orange by the United States, on the jungle and peoples of Vietnam; the purpose was to defoliate the forests and deprive enemy of cover. The use of the herbicide, belonging to the dioxin family, has had devastating long term effects on people. Crippled and deformed human beings remind us of another time of madness three decades ago. The spectre of the use of chemicals in modern warfare loomed briefly in recent times, during the Gulf War. But, the lessons of history rarely make an impact. The developed countries of the world, led by the United States, have made a virtue of developing the technologies that make nuclear, chemical and biological weapons possible. Inevitably, the poorer countries have made the acquisition of the technologies of destruction a priority. We have thus reached a stage where the weapons of destruction are becoming very widely available and it does not require a strategic analyst to conclude that isolated groups of terrorists will soon have their hands on weapons that can be used on 'high value targets'. The phrases 'terrorist state' and 'state-sponsored terrorism' are being widely used and seem to mean different things to different people.

In this world haunted by terror and counter-terror, reports that 'US military advisers would like to rewrite the treaties banning chemical and biological weapons' are alarming. Their purpose, ostensibly, is to develop 'non-lethal weapons; for example, gases that 'calm crowds rather than kill them' (Edwards, R., *New Scientist*, 16 December 2000, p. 4). Rewriting the existing conventions on biological weapons (1972) or chemical

weapons (1993), in order to legalize development of new military technologies threatens to open a Pandora's box; undoubtedly today's strategic weapons will be used by tomorrow's terrorists. Research in some areas of biology is also raising questions of concern. The development of an extraordinarily lethal, engineered 'mousepox' virus, created ironically enough in research aimed at developing a contraceptive vaccine, has raised the possibility of creating new viruses that will prove lethal to humans. Serendipity, may sometimes be a mixed blessing. A new dimension in the scenarios for biological warfare clearly looms on the horizon (Nowak, R., *New Scientist*, 13 January 2001, p. 4). Enough destructive technologies exist and more will be dreamt up, some by design others by accident, even as the world wrestles with the discomforting realization that every valuable technological advance may prove to be a double-edged sword.

Technological progress has also widened disparities between the rich and poor countries. In India, excessive consumption by those who have is creating an unbridgeable gulf between the rich and the poor. Even as the disparities increase, so do the tensions. The world has not been helped when some of the poorest countries of the world have been used as the staging ground for demonstrating the reach of modern weaponry and the power of Western states. The world has been ill served when religion has been used as a weapon, carefully cultivated when it is directed towards one side but abhorred when it attacks the hand that has fed it. We live in a world that appears to be under a unique threat, created by the marriage of technological sophistication and fanaticism, fuelled by religion and ideology. The world will become even more dangerous as the richest countries attempt to use economic and military power to create an order which has only one purpose – maintenance of a society whose abilities to consume are matched only by their inability to recognize the growing tension in the world around them. Every international agreement ranging from those on controlling green house gas emissions to those relating to trade barriers and intellectual property rights perpetually face the threat of an American veto. Any agreements that seek to limit, contain and destroy stockpiles of chemical, biological and nuclear weapons lie under the some cloud. If the world is a dangerous place today, it is because the seeds of terrorism were sown in the past and the tree of terror has been painstakingly nourished over the decades. In this technologically sophisticated world the Newtonian dictum that every action has an equal and opposite reaction is followed in the world of geopolitics; indeed, unlike in mechanics, the reactions in the real world are often disproportionate in their magnitude. The Gandhian adage that if 'an eye for an eye' was an acceptable response we would all soon be blind, has generally been forgotten. But, it is important to learn some lessons from the events in New York and Washington and all that has gone before. Otherwise, the thousands of innocents who have died, would have died in vain.

P. Balaram

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EDITORIAL

Macaulay's children

I wish I were as sure of anything as Macaulay is of everything.

William Windham

English has been the language of choice for higher education in India. For the sciences, medicine, engineering and a host of other disciplines, this has, generally, been perceived as an advantage. Indian graduates gain easy acceptance internationally, particularly at a time when English has become the dominant language of science and technology. In the last few years, even European journals have succumbed to growing pressures, switching over to publishing in English. Russian, Japanese and Chinese scientific journals command little influence in the world of science, when they are not published in English. The British Empire may have vanished into the pages of the history books, but English remains an enduring and sometimes controversial legacy. There have been several educational commissions and reviews of national education policy, in the years since independence, which have emphasized the importance of instruction in regional languages, often echoing Rabindranath Tagore and Mahatma Gandhi and many others who went before them. But academic discussion apart, little has happened in seriously promoting any Indian language to a level, where they might appear as a serious and locally relevant alternative to English, in colleges and universities. Even at school level, English as a medium of instruction provides a major advantage to children, as they move into higher classes. But, English has its share of detractors, who believe that creativity and originality can hardly find expression in a language other than the mother tongue. This school of thought strongly argues that the turning point really came in 1835, when Macaulay wrote his famous 'Minute on Education'. Indeed, to those who believe that overemphasis of English and concomitant neglect of our languages has caused irreparable harm to Indian education, many of us are 'Macaulay's children'; a mildly derogatory description applied to those who are more comfortable with English, than any Indian language. A letter in this issue of *Current Science* describes the many remarkable medical insights in the ancient manuscript, *Bhrigu-Samhita*, and argues that science is indeed 'culturally conditioned', suggesting as a corollary that the universality of science may in fact be a myth. The writer laments that 'we still continue to be Lord Macaulay's educational products' (A. D. B. Vaidya, *Curr. Sci.*, 2001, **81**, 735).

Who was Lord Macaulay? When Hastings, Wellesley, Cornwallis and Curzon have faded into the dim past of history, even the boulevards bearing their names in Delhi have been rechristened, why does Macaulay, a man who held no high office and commanded no armies, surface so frequently in our laments on the state of education in India. Thomas Babington Macaulay (1800–1859) came to India in 1834 to be a legislative member of the Council of India; according to biographical notes his intention was to 'achieve financial independence'. He stayed only till 1838, but in writing his famous 'minute', a year after his arrival, he ensured that he would be with us even a century and a half later. He was appointed President of the Council of Education by the then Governor General William Bentinck, who finds a place in history books for his assault on 'The Thugs' and his decisive step in abolishing Sati. The problem before Macaulay was clear and he stated the issue simply: 'We have a fund to be employed as government shall direct for the intellectual improvement of the people of this country. The simple question is, what is the most useful way of employing it?' Which language should be supported as the medium of instruction? The Council of Education was then divided: Orientalists on the one hand favoured Sanskrit, Persian or Arabic, classical traditional languages which were, however, not widely spoken, while Anglicists were clear that English would prove superior. Raja Rammohan Roy's reformist movement had already taken root in Bengal, its founder arguing forcefully in favour of English; indeed establishing an English school in Calcutta in 1817, nearly two decades before Macaulay wrote his 'minute'.

Macaulay confronted the Orientalists on the committee with many overstated and sometimes offensively worded sentences. His 'minute' has often been selectively quoted to highlight statements which appear to epitomise the arrogance of a colonising power. He states infamously: 'I

am quite ready to take the Oriental learning at the valuation of the Orientalists themselves. I have never found one among them who could deny that a single shelf of a good European library was worth the whole native literature of India and Arabia.' Much later in his 'minute', Macaulay makes his most widely quoted proposition: 'We must at present do our best to form a class who may be interpreters between us and the millions whom we govern, a class of persons, Indian in blood and colour, but English in taste, in opinions, in morals and in intellect.' These two extracts of Macaulay's 'minute' have been held, for much of the 20th century, as being the root cause for many of the ills that affect our educational system, including the ever-increasing divide between the institutions of higher education and the mass of people who may aspire to a better future. But, in making Macaulay a favourite whipping boy most analysts use a common journalistic strategem, the technique of selective quotation. Reading the text of Macaulay's 'minute' (cf. Edwards, M., *British India 1772–1947*, Rupa Press, New Delhi, 1967) is instructive, for it reveals the thinking of man, dispassionately analysing a problem with which he was charged. Realizing that the commonly spoken languages in the areas under British control may be unsuitable for higher education, Macaulay faced limited choices; Sanskrit and Arabic on the one hand and English on the other. Indeed, none of the languages considered were commonly spoken at that time. But in proposing the creation of an intermediate English speaking class Macaulay had a purpose: 'To that class we may leave it to refine the vernacular dialects of the country, to enrich those dialects with terms of science borrowed from the Western nomenclature and to render them by degrees fit vehicles for conveying knowledge to the great mass of the population'. In proposing the abolition of the 'Madrassa and Sanskrit College at Calcutta' and the retention of the 'Sanskrit College at Benares and the Mohamedan College at Delhi' Macaulay felt that enough was being done 'for the Eastern languages'. But he did propose that 'no stipend shall be given to any students who may hereafter repair tither, but that the people shall be left to make their own choice between the rival systems of education without being bribed by us to learn what they have no desire to know'. Macaulay was far sighted in many ways. In a remarkable speech in the British Parliament on the Government of India Bill in 1833, he had this to say: 'Are we to keep the people of India ignorant in order that we may keep them submissive? Or do we think that we can give them knowledge without awakening ambition? Or do we mean to awaken ambition and provide it with no legitimate vent? Who will answer any of these questions in the affirmative? Yet one of them must be answered in the affirmative, by every person who maintains that we ought permanently to exclude the natives from high office. I have no fears. The path of duty is plain before us; and it is also the path of wisdom, of national prosperity, of national honour.'

Reading Macaulay, I could not help but wonder at the free fall of our educational system over the years, with little by way of purposeful intervention. Should it not have been possible to generate an equally influential 'minute' which would redirect the sprawling system of school and university education? Or is it that we have collectively lacked the tenacity of purpose to confront the problems of our times? Or is it simply just wise to retain our present system and confine ourselves to occasionally deriding Macaulay's children?

P. Balaram

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EDITORIAL

Science and the Third World

More than three-quarters of the world's population lives in the Third World; a loose, depressing description of a large, diverse group of countries, which seem to be interminably involved in a futile struggle against the crushing burdens of poverty, disease, strife and increasing populations. Alfred Sauvy, a French demographer is credited with coining the phrase 'tiers monde' in 1952, presumably following the pre-revolution description in France of commoners as the 'third estate'. In France of the Bourbons, priests and nobles constituted the first and second estates. In our times the term First World describes the economically advanced countries of Western Europe and North America and Japan and Australia. The Second World was always a shadowy, imprecisely defined term, presumably encompassing the erstwhile Soviet Union and its East European satellites, countries with apparently impressive industrial and military strengths. But, in the aftermath of 'glasnost' and 'perestroika' the Second World seems to have disappeared, its constituents seamlessly integrating into the First and Third Worlds. Russia appears to be reclaiming a Western identity; East Germany has entered the realm of plenty, with reunification. In the aftermath of the declaration of the 'war on terrorism', television images suggest that Tajikistan, Turkmenistan and Uzbekistan may well qualify to join the swelling ranks of the Third World, which now cuts a broad swathe through Asia, Africa and Latin America.

In their drive towards development the larger countries of the Third World, China, India and Brazil are examples, have recognized the critical importance of science and technology; devoting the years since the 1950s to building up a significant infrastructure for research. But, research in modern science can be a lonely and sometimes unreal occupation in the poorest countries of the Third World. In a remarkably far-sighted move, the Pakistani physicist Abdus Salam realized the utility of forging collaborative interactions between Third World scientists and the importance of generating a transnational body 'to act as a pressure group to persuade Governments of the South to make the necessary commitments to scientific research'. Salam's vision led to the birth of the Third World Academy of Sciences (TWAS) in 1981, which today has 480 Fellows from 62 developing countries. Twenty years after its founding, TWAS was scheduled to hold its 8th General Conference in New Delhi, beginning 27 October 2001. This issue of *Current Science* was intended to provide some images of science in the Third World and to welcome our colleagues to this country.

Unfortunately, the political problems of the Third World have overtaken the conference, leading to its postponement. Afghanistan, one of the poorest countries in the world, is now subject to the might of modern, military technology, ostensibly to strike at the heart of international terrorist organizations. Its impoverished citizens are being driven into refugee camps, from which the next generation of terrorists must inevitably arise. Afghanistan does not have a single member in TWAS, an indicator that there are still countries in the world, where the practice of science has to take root. Afghanistan is a stark reminder of the bottomless pit into which peoples and nations can sink, their slide promoted by the political imperatives of the First World. For the countries of the Third World, the path to development is tortuous and the obstacles formidable. Many countries have been very poorly served by their leaders, who have been propped up by First World countries, which generally view every international problem through the distorting prism of self interest.

In a world being driven, relentlessly by the West, towards a market economy, it has become imperative that Third World countries learn to stand on their own feet, strong enough to stem the tide of globalization. The forced liberalization of Third World economies, dictated by the World Bank and the International Monetary fund, is all set to create almost unacceptable inequalities, within the developing countries. The World Trade Organization (WTO) is ready to join the ranks of the forces that dictate Third World policies with scant regard for local imperatives. Almost a quarter century ago Mahbub Ul Haq delivered a judgement, which the Third World might well worry about: 'Markets...enrich the rich and pauperize the poor'. Other analysts

have also been very pessimistic, particularly of the slowdown in progress in the last twenty years: 'The Third World has had 40 years of development and things are not getting better...time after time development seems simply to modernize poverty at huge environmental cost' (Mayo, E., *New Economies*, 1993, **27**, 7). This haunting conclusion might well be applicable to our many developmental projects, which have turned controversial, the Narmada dam amongst them. The strategists, who plan development in the Third World, must walk a tightrope to ensure that the path to progress does not cause irreparable environmental and societal damage. Both 'sustainable development' and 'sustainable consumption' are set to become the key phrases in discussions of Third World development.

Science and technology are, of course, central to the accepted routes to development. In an essay, a few years ago, José Goldemberg, of the University of Sao Paulo, Brazil, asked the key questions: 'What is the role of science in developing countries?' (*Science*, 1998, **279**, 1140). Writing in the aftermath of the Indian and Pakistani nuclear explosions, he concluded, 'that development does not necessarily coincide with the possession of nuclear weapons or the ability to launch satellites'. In searching for a prescription for developing countries, Goldemberg argues that 'we...should not expect to follow the research model that led to the scientific enterprise in the United States and elsewhere'. In the most developed countries of the Third World, China, India and Brazil amongst them, a large and well developed science infrastructure has been built, over the past half century. There are clear and positive signs that the science and technology establishments in these countries are indeed gearing up to hasten the pace of progress and to face the challenges of a competitive, international marketplace. But, there are countries in the Third World, many in Africa, where the practice of science and access to modern technology is still extremely limited. Mohamed Hasan, President of the African Academy of Sciences puts it bluntly: 'The truth is that stark disparities exist not only between Africa and the rest of the world, but between Africa and the rest of the developing world. What accounts for Africa's impoverished state? There are many political, socioeconomic and environmental factors. Centuries of foreign colonialism followed by decades of home grown authoritarian governments. A lack of transparency in economic transactions often accompanied by mismanagement. Unsustainable use of natural resources. Marginal participation in the global economy. However, there is another factor that may not be as visible or dramatic as those mentioned above but that may nevertheless play a central role in the continent's inability to participate in the global economy, protect its environment and devise sustainable strategies for economic growth. That factor is Africa's woeful shortcomings in science and technology.' (*TWAS Newslett.*, 2001, **13**, 4.)

The growth of the Third World Academy of Sciences provides a wonderful opportunity for collaboration and cooperation between scientists and their institutions in developing countries, spread across the globe. Working together on the many common problems that Third World countries face, scientists could become the frontline in promoting greater harmony, facilitating a purposeful attack on the formidable developmental issues faced by the poor countries. Thus far, the Third World has been ill-served by politics, religion and national, tribal and ethnic pride, all of which have largely promoted internal strife, external conflict, destruction and death. History hangs heavily upon the Third World; the past often providing a compelling rationale for the inequities of the present. But, it is time to look ahead. Cooperative science is a laudable human endeavour and may indeed help the Third World and its peoples move towards a better future.

P. Balaram

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EDITORIAL

Science and Society: Needham, Bernal and Pauling

Calcutta lost its status as capital of British India ninety years ago, when the seat of imperial power moved to Delhi. In the span of a century from Rabindranath Tagore to Satyajit Ray and Mother Teresa, Calcutta has been home to both the highest levels of human achievement and the lowest depths of human despair. Calcutta (or Kolkata as it is now known) has been the capital of science in India through much of the 20th century; its most glorious moments falling in the last century, when Jagadis Chandra Bose, C. V. Raman, Meghnad Saha and Satyendranath Bose raised Indian science to a height, which has since remained unmatched. Even earlier, P. C. Ray's pathbreaking research in chemistry was a sign that Calcutta might prove hospitable to a fledgeling science. Two of India's most far-sighted institution builders. Mahendra Lal Sircar and Asutosh Mukherjee called the city their own. Many other remarkable men and women of science have emerged in the years since; sadly, in the last three decades the lustre of Kolkata's science institutions has dimmed, even as they struggle against the tide of declining intellectual standards, diminishing resources and a deteriorating academic infrastructure. However, through good times and bad, Kolkata has provided a home to some of the most important intellectual and political movements of our times. Ramachandra Guha, a sociologist and writer who for a while called the city his home, passes a perceptive judgement: 'In so far as thought and debate can flourish only in conditions of political freedom, it is Calcutta rather than Peking, Hanoi or Havana, which is the intellectual centre of world Marxism' (*An Anthropologist Amongst the Marxists*, Permanent Black, 2001, p. 10).

I was therefore only mildly surprised to receive an invitation to a national seminar on 'Science, Scientists and Society' to be held in Kolkata. The occasion was to mark the birth centenaries of three remarkable Western scientists, all of whom entered the world in the period 1900–1901; John Desmond Bernal, Joseph Needham and Linus Pauling. These men were at the peak of their intellectual powers at a time when the West was undergoing a remarkable period of political ferment, in the years following the rise of Adolf Hitler in Germany. Rarely has there been an ideology as opposed to rational enquiry as Nazism; unsurprisingly, many of the most talented Western scientists of the times drifted leftward, beckoned by the seductive ideology of Marx, little realizing that the Soviet Union under Stalin would be as repressive and intolerant of rational thought as Germany under the Nazis. Bernal and Needham were products of a unique intellectual environment in Cambridge in the 1920s. In an appreciation of Needham, Mansel Davies says: 'The Needham peak of enlightenment did not rise from a plain. Fujiyama is unusual in rising almost alone from the greatest of plains, the sea. Mount Shastra, the Matterhorn and K2 are far more typical in being surrounded by other notable peaks. Joseph Needham is very much a Cambridge man. During his formative twenties and thirties there were contemporary with him at Cambridge (to mention only those in science) J. J. Thomson, Rutherford, Eddington, Keynes, Adrian and Sherrington. To say that these were in the Nobel class is an understatement. The Nobel class in Cambridge over those years comprised some ten others, from Blackett and Bragg to C. T. R. Wilson. But Needham himself would almost certainly mention in this context, his mentor, Gowland Hopkins, and his colleague and friend the physicist J. D. Bernal.' In thinking about the Kolkata seminar that was intended to honour these remarkable men of 20th century science, I realized that I knew a little about Pauling and Bernal, men who contributed greatly to fields I have studied; but of Needham I was ignorant, except for his legendary fame as a scholar of 'science and civilization in China'.

In searching for a quick overview of Needham's life and times I turned to the marvellous compilation of *A Selection from the Writings of Joseph Needham* put together by Mansel Davies (The Book Guild Limited, 1990). Here one can get a glimpse of Needham's far-ranging mind, without having to contend with the formidable size and scholarship of his work on science in China. In the years between 1920 and 1942 Needham was a biochemist of the highest order, before he began an almost half a century association with China. Needham's writings on science, philosophy and religion began even during his days in The Cambridge Biochemical Laboratory,

but these were in his own words, 'by-products, the results of evening reading or time for extraneous study snatched while waiting for the completion of a distillation or an incubation'. Needham's luminous scholarship shines through an essay based on an annual lecture given in 1963 in the chapel of Gonville and Caius College, Cambridge of which he was Master. Needham reminds the college's students that, 'it is good to remember, therefore, that our own pious founders were not the only men, and that Christendom was not the only culture, to set on foot great and noble institutions of learning where successive generations of students assembled to get the benefit of education and research. When the men of Alexander the Great came to Taxila in India in the fourth century B.C. they found a university there the like of which had not then been seen in Greece . . . and was still existing when the Chinese pilgrim Fa-Hsien went there about A.D. 400. Later the torch of learning moved to Buddhist Nalanda in Bihar, as we know from the account of that other great pilgrim Hsüang-Chuang in the seventh century. In China the foundation of the Imperial University goes back to 165 B.C. and by the beginning of the Christian era it had no less than 3,000 students'. In turning to the arts and sciences of Arabic culture, Needham reminds his listeners of a provocative Islamic saying, 'the ink of science is more precious than the blood of martyrs'. Needham goes on to ask: 'How are we to look upon all these achievements of people who were neither British nor European, neither Christian, nor 'white'? Surely, it can only be with appreciation, indeed with admiration. Truly 'God is no respector of persons'. Today, at a time when international political tensions are intermingled with racial factors, it is more than ever essential that we approach people of other cultures with the conviction that they have at least as much to give us as we have to give to them.'

Desmond Bernal had an unmatched record as a mentor. Dorothy Hodgkin, Max Perutz and Maurice Wilkins, all of whom were associated with him, went on to win Nobel prizes. The application of X-ray diffraction to the study of protein crystals began in Bernal's laboratory. But while the most remarkable advances in structural biology, including the determination of the structure of DNA, were made in England during the three decades from the 1930s to the 1960s, Bernal himself appeared more as a motivator; a man generous with his ideas, whose concerns and interests transcended a narrow commitment to laboratory science. Bernal was an unabashed Marxist in the years before World War II. In the aftermath of Hiroshima and Nagasaki he turned his immense energies and penetrating intellect to the promotion of peace and nuclear disarmament. Bernal was also a major contributor to the issue of assessing a role for science in the civil economy. Indeed, Bernal was at the forefront of discussions in Britain in establishing 'science policy and technology policy as an important issue of public debate and government intervention'. In an assessment of Bernal, C. P. Snow notes: 'People have asked, just how will he rank in scientific history in the narrow sense. I think the answer is that in natural gifts he stands very high; he is the most learned scientist of his time . . .. But he has suffered from a certain lack of the obsessiveness which most scientists possess and which makes them want to carry out a piece of creative work to the end.'

We turn finally to Linus Pauling. Far removed from the intellectual ambience of England and Europe, Pauling epitomized the emergence of the New World. His contact with the centres of quantum physics in Europe during a brief period, 1926–27, was catalytic. He was to subsequently transform chemistry in a manner that was unanticipated by Dirac when he made his famous comment that 'quantum mechanics explains much of physics and all of chemistry'. Pauling could also legitimately claim that molecular biology was born in his laboratory in Pasadena and even before the century had reached its half-way mark, he had established the connection between molecular structure and disease. The Nobel Prize for chemistry was his in 1954 and possibly his greatest honour came posthumously, when members of the American Chemical Society voted him as the most influential chemist of the 20th century. But, the seminar in Calcutta really pays tribute not to Pauling's science but his unyielding commitment to the cause of peace. His pacifism must be seen in the context of the times. Pauling used his immense scientific stature to espouse the unpopular cause of banning nuclear weapons, immediately after the war, and spoke loudly and clearly even at the height of the anti-Communist movement in the United States, led by Senator Joseph McCarthy. Indeed, Pauling's 1962 Nobel Peace Prize was dubbed 'an extraordinary insult to America' by *Life* magazine.

In considering the contributions of Needham, Bernal and Pauling to the causes of peace, understanding and application of science to human betterment, we touch upon a subject that is as important today as it was in their times. The weapons of war and destruction are even more powerful and more widely dispersed today, than a couple of decades ago. Historical conflicts, religious intolerance and economic exploitation have been nurtured over the years. The world remains a dangerous and difficult place. But, there is hope that out of the ferment of the new century will emerge the successors of Needham, Bernal and Pauling.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science and the ascent of man

Man is distinguished from other animals by his imaginative gifts. He makes plans, inventions, new discoveries by putting different talents together; and his discoveries become more subtle and penetrating as he learns to combine his talents in more complex and intimate ways. So the great discoveries of different ages and different cultures, in science, in the arts, express in their progression a richer and more intricate conjunction of human faculties, an ascending trellis of his gifts... .

In every age there is a turning point, a new way of seeing and asserting the coherence of the world... . Each culture tries to fix its visionary moment, when it was transformed by a new conception either of nature or of man. But in retrospect, what commands our attention as much are the continuities – the thoughts that run or recur from one civilization to another.

— Jacob Bronowski

Books make wonderful gifts; they are rarely discarded, even if unread. Following a conviction, that is becoming increasingly difficult to uphold, that books are amongst the most lasting of presents, I searched many years ago for a gift, in an unprepossessing local bookstore. The intended recipient was a favourite niece, then on the verge of her teens. Carelessly placed on a table intended for books on sale, I stumbled upon Jacob Bronowski's *The Ascent of Man* (British Broadcasting Corporation and Little Brown and Company, USA, 1973), a magnificently illustrated script of the famous BBC TV series. Here was a book I would have loved to possess; but priced as it was it seemed more legitimate to buy it as a gift. Some fifteen years later after my niece, like many others of her generation had left for the United States, I found Bronowski's book lying forlorn, mildly worn and probably untouched by its owner in a forgotten cupboard. Somewhat guiltily I reclaimed it, the smudged, fading inscription on its cover reminding me that I was indeed guilty of petty larceny. As an act of repentance, Bronowski's tale may be worth recalling in these columns, for it is a story in which science and human endeavour are intertwined in a saga that spans the millennia.

Biology rests on the firm Darwinian foundations of shared evolutionary history of living organisms, common ancestry, adaptation and natural selection. All that has happened since Darwin, has only reinforced the essential unity of biology; today's thrust in comparative genomics highlights the remarkable connections between the genetic blueprints of diverse organisms. In the monotonous and mind numbing sequences of millions upon millions of base pairs of DNA, lie hidden the molecular imprints of a shared history; humbling connections between bacteria, worms, flies and man. But man is decisively different, in that he alone has been able to shape his environment; his survival in inhospitable surroundings determined more by his inventive abilities, rather than the slow and seemingly limited process of biological adaptation by natural selection. Bronowski begins his fascinating story in an area that might indeed be a good candidate as man's original home, 'the valley of the river Omo in Ethiopia near Lake Rudolf', not far from the equator. It is here that an amazing fossil record was discovered in the 1920s, remains of animals and the forerunners of modern man, dating back about two million years. Remarkably, animals appear to have changed little over the years, but the skeletons of our ancestors differ dramatically from our own. Bronowski's interpretation is provocative: 'Human evolution began when the African climate changed to drought: the lakes shrank, the forest thinned out to savannah. And evidently it was fortunate for the forerunner of man that he was not well adapted to these conditions. For the environment exacts a price for the survival of the fittest; it captures them'. Bronowski, a mathematician by training and vocation began his musings on the origins and evolution of man, in 1950, when he was posed the problem of discriminating the teeth of the *Australopithecus* baby skull found south of the equator in Africa, from the teeth of apes. He recalls his sense of excitement: 'I, at over forty, having spent a lifetime in doing abstract mathematics about the shapes of things, suddenly saw my knowledge reach back two million years and shine a searchlight into the history of man'.

In tracing the ascent of man, we must and Bronowski does, separate the long period of biological evolution which stretches over millions of years, from the relatively short span of cultural evolution, which does not stretch beyond ten thousand years; a period for which we have little by way of historical record, beyond about three thousand years. Bronowski prods us gently to ask:

'Why did the cultural changes that have made man the master of the earth begin so recently?' Why, indeed, is the pace of cultural evolution so breathtakingly rapid 'once it takes off', as compared to the slow, hesitant and seemingly random steps of biological evolution? But these are questions which have no real answers as yet; having raised them, Bronowski moves briskly to examine the surging progress of man over the past few millennia. Science is at the heart of Bronowski's story.

In defining the conceptual 'turning points' in the ascent of man Bronowski adopts what a carping critic might call a Eurocentric view; the Mediterranean and the Old and New Testaments form the threads on which his narrative is woven. Bronowski realizes this and says in defence: 'Of course there were great civilizations. Who am I to belittle the civilizations of Egypt, of China, of India, even of Europe in the Middle Ages? And yet by one test they all fail: they limit the freedom of the imagination of the young. They are static, and they are minority cultures. Static, because the son does what the father did, and the father what the grandfather did. And minority because only a tiny fraction of all that talent that mankind produces is actually used; learns to read, learns to write, learns another language and climbs the terribly slow ladder of promotion'. Having defended the setting of his stage, Bronowski embarks on a remarkable tour; one that begins with the 'agricultural revolution' and ends in a contemplative view of the crematoria of Auschwitz. It is a gripping story, told in an engaging manner, in which we see the evolutionary origins of much of modern science and culture. But its beginnings are rooted, as they must be, in that crucial turning point where man recognized the potential of a unique relationship with plants and animals; the transition from the unsettled ways of the nomads to the apparent tranquility of an existence based on agriculture. The domestication of animals provided also one of the first instruments of war, the horse. And in the pages of Bronowski's book I found a description of one of the last 'war games' using horses that remains in existence. This is a game called 'Buz Kashi', a feat of competitive riding played in Afghanistan, a relic of the days of the Mongols. Bronowski's conclusions are prophetic: 'The Buz Kashi is a war game. What makes it electric is the cowboy ethic: riding as an act of war. ...The nomad in his last historic role as warmaker is still an anachronism, and worse, in a world that has discovered in the last twelve thousand years, that civilization is made by settled people'.

The narrative quickens as Bronowski calls attention to the development of architecture, metallurgy and a rapidly increasing 'understanding of the material world'. Straddling the period between the ages is the haunting science of alchemy, a subject Bronowski treats with understanding; after all, alchemy represented the prolonged groping of man to understand the world around him. In presenting the rising tide of human knowledge in the period between the 16th and 19th centuries that Bronowski celebrates 'open and personal discoveries'. The act of Paracelsus, in 1527, when he 'threw into the traditional student bonfire an ancient medical textbook by Avicenna, an Arab follower of Aristotle', is interpreted as a gesture which said: 'Science cannot look back to the past. There never was a Golden Age'. Bronowski pauses at times to take a reflective look; his discussion of sculpture, symmetry and art moves him to quote Michelangelo:

'When that which is divine in us doth try
To shape a face, both brain and hand unite
To give, from mere model frail and slight
Life to the stone by Art's free energy'.

It is this unification of the brain and the hand that fascinates Bronowski; it is here that he states his philosophy: 'We have to understand that the world can only be grasped by action not contemplation. The hand is more important than the eye...; and indeed we know as something more than a symbolic accident in the evolution of man that it is the hand that drives the subsequent evolution of the brain'. Bronowski notes approvingly that Benjamin Franklin, in 1778, called man 'a tool making animal'. To Bronowski,
of man is the refinement of the hand in action'.

Bronowski moves with felicity through the development of modern science, Galileo, Newton, Darwin, Wallace, Pasteur, Einstein, stories which bear retelling. In reflecting on the development of atomic theory, Bronowski notes that the 'human imagination working communally has produced no monument to equal it, not the pyramids, not the Iliad, not the ballads, not the cathedrals'. In listing the cast of heroes, Bronowski chooses Mendeleev, J. J. Thomson, Rutherford, Bohr, Chadwick and Fermi. But I was struck by the fact that he placed at the head of them all, 'the first founders of the new conceptions', Max Planck and Ludwig Boltzmann; 'to whom more than anyone else we owe the fact that the atom – the world within a world – is as real to us now as our own world'. Boltzmann lived at a time when the ascent of man teetered on a fine intellectual balance'. Boltzmann committed suicide in 1906, little knowing that the 'atomic doctrine' he espoused was going to win. To Bronowski no phrase can 'match the compact and penetrating beauty' of the immortal formula $S = k \ln W$, carved on Boltzmann's gravestone in Vienna.

To end we must return to the beginning and Bronowski: 'Man is not the most majestic of creatures.... But he has what no other animal possesses, a jig-saw of faculties which alone, over three thousand million years of life, make him creative. Every animal leaves traces of what it was; man alone leaves traces of what he has created'. We might, following Bronowski, wonder where we are in the ascent of man and where we are going. Unfortunately, the past may not be a good guide to the future.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The integrity of structures

Science journals operate on an implicit principle of trust, assuming that authors of all papers, that are submitted, report results as they have been obtained. While interpretations may sometimes be incorrect, the facts are generally assumed to be incontrovertible. Mistakes and errors, of course, abound in the scientific literature, an inevitable part of scientific research; when uncovered, often by the original authors themselves, these are recorded without undue fuss in the journals of science. This issue of *Current Science* documents an instance where experimental results, reported in a paper submitted for publication, were alleged to be fraudulent; after preliminary enquiry the paper was withdrawn, but the issue of whether the original data was genuine or not led to subsequent enquiries and also resulted in the extensive correspondence that appears in this issue. The setting for this unfortunate episode is one of the best institutions of the chain of laboratories of the Council of Scientific and Industrial Research (CSIR), the National Aerospace Laboratories (NAL). The manuscript in question was authored by B. K. Parida and coworkers and was submitted for publication as a Special Test Publication (STP) of the American Society for Testing Materials (ASTM). In 1998, *Current Science* received a letter for publication authored by R. Sunder, alleging that the Parida paper was based on fraudulent data and further highlighted the apparent reluctance of the organization to investigate the charge. While this letter remained unpublished, its circulation together with the vigorous intervention of a former Director of NAL, S. R. Valluri, led to the initiation of an enquiry ordered by the Director-General of CSIR. Two enquiry committees and two and a half years later, the conclusion was reached that the principal author of the original manuscript, B. K. Parida 'committed serious technical mistakes'. The 'whistle blower', Sunder clarified that plagiarism had never been a charge. This left the origin of the data, a fractograph in the original manuscript submitted to ASTM, a puzzle. Indeed, the enquiry committee found 'that the ownership of the fractograph in ASTM-CP will probably remain a mystery unless one carries out a different kind of investigation, perhaps even forensic in nature'. The entire episode was further clouded by the fact that Sunder had left the Structural Integrity Division at NAL a few years ago and had indeed been closely associated with the activities of the laboratory from which the disputed manuscript originated.

In considering the publication of the correspondence in this issue several questions arose: First, should *Current Science* consider controversial issues pertaining to an unpublished manuscript? Secondly, should this journal consider discussion of specific cases where ethical issues in science are involved, or should it confine itself solely to discussions of the abstract problem of misconduct in science? Thirdly, would not public airing of contentious issues sully the reputations of individuals and institutions? Fourthly, would not publication of such correspondence diminish the image of the journal, bringing a touch of the popular press, to a journal devoted to the dissemination of science? It was precisely these considerations that led to non-publication of Sunder's original letter; but the matter surfaced again with the resubmission of a technical manuscript, which cleverly highlights the complex issues involved. Based on opinions of expert referees this manuscript was revised and accepted and sent to authors of the original disputed manuscript and the head of NAL for their responses. The entire material was then scheduled for publication in our August 10 issue, but the impending report of the second committee of enquiry led to its withdrawal from the press. While this report found evidence for 'serious technical error' its content and conclusions continue to be debated. Both sides of the issue have used the popular press, a medium in which technical matters can be easily obfuscated and personal weaknesses highlighted. As this journal vacillated between final publication and rejection, pressures have mounted. The proponents of public airing of the issue charged this journal with bowing to pressure and accused the editors of paying 'only lip service' to the cause of ethics in our science. The opponents of publication charge that by printing Sunder's note this journal maligns an institution; some go so far as to impute motives to this journal for even entertaining Sunder's original submission.

The decision to publish is, finally, solely that of the editors. The cause of science is not helped by avoiding

discussion of the difficult issues of acceptable conduct in the practice and reporting of research. Institutional images do not depend on individuals, but are the result of peer perception, accumulated over many years. It is important that individual reputations do not suffer unfairly; but enlightened readers can also be discerning judges. Inevitably, such editorial decisions are tinged with regret, but there is the hope that public contemplation of contentious issues, will allow us to evolve robust mechanisms for swiftly addressing allegations of scientific misconduct. As in the present case, the slow pace of enquiries and the tendency to centralize the decision-making processes of administration, lead to wounds that fester and eventually become untreatable. The rapidity with which investigative actions can follow the charge of a misdemeanour is illustrated by the action of the Johns Hopkins University, in examining the case of the clinical trials conducted collaboratively with the Regional Cancer Centre, Thiruvananthapuram (*Nature*, 2001, **414**, 386).

A major difficulty is to distinguish between flimsy allegations that appear to be motivated by personal jealousies and animosities from those that appear to be based on moderately reliable evidence. A swift, preliminary, internal enquiry is indeed necessary to establish whether a *prima facie* case exists or not. If there is a case, a purposeful and quick institutional investigation is necessary to clear the air. Most importantly, if mistakes have been made it may be easier to acknowledge them at the earliest, rather than to convert the issue into a major battle, with the protagonists ranged on opposite sides. The tendency to paint all 'whistle-blowers' as disgruntled elements, out to rock the boat of scientific progress must be avoided. When misdemeanours or errors of judgement are uncovered, the punishments must fit the situation; harshness is generally unwarranted. Regrettably, there has always been a tendency to award harsh punishments to those who may least deserve it; students and postdoctoral fellows can easily be removed from their positions when there is a whiff of trouble. Senior scientists, on the other hand, are protected by institutional armour, powerful colleagues and the general reluctance to wage a prolonged battle to establish facts.

While a significant proportion of the scientists who matter, subscribe to the dictum 'hear no evil, see no evil, speak no evil', at least in part, the cause of upholding scientific ethics is not helped by the intemperate posturing of those who don the role of guardians of scientific values. Curiously, many leading science administrators develop a passion for just causes after retirement from powerful positions. Having been out of touch with the practical aspects of scientific research, these guardians of integrity see skeletons in every closet; sometimes their charges border on the ridiculous. One interesting example is the oft-repeated statement that the results of Ph D research must appear in papers that always bear only the supplicants' name; the appearance of the names of mentors or collaborators is construed as an example of misconduct. It is this kind of attitude, that betrays a complete lack of understanding of any of the processes of modern science, that has led to the community generally discrediting the views of our champions of integrity. 'Crying wolf', once too often, is not a fruitful way of promoting the cause of ethical behaviour in science. Suggestions that issues of scientific misconduct be examined by the Central Vigilance Commissioner (CVC) reveal a touching naivete, which fails to acknowledge the complexity of the issues involved and the inability of the CVC to even check widespread bureaucratic and political corruption. But, individual conscience keepers have played a vital role which must not go unacknowledged.

The Sunder–Parida episode has also strained the credibility of our editorial process, as judgements on submitted manuscripts have wavered over time. But, pressures on editors are not uncommon, as exemplified by the recent report that the journal *Human Immunology* has deleted an accepted paper on genetic variability in Palestinians. The stated reason was that the paper contained 'inappropriate political comments about the Israeli–Palestinian conflict' (Klarreich, E., *Nature*, 2001, **414**, 382). It is indeed with some regret that we publish the correspondence in this issue on the 'Reproducibility of metal fatigue'. But, publication and transparent enquiry may be essential for the maintenance of the integrity of our structures — institutions and journals amongst them.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Libraries

'The faint aroma of gum and calico that hangs about a library is as the fragrance of incense to me. I think the most beautiful sight is the gilt-edged backs of a row of books on a shelf. The alley between two well-stocked shelves in a hall fills me with the same delight as passing through a silent avenue of trees. The colour of a binding-cloth and its smooth texture gives me the same pleasure as touching a flower on its stalk. A good library hall has an atmosphere which elates. I have seen one or two University Libraries that have the same atmosphere as a chapel, with large windows, great trees outside, and glass door sliding on noiseless hinges.'

— *R. K. Narayan*
(*The Hindu*, 23 September 1951)

Libraries are central to academic life; they are the heart of a University. Public libraries, friendly and accessible, were, not too long ago, powerful instruments for educating, informing and entertaining a wide clientele. In a recent issue of a chemistry journal, I came upon an evocative description of a library by Roald Hoffmann, Nobelist, chemist, poet and playwright: 'To a secular refugee of World War II who did not own a book until age 13, a library was about as close to a holy place as I could imagine. ...They were filled with organized knowledge, with wisdom, with the salacious (to a boy), hidden under the librarian's desk. These true libraries were the work of men and women shaping laughter and tears, defeating entropy' (*Angew. Chem. Int. Ed. Engl.*, 2001, **40**, 3337). To both Narayan and Hoffmann, libraries provided a window to the remarkably rich human experience. Half a century has passed since R. K. Narayan pondered on libraries in his characteristically simple style. In this period, the state of many wonderful libraries in India has deteriorated; the decay hastened in recent years by the seductive promise of the new digital technologies.

For the proponents of digital libraries exciting times lie ahead. In describing a vision for the future, Ian Witten says: 'Digital libraries whose history spans a mere dozen years will surely figure amongst the most important and influential institutions of this new century. The information revolution not only supplies the technological horsepower that drives digital libraries, but fuels an unprecedented demand for storing, organizing and accessing information. If information is the currency of the knowledge economy, digital libraries will be the banks where it is invested' (Conference Papers, The 4th International Conference of Asian Digital Libraries, eds S. R. Urs, T. B. Rajashekar and K. S. Raghavan, 2001, Bangalore). Clearly, the rhetoric is a far cry from Narayan's simple-minded view of libraries; information and knowledge are now viewed as key elements of a ruthlessly competitive global economy. But in concluding his essay, Witten reminds us of S. R. Ranganathan's five laws of library science, paraphrased succinctly: (1) Books are for use. (2) Every reader his book. (3) Every book its reader. (4) Save the time of the reader. (5) The library is a growing organism. These laws might, of course, apply to the digital libraries of the future.

For the time being, however, our academic institutions continue to revolve around traditional libraries; most often, imposing buildings on our campuses, whose impressive exteriors hide a developing crisis caused by diminishing resources, rising costs of books and journals, difficulties of procurement and, most importantly, the declining number of dedicated professional librarians. Indeed, in our academic institutions, the key role of professional and committed librarians has rarely been acknowledged. There have been few attempts to define criteria to assess a librarian's effectiveness. Libraries have been built around the importance of acting as a 'storehouse of information', with little emphasis on the 'delivery of information'. There is little interaction between librarians and users; the scholarly librarian with a love for books and a vision for the future has become an endangered species. For this extinction, the academic community bears a major responsibility; years of downgrading the importance of libraries and their custodians in the institutional hierarchy, have resulted in lifeless entities which merely act as a storehouse for holdings of books and journals. The dramatic increase, over the last two decades, in the volume of scientific publications, requires that libraries must carefully pick and choose

journals for their collections; carefully balancing the academic requirements of their institutions with the budgetary constraints under which most organizations operate. Even in the most fortunate of institutions, library budgets can hardly keep pace with rising costs of acquisitions. But, how is the effectiveness of a library judged? Blagdon has an interesting view of library audit: 'It is important to establish how much good does a library do, rather than how good a library is' (Library and Administration Management Association, 1980, p. 25). Many of our libraries have become extremely restrictive and unfriendly to users, increasingly becoming mausoleums for books and journals. Surprisingly, in my hunt for material for this essay, I found that a long time ago, R. K. Narayan had said it all, in his inimitable manner—'Library Science seems all concerned with furniture, locks, keys, registers, vouchers and statistics, everything except reading. The most noticeable deficiency in any library today seems to be a lack of propaganda for books themselves. It would be useful to inscribe on every library-wall the motto: "Books are meant to be read and not merely to be classified and preserved"'. In describing his experience at a new public library, Narayan laments on the budgetary plan: 'Building Rs 2,00,000; lighting Rs 15,000; plumbing, etc. Rs 12,000; garden lay-out and supervisors' charges Rs 5000, counters, shelves and furniture Rs 30,000; opening ceremony, printing of invitations, welcome address, president's speech and secretary's report Rs 2000; pandal and tea Rs 4000 and books, no budget yet' (Narayan, R. K., *The Hindu*, 5 April 1953). The budgets may have gone up substantially since Narayan wrote his piece, but priorities are still skewed in most public libraries.

With the digital age having come upon us, conventional libraries are likely to be hard pressed to even maintain their present state. A new breed of information managers have sprung up, who speak of technological fixes to all problems of access to information. Lazy browsing amongst tightly packed shelves of books and journals, an intensely pleasurable activity for members of a disappearing generation, is giving way to browsing on the internet, a remarkable, formless, unorganized digital library made navigable by the spectacular prowess of the technology of the 'search engine'. For many students, introduced early to the wonders of the world wide web, limited reading and minimal attention spans become habits which may be difficult to shake off in the future. In our increasingly networked world, a new digital divide is becoming evident; a schism based on age. For the young, the internet appears the most efficient and desirable source of information, the printed book and journal rapidly becoming secondary in importance. For the older academics, this writer amongst them, the digital world poses many psychological hurdles. In preparing to speak, as a conventional and conservative editor, at a recent conference on Digital Libraries, I was reminded of Wordsworth's famous lines celebrating the French Revolution:

'Bliss was it in that dawn to be alive
But to be young was very heaven.'

(The Prelude XI)

But, on the night before my talk, the second line eluded me; lost in those suddenly inaccessible recesses of the mind. The internet and Google came quickly to my rescue. Searching that ocean of stored information, using Wordsworth's first, and only remembered line, as my search string I discovered 17,500 hits in a blindingly fast response time of 0.73 seconds. Fortunately, the very first site listed, yielded after a little perusal, Wordsworth's immortal second line. But, I was not so lucky when I searched for 'digital libraries'; finding as many as 1,020,000 hits in 0.27 seconds, quite literally the blink of an eyelid. This is, of course, both the strength and weakness of the digital age. Information is literally available at your fingertips in an instant, but most often there is a surfeit of it, making it very difficult to sift the relevant from the irrelevant, the good from the bad, the reliable from the unreliable. The explosion of digitally stored information can only be harnessed usefully if new and thoughtful ways of organizing digital libraries are evolved, a process which will require a new generation of scholarly information managers, who have the ability to clearly distinguish between information, knowledge and wisdom. In the immediate future an important task may be to protect conventional libraries from being swept away in the deluge of hype and misinformation that heralds the digital age.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science, technology and public perception

'Pure science requires no justification outside itself, and its usefulness has no bearing on its validation.'

P. B. Medawar

'The effects of science are of various, very different kinds. There are direct intellectual effects, and the adoption of others suggested by the success of the scientific method. Then there are effects on technique in industry and war. Then, chiefly as a consequence of new techniques, there are profound changes in social organization which are gradually bringing about corresponding political changes. Finally, as a result of the new control over the environment which scientific knowledge has conferred, a new philosophy is growing up, involving a changed conception of man's place in the universe.'

Bertrand Russel
(*The Impact of Science on Society*,
Unwin Hyman, 1952, pp. 11–12)

'Technology, unlike science, is a group activity. It is not based on an individual intelligence, but on the interacting intelligence of many.'

A. P. J. Abdul Kalam
(*Wings of Fire*,
Universities Press, 1999, p. 169)

Science and technology are inextricably linked, yet they are divided by a chasm which is sometimes difficult to bridge. Public understanding of science and technology is clouded by many misconceptions, some of which are of little consequence, while others are more 'mischievous'. In Peter Medawar's words, 'all help to estrange the sciences from the humanities and the so called "pure" sciences from the applied' (*Pluto's Republic*, Oxford University Press, 1983, p. 28). Two centuries of extraordinary progress in science and its application have propelled science and technology (S&T, in government parlance), to a position where the public at large and decision makers in particular, can ill afford to be completely ignorant of the complex relationships of scientific research, technology development and its eventual impact on society.

In India, the lines dividing science and technology have, traditionally, been drawn very sharply. Even at the level of governmental policy statements, the divide is emphasized by the existence of two distinct enunciations of national intent – the Science Policy Resolution of 1958 and the Technology Policy Statement of 1983. The former recognized that 'technology can only grow out of the study of science and its applications', while the latter emphasized the need for 'the development of indigenous technology and efficient absorption and adaptation of imported technology appropriate to national priorities and resources'. Science, to most people, seems to embody the 'pure' sciences taught in colleges and universities and researched in quiet and unobtrusive institutions. Very few realize that connections may exist between scientific research and the teaching of science on the one hand and the many practical and seemingly obvious advantages of applicable advances in science, on the other. Technology is more clearly perceived and assessed. Technology, in the public mind, is associated with engineering; broader definitions that encompass areas like agriculture are appreciated only in specialized circles. It is not an accident that the most visible and successful educational institutions in post-Independence India have been the Indian Institutes of Technology (IITs), which have produced generations of outstanding engineers and entrepreneurs, who have distinguished themselves globally. Interestingly, we have only one Indian Institute of Science, whereas the number of IITs is swelling; as many as seven at the last count. Even 'Indian Institutes' in subjects like management and information technology have burgeoned, apparently serving an increasing public demand. Clearly, there is an important difference in the way in which the lay public and even decision makers, politicians, bureaucrats, scientists and technologists amongst them, perceive the importance and needs of the twin arms of science and technology. Sometimes, the operational phrase is 'science *and* technology'; on many occasions, it seems to be 'science *versus* technology'.

There are profound differences in the manner in which basic scientific research is pursued and the pathways by which successful technologies are developed and imple-

mented. Abdul Kalam delineated these differences clearly, painting the view of science as an individual activity, while technology development invariably is a coordinated, group activity. So it has been for much of the period over which modern science has developed. But, in more recent times the distinctions between scientific discovery and technology development have become blurred. Biotechnology is a dramatic example. In this area the borders between 'pure' and 'applicable' science dissolve very quickly. Today's laboratory curiosity can become tomorrow's favourite technology of the market place. Biotechnology is a curious arena, where the transition from science to technology does not always have to be midwifed by engineers. The direct impact of biotechnology on vast areas of medicine and agriculture is transforming the way in which the relationships between science and technology are perceived.

The importance of enhancing the public understanding of science and its potential applications has grown in recent times; the fact that many disciplines of modern science can only be undertaken with a considerable investment of public funds, imposes on its practitioners a major responsibility to communicate in a clearly understandable manner the results of studies that may have a direct societal impact. In a timely 'millennium essay', Nancy Rothwell notes that 'the communication of science to society is not new, nor is it an easy task. It is almost two hundred years since Albemarle Street in London, home of the Royal Institution was designated the first one way throughfare'. The horse-drawn carriages that clogged the street outside the Royal Institution, brought much of the aristocracy of Victorian England to hear Humphry Davy and later, the incomparable, Michael Faraday. In India the public communication of science has been limited and there is little effort to explain the pros and cons of most new technologies in a manner that promotes informed debate. Major issues like the nuclear power scenario, the introduction of genetically modified crops or the mass immunization strategies are rarely discussed or explained until a controversy erupts. Even then, the S&T establishment generally takes the view that the shaping of public opinion is unimportant, tacitly assuming that the comprehension of the niceties of technical issues is generally outside the capabilities of the lay public. Indeed, on most issues of public concern, even scientists and technologists outside a specialized field do not form informed and independent opinions. Scientists also take little time to convey the great sense of excitement and challenge that motivates many researchers, who work at the cutting edge of science, thereby missing an opportunity to recruit public support for scientific endeavour.

But, in considering the public perception of science news from Europe makes interesting reading. A European Commission Survey revealed (*Nature*, 2000, **414**, 680) that 'when the public were asked which professions they held in the highest esteem', doctors, scientists and engineers took the top three spots, edging out sportsmen, lawyers and journalists. True to form, politicians brought up the rear. However, less than half of those polled, described themselves as interested in science. There is a growing need to enhance the public perception of science in India; the onus for any initiatives rests with the scientific community.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Transition

I have got my leave. Bid me farewell my brothers! I bow to you all and take my departure.

Here I give back the keys of my door – and I give up all claims to my home. I only ask for last kind words from you.

We were neighbours for long, but I received more than I could give. Now that the day has dawned and the lamp that lit my dark corner is out. A summons has come for me and I am ready for my journey.

— *Rabindranath Tagore*
(*Gitanjali*, Translation by the Author, Macmillan, 1913)

Life is real, life is earnest,
And the grave is not its goal,
Dust thou art, to dust returnest,
Was not spoken of the soul

— *Henry Longfellow*

Bangalore was a quiet, sedate town in the 1950s and the Indian Institute of Science was tucked away in its northern corner, its existence unannounced even by a signboard. There were no impressive gates at its entrance, no visible sign that the cluster of low-lying buildings housed an institution that already had an international reputation as a centre for scientific research; one that would serve as a centrepiece for the development of science in post-independence India. C. V. Raman had already established the physics department as a major presence, in the tumultuous period between his entry into the Institute as its Director in 1935 and his formal retirement as a professor of physics in 1948. Despite being armed with the Nobel prize, a level of attainment that would have made him invincible today, Raman was removed as Director by the Institute's Council in 1937, moving him from the centre of administrative power to the seclusion of his own laboratory. Homi Bhabha had come and gone, moving to Bombay (now Mumbai) to begin the establishment of the Tata Institute of Fundamental Research and the Atomic Research Centre at Trombay, now named after him. By the mid-1950s the Institute had begun to lapse into middle-aged obscurity. Jawaharlal Nehru and his principal advisors were already embarking on a major program of building India's scientific infrastructure; new institutions, national laboratories and the glamorous Indian Institutes of Technology were already on the anvil. Few would have spared a thought for an ageing and established institute that seemed to be slowly slipping into a comfortable state of academic somnolescence. It was into this ambience that Satish Dhawan made his entry into the Institute in 1951, as a member of the faculty of the Aeronautical Engineering department, rising in dramatic fashion to become its Director in 1962, at the remarkably young age of 42. When he formally retired in 1981, he left an institution that had grown enormously in size and scope and was arguably, the pre-eminent institute of science in the country, comparable to many in the developed nations of the West. Legend has it that Dhawan entered the Institute as an extraordinarily dashing young man, driving a red sports car; I saw him when he left it, a remarkably handsome and distinguished figure exuding a quiet charm that was uniquely his. In this period he transformed the academic structure of the Institute, moving it from a feudal departmental structure presided over by a single, powerful, and most often, inhibitory professor to a more collegial model which promised the prospect of collective decision making on academic matters.

During his long innings as its Director, Satish Dhawan gently and at times, unobtrusively guided a transformation of an established academic institution; a formidable task that required a clear vision, a firm resolve and an ability to persuade recalcitrant academics to tread a new path. Experience tells us that in our surroundings when old institutions begin to falter, it is easier to contemplate setting up new ones; the hope is that this strategy sidesteps the difficult problem of effecting reform and change, in institutions with set traditions. Dhawan's administrative achievement at the Institute in Bangalore, has faded into obscurity, dimmed by the lustre of his achievement in building up the Indian Space Research Organization (ISRO) into the formidable structure it is today.

In the period between the late 1960s and early 1970s, Dhawan began a program of expansion at the Institute, which would bring in a very large number of faculty, some of them at the professorial level. The institution was soon to acquire a breadth of discipline that was

unprecedented; the influx of new blood hastening the process of democratization. Many new departments, which were later to become pre-eminent, were established during this time, among them were Computer Science and Automation, Centre for Theoretical Studies, Molecular Biophysics Unit and Solid State and Structural Chemistry Unit. The latter two units were to bring to Bangalore two of India's most accomplished scientists, G. N. Ramachandran, already a world renowned figure, and C. N. R. Rao who would very quickly have a major influence not only on the development of the Institute, but indeed on the growth of contemporary science in India. Dhawan's ability to attract men of unique and varied talents and his role in shepherding the Institute during this phase of explosive growth, a period that coincided with the development of mechanisms for large-scale funding of academic science, must surely rank as one of his finest achievements. The Institute`s focus on research, nurtured carefully by Dhawan, deepened in later years, providing an institutional ambience that was clearly distinct from other institutes of technology.

Dhawan's love for Bangalore and his attachment to the Institute led inevitably to the choice of this city as the headquarters of the Indian Space Research Organization. Vikram Sarabhai's untimely death in 1971, catapulted Dhawan into a dual role heading both ISRO and the Institute for a decade. The space program has had a wonderfully romantic history, with Satish Dhawan as the guiding force; in times of failure he shouldered responsibility, in the heady days of success he stood quietly by the sidelines. Dhawan`s ability to build an organization whose success relied on teamwork, discipline and collective dedication was truly remarkable, particularly when one recognizes that he was simultaneously guiding an institution, where individuality and idiosyncracy were cherished qualities.

My view of Satish Dhawan is necessarily circumscribed by the limited perspective from which I saw him. For a man of many facets, any historical assessment must, of course, come from a scholarly study of his life and times. But, at the distance from which I viewed him, separated by the gulf of age, position and discipline, Dhawan was a man who engendered immense respect by his grace of conduct. There was an engagingly shy air of reticence about him, uncommon in men, who have experienced power and distinction. In thinking about Dhawan, my thoughts drifted, almost inevitably, to some of his contemporaries, whom I have known, who have also departed in the last few months. Just a few days before Dhawan's passing, T. R. Govindachari, an organic chemist of special distinction, died in Chennai at the age of 86. In the months before, two more of India's eminent scientists passed away; V. Ramalingaswami at the age of 80 and G. N. Ramachandran in his 79th year. Govindachari built an extraordinary record of natural products research in the most modest surroundings of Presidency College, Chennai in the 1950s. He moved in the mid-1960s to the Ciba-Geigy Research Centre in Goregaon as its first Director; an unusual step at the time. This was the first major private R&D centre in India, opened by Jawaharlal Nehru in 1963. Govindachari was an amazingly uncomplicated person, interested passionately in experimental work. He managed to work in a laboratory well into his eighties, returning to Chennai to set up two private R&D centres. I have rarely met anyone with such an enthusiasm for experimental detail; he was always completely immersed in the wonderful molecules he coaxed out of nature. Govindachari set standards for scholarship and rigour that few are prepared to emulate today. G. N. Ramachandran, a man of remarkable achievement and distinction, also did his path-breaking work on protein and polypeptide structures in Chennai, before moving to Bangalore in 1971; a transition initiated by Dhawan. Ramachandran, a biophysicist, was brilliant, unpredictable, totally dedicated to his science, leaving behind him an indelible impression on his discipline. Finally, there was V. Ramalingaswami, who more than anyone else influenced the development of medical research in India. He headed the All India Institute of Medical Sciences and later the Indian Council of Medical Research. His work in the area of nutrition brought him great international distinction. Until the end he was a much respected figure in making biomedical research policy. I saw and heard him on many occasions and was always struck by the clarity of his thinking, the elegance of his presentations, the intensity of his committment and above all, the graciousness of his manner. In thinking about this quartet of remarkable men of science, I was reminded of the famous quartet of Indian spinners, who spun us to many a famous victory – Bedi, Prasanna, Chandrasekhar and Venkataraghavan. Here were men of different backgrounds, contrasting styles and distinctive abilities, consistently propelling their team towards success. The four men of science also had their unique styles; Dhawan and Ramalingaswami were men who presided over Academies, rubbed shoulders with Prime Ministers, accepted great responsibilities and challenges, whose dedication is evident in the accomplishment and character of the institutions they built. Ramachandran and Govindachari were academic scientists, focusing intensely on their chosen disciplines, carving for their laboratories a level of international visibility, that allowed their surroundings to bask in reflected glory. These were all men who dedicated themselves to the cause of science in India, as men in the prime of youth in the heady years after independence. They played full and magnificent innings, which bear retelling in the years to come. For those who bemoan the absence of role models in our midst, reflecting on Satish Dhawan and his remarkable contemporaries may be instructive. Many of us must count ourselves as fortunate to have seen them all.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Higher education in science

It is hard to attend any gathering of scientists, where the conversation does not eventually drift towards a discussion of the rapidly diminishing interest of good students in pursuing careers in science. Academic researchers lament the decreasing quality of students enrolling for doctoral degrees. In some disciplines like physics and mathematics, even the best of institutions in India base their annual selections for Ph D programs on a limited pool of applicants. The decline of interest in science starts early, with students, as they leave school, opting for engineering, medical or even commerce courses, in preference to science. As the number of engineering and medical colleges has mushroomed, even students with moderate academic attainments have little difficulty in getting admitted to these favoured courses. Undergraduate science courses are left depleted, sometimes serving as the last refuge for those whose options are limited. Indeed, in some undergraduate colleges in states like Karnataka some science courses suffer a sudden reduction in the size of the classes, when supplementary admissions to the myriad engineering and medical colleges are announced. Clearly, science is not perceived as an attractive career option by both parents and children; many other professions appear to offer greater opportunities. But given the enormous size of the student pool in India, there must still be a very large number of students who are motivated to study science; often attracted by the glamour and mystique associated with scientific discovery. Why is it that many of these students also end up in other courses? Even the few motivated students who enter undergraduate science colleges appear to be driven by a desire to use a science degree as a passport to postgraduate education overseas. Some of those who survive the undergraduate and master's courses enter the portals of our research institutions and university departments in search of Ph D degrees; it is their declining numbers and quality that trigger the familiar lament on the state of higher education in science today.

Can undergraduate science courses be made more attractive? Can the decaying structures of our science colleges and universities attract new students harbouring high ambition? Can latent talent blossom and flower in surroundings that are far from inspirational? Is it possible that the very fact, that undergraduate engineering education in the IITs is extremely well organized and challenging, is sufficient to attract the best of students, since they are guaranteed a sound technical grounding, which will serve as a launchpad to a future career. The IITs have indeed acquired over the past forty years an enviable reputation for rigorous student selection and high standards of training, providing in today's parlance a 'brand equity', which can hold its own in the global marketplace for human resource. Unfortunately, there are no equivalents in science. A rather critical observer of the academic science scene caustically pointed out to me that 'ivory tower scientists' discuss the issue with enthusiasm, but take little initiative in working towards innovative strategies for enhancing the quality and attractiveness of undergraduate and postgraduate education in science.

Some years ago the depressing state of science education in India was starkly highlighted in a proposal to establish a National Science University (NSU). The proposal which had a foreign origin, with its principal champion being a non-resident Indian, prompted an outcry from the scientific community, in a debate in the columns of this journal (*Curr. Sci.*, 1994, **67**, 502–519). The idea of spending large sums of government money on a new, apparently elitist institution for science seemed abhorrent. The proposal died, its demise hastened by a deluge of adverse reaction. Subsequently, indigenous versions of the NSU proposal surfaced briefly, only to drown in a sea of lethargy. All these proposals had a common theme; the creation of a new national institution, where undergraduate science education would flourish alongside frontline academic research, exposing prospective entrants to science to an academic culture which might prove seductive. This approach would have been in sharp contrast to our existing model where teaching is largely relegated to colleges, where little research is practised. The success of many major American universities, where undergraduate education and research coexist, seemed to provide a rationale for these proposals. But, in India the historical development of institutions has followed a different path. Research in many disciplines

of science has slowly become concentrated in national laboratories and institutions which have no teaching programs in science; the focus is exclusively on research, leading to Ph D degrees. Even in most central universities, teaching in the science departments is limited to Master's students. The gulf between students who have just left high school and research departments has been occasionally bridged in integrated M Sc programs in sciences at a few IITs, but the number of entrants to these courses has been very limited. In most university science departments, both teaching and research are at a low ebb. In the research institutions, complaints about the background of entering Ph D students are shrill and frequent, but little by way of teaching and training programs have been introduced. Many national laboratories admit large numbers of Ph D students, who will eventually submit doctoral theses to universities to which they are only formally associated. At these institutions Ph D students are viewed as the means by which research is carried out; there is limited interest in their overall training in the broad disciplines in which they supposedly specialize. In such an environment, faculty interest in teaching activities is minimal. Ironically, many of these institutions would now like to acquire the status of 'deemed universities'; a convenient device by which they will be able to give their own degrees, despite the complete absence of academic structures which define a conventional university.

Could the research institutions play any role in an attempt to reconstruct higher education in science? Is it possible to imagine an activist scenario in which attractive teaching programs emerge in institutions whose predominant concern, thus far, has been research? Would existing institutions be prepared to forge partnerships and invest their resources, human and material, in creating undergraduate and postgraduate programs, which attract the best of students? If the answers to these questions are affirmative, we may have the seeds of an experimental initiative which may prove to be a good, long-term investment for the research institutions.

The importance of 'educating future scientists' was the subject of editorial comment in *Nature* (2001, **414**, 673), ending in the gentle admonition: 'Now is the time for researchers—some of whom are reluctant even to engage properly in undergraduate education—to seek these out and participate in them.' In India there have been a couple of instances where sophisticated research institutes have been built within university campuses, presumably with the hope that a fruitful symbiosis between research and teaching would emerge. Unfortunately, these institutions are now fortresses insulated by a wall of security from their surroundings. The number of foreign universities who now vigorously recruit science students in India, for their programs, is on the increase. Our research departments face the threat of a further erosion in the quality of their input and the number of applicants who knock on their doors. It may indeed be an opportune time to once again talk of research and teaching in the same breath.

P. Balaram

CURRENT SCIENCE

Volume 82 Number 4 25 February 2002

EDITORIAL

Environmental wars

'The lady doth protest too much, methinks.'
Hamlet, Act III, Scene 2

'There are three kinds of lies: Lies,
damned lies and statistics.'

— Benjamin Disraeli

The environmental debate can be raucous, unrestrained and entertaining. The depredations of development are all too obvious to green activists, whose protests are compellingly dramatic. Conservative economists (and politicians) are generally prone to dismiss the entire spectrum of environmental concerns as the hysterical outpourings of an extremist fringe. In the middle are a large body of respectable, environmental scientists and policy makers, who attempt to assess the growing body of facts, that must provide the basis for building predictive models for the future, which in turn may be crucial in charting the course of developmental strategies. *The Economist* (2 February 2002, p. 71) lists four, much touted environmental fears:

- Natural resources are running out.
- The population is ever growing, leaving less and less to eat.
- Species are becoming rapidly extinct, forests are vanishing and fish stocks are collapsing.
- Air and water are becoming ever polluted.

It is this 'litany' of concerns that has been addressed by a hitherto little known Danish analyst, Bjorn Lomborg, who teaches statistics in the political science department of the University of Aarhus. Lomborg has taken the environmental world by storm, publishing a book, *The Skeptical Environmentalist* (Cambridge University Press, 2001). His thesis, simply stated, is that the litany of environmental concerns is exaggerated and unfounded; we have little to worry and we can dismiss all fears for our environment as the pessimistic projections of 'prophets of doom and gloom'. Lomborg's views may never have become a subject of public debate if it had not been for the ferocity with which his book has been attacked in reviews in science journals. Both *Nature* and *Science* published devastatingly dismissive reviews, while *Scientific American* took the extraordinary step of putting together a collection of four essays attacking Lomborg's thesis, under the provocative title 'Misleading Math about the Earth'. For good measure, the editor highlighted the section with the blurb 'Science defends itself against the Skeptical Environmentalist' (*Scientific American*, January 2002, pp. 59–69). Who is Lomborg and why is the environmental science establishment aroused to such fury?

Bjorn Lomborg, in the words of one of his many critics, 'occupies a very junior position in Denmark (an associate professor does not exactly mean the same thing that it does in the United States), he has one probably very flawed paper in an international journal on game theory, no publications on environmental issues, and yet manages to dismiss the science of dozens of the world's best scientists, including Nobel laureates, Japan and Crawford prize winners and the like' (*The Economist*, 2 February 2002, p. 71). In his own words, Lomborg, was intrigued by the view of an American economist, Julian Simon, who argued that 'our doomsday conceptions of the environment are not correct'. Simon, according to Lomborg 'maintained that much of our traditional knowledge about the environment is quite simply based on preconceptions and poor statistics'. Lomborg describes himself as 'an old left-wing Greenpeace member' who 'for a long time had been concerned about environmental questions'. He adds: 'At the same time I teach statistics, and it should therefore be easy for me to check Simon's sources. Moreover, I always tell my students how statistics is one of science's best ways to check whether our venerable social beliefs stand up to scrutiny or turn out to be myths.' And, it is indeed statistics and their interpretation that seems to be at the heart of the battle. Stuart Pimm and Jeff Harvey in a harshly-worded critique, join issue: 'Lomborg's great optimism about humanity's future shows up in the way he presents statistics. In the hellhole that is so much of sub-Saharan Africa, "starving people" constituted "38 percent in 1970"... (but only) "33 percent in 1996". (The percentage is) expected to fall even further to 30 percent in 2010.' The absolute numbers of starving are curiously missing from these paragraphs. Roughly, the region's population doubled between 1970 and 1996. To keep the numbers of starv-

ing constant, the percentage would have had to have dropped by more than half. The absolute number of malnourished in the region ... is surely inconsistent with the first-listed 'global trend' in a chapter entitled 'Things are getting better' (*Nature*, 2001, **414**, 149). Lomborg's response to this criticism is to raise a question: Should we use absolute or relative figures 'when we look at a problem such as hunger or a shortage of pure drinking water'? There are many situations in which one figure increases while the other decreases, making judgements contentious. But, it is Lomborg's use of developmental statistics, put out by international agencies, the United Nations and subsidiary organization and, of course, the World Bank and the International Monetary Fund, which is calculated to raise the most hackles. Lomborg's mining of data is 'curiously selective' and as pointed out by Michael Grubb he 'pays inadequate attention to serious environmental problems in developing countries and his casual assumption that they too will improve as we all get richer' show a 'stunning lack of attention to cause and effect' (*Science*, 2001, **294**, 1285). But, Lomborg is not without supporters, with *The Economist* calling his book 'one of the most valuable books on public policy – not merely on environmental policy – to have been written for the intelligent reader in the past ten years'. The magazine extols Lomborg for 'insisting on statistical probity' and commends him for bringing 'another intellectual virtue to the task; an interest in feasible alternatives'.

Lomborg's thesis and the vigorous debate it has engendered highlight the contentious nature of discussions of environmental policy. Lomborg's detractors, the establishment of environmental sciences, have protested a shade too loudly; the lesson of Queen Gertrude in *Hamlet* evidently forgotten. The inevitable backlash from the conservative opponents of environmental causes is likely to be strong, undoubtedly emboldened by Lomborg's clever use of establishment statistics. Lomborg's supporters might, however, do well to remember that their principal weapon, selective statistics, can be a double-edged sword. Mark Twain in his autobiography immortalized Benjamin Disraeli's famous characterization of lies, damned lies and statistics.

But, in our own surroundings we hardly need the crutch of statistics to come to conclusions about the effects of development on our environment. The push for 'development at all costs' is an approach that must lead to a high environmental price. In Lomborg's Denmark or the Western campuses of his critics, the quality of life may indeed be getting better. The price is paid, however, by others. In much of the developing world environmental concerns are real and must be carefully balanced with the imperatives of development. The choices are often hard and statistics does not always help to clarify the decision-making process. The need for ensuring food and water security in poor countries is real and pressing; the need to clean up the air in the major centers of the South is immediate; receding forests and loss of biodiversity are a reality in many areas of the developing world. Sitting back and enjoying Lomborg's unbridled optimism for our environmental future may not be an advisable strategy.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Budgeting for science: A clearing in the clouds

In a normal year, the end of February always brings the annual budget, fuelling expectations from all sections of the national economy. Science is but a small component in an enormously complex budget document. But, science and technology is a sector that is almost completely dependent on government support, making budget exercises critically important for our vast network of research and academic institutions. For the past several years, almost coincident with the rise of the liberalized economy, the academic constituent of the S&T sector has had little to celebrate in each annual budgetary exercise. This year seems different. When the Finance Minister finally unveiled his package, there appeared to be much that the scientific community might welcome, although the budget itself has been met with a lukewarm response from industry and murmurs of discontent from a middle class, which can hardly be enthused by the prospect of higher taxes and prices. While the Departments of Atomic Energy and Space have received their customary annual increases (over Rs 300 crores in each case), researchers in national laboratories and universities may be wise if they cast their eyes over the figures for the three major wings of the Ministry of Science and Technology, the Departments of Science and Technology (DST), Biotechnology (DBT) and Scientific and Industrial Research (DSIR). The increases for these three departments, which are responsible for supporting much of the non-strategic scientific activity of the country, including basic research, are significant; pushing well beyond the normal adjustments for inflation. The budget is indeed good news for the Department of Science and Technology, providing it with the means to execute an ambitious plan to enhance research infrastructure in university science departments. If all plans stay on course, there will be an infusion of much-needed equipment in many departments. The growing backlog in the funding of approved research projects may indeed be checked, and hopefully eliminated. But the figures (see Table 1, p. 496 of this issue) hide many problems, some of which are not widely appreciated. The DST and the CSIR are committed to the maintenance of a string of institutions, many of which have enormously unfavourable ratios of salary expenditure to research spending. This is also true for the vast majority of our academic institutions and universities. The impact of the excesses of the Fifth Pay Commission and the near impossibility of downsizing staff strength are likely to ensure that it will take a very long time before old institutions are able to fund research to a reasonable extent from their annual allocations.

Even as we welcome a budget that might hold some cheer for scientists, it may be worthwhile to reflect on the costs of research and the functioning of our institutions. In many areas of science the costs of instrumentation and maintenance of major facilities are spiralling. In biology and to a lesser extent in chemistry, the costs of reagents and chemicals increase alarmingly with each passing year, aided by the slow and steady slide of the rupee against international currencies. Since much of the equipment and materials for research are imported, the costs of science increase inexorably with time. With resources necessarily limited, it may also be pertinent to ask whether our institutions and the scientists who inhabit them, spare even a stray thought to the problems of resource sharing and the generation and upkeep of common facilities. A distressing feature of many of our institutions is their remarkable insularity, restricting use of many valuable facilities by those who work in less favoured laboratories. Even within institutions, the practice of science is hindered by disciplinary and departmental barriers, which act as a dampener for resource sharing. Quite often, major grants to university departments fail to benefit a majority of the faculty; the absence of a democratic academic structure ensuring that grants are utilized, with little impact on the activities of those who seek to do research. The internal administration of many universities and institutions ensures 'departmental autonomy', a device which in some places permits the unfettered exercise of despotic powers by heads of departments. Clearly, there are likely to be several instances where the infusion of money alone will not yield any tangible increase in research output. There must be a fresh and purposeful effort by the funding agencies to promote the formation of centres for shared instrumentation, by facilitating a consortium approach between

institutions. Agencies like the DAE, DRDO and Department of Space might also participate in the creation of major facilities in academia, a move which may yield long-term dividends. The carrot of funding may facilitate the formation of research alliances both within and between institutions.

There is also another hurdle to the efficient conduct of research. The internal administrative structure of the funding agencies and the recipient institutions ensures that the tasks of disbursing, receiving and spending funds and finally accounting for their utilization is a process which can be mentally and physically draining for researchers, who do manage to secure funding for their pet projects. In many cases, the financial procedures at the agencies and the recipient institutions appear to work at cross purposes; purchase departments also operate more often as impediments to the use of grants, rather than as important facilitators of research. In the older institutions and in large universities successful scientists need to acquire a new basket of skills that enable them to negotiate the minefield of bureaucratic procedures. Our administrative structure collectively fuels 'March mania', where we see the wasteful and thoughtless expenditure of 'unspent balances', which suddenly emerge from the accountants' pages as the financial year draws to a close. Since no critical piece of equipment or material can be easily purchased at short notice, there is the demoralizing spectacle of money being used for purposes, which are far removed from research and for many a casual observer, non-essential. There have been some recent attempts to re-examine financial and administrative procedures at the governmental level, but more needs to be done. It must be clearly recognized that the issues confronting the universities and academic institutions are varied and difficult to address. A concerted effort needs to be made to ensure that enhanced budgets can indeed translate, eventually, into improved performance.

Does funding for science have to be limited almost entirely to government sources? The contribution of private industry to the S&T sector is very small. Even in an area like pharmaceuticals, where research may hold the key to future success, investment by companies in academic institutions is negligible. There are also no major private foundations which support scientific research on a notable scale; in contrast to the situation in the US or UK, where private bodies have slowly acquired an important role. We need to devise mechanisms to enhance private sector involvement. Unfortunately, we are at present in a situation where industry perpetually seeks budgetary concessions from the government, with little thought of contributing to overall development. Many schemes introduced to promote a three-way collaboration between academia, industry and government appear to be devices by which private sector laboratories benefit disproportionately, contributing only in a limited sense to a collective research endeavour.

Despite the formidable set of problems faced by researchers and institutions, Mr Yashwant Sinha's new budget holds promise for science. A turnaround may be catalysed by the 2002 budget, which might help to halt the steady erosion in support for science. But, this is only a small, but nevertheless important step, in shoring up the scientific enterprise. A clearing does indeed seem visible amongst the clouds.

P. Balaram

CURRENT SCIENCE

Volume 82 Number 6 25 March 2002

EDITORIAL

Credit and discredit

A wonderfully provocative commentary appears in a recent issue of *Nature* (2001, **413**, 835). Writing about the allocation, or more correctly the misallocation, of credit, the Cambridge geneticist Peter Lawrence drew attention to the fact that it may indeed be time 'to bring justice to the allocation of credit'. Lawrence seemed incensed by the prospect of a brave new world of science where 'scientists are ranked like tennis players, measured by their numbers of papers, impact factors of the journals concerned, their position in the author list and the number of citations their papers receive'. To climb up the ranking list requires strategic accumulation of credit and Lawrence argues 'that a common way to build rank is to annex credit from junior colleagues'. Lawrence suggests that granting agencies 'ensure that those they pay to run research groups put caring for their groups first and swanning around the world or running companies second. They, as well as prize committees and those assessing job applicants, must cease rewarding those who misappropriate credit'. Lawrence's sharply-worded essay raises many contentious issues, problems of authorship (whose name must appear first in the bylines of scientific papers), the clever use of the conference circuit to build up a few stars, the treatment of research students by supervisors, 'competition within and between groups' and the increasingly damaging impact of the impact-factor measurement. While Lawrence's litany of complaints addresses the contemporary practice of science in Western laboratories with a focus on biomedical research, many of his concerns might indeed be viewed in a wider context.

Modern research has come a long way since the times of Newton and Faraday. The lone researcher toiling away in isolation is practically extinct. Research groups in many disciplines of science are large and the practice of science has become immensely competitive. In academic settings, groups have a pyramidal structure with the 'principal investigator' presiding over a team of graduate students working for Ph D degrees and post-doctoral fellows, who lie at the bottom-most rung of the academic ladder. Many areas of research require large-scale-collaborative effort and cooperation amongst members of a research group or even between groups. The sharing of 'credit' necessarily becomes a complex and sometimes unpleasant issue. Conventionally, the scientific success of group leaders is recognized by grants, awards and lecture invitations; much less credit seems to be distributed to other members of a group. But, in most instances successful attacks on scientific problems requires years of effort, steady direction by groups leaders and appreciable inputs into the training of apprentice scientists; at the end of the day consistent performance of laboratories over long periods of time can be traced back to their heads, who most often get the credit that is their due. There are inevitably and deplorably, cases of exploitation, where the origin of ideas and innovative technique are conveniently forgotten, especially when the advances emanate from the least well-positioned members of a research group. Lawrence uses an evocative metaphor: 'Students are like boosters on space rockets, they accelerate their supervisors into a higher career orbit, and, when their fuel is spent, fall to the ground as burnt-out shells'. Here Lawrence exaggerates, his perceptions clouded by images from some of the most ruthlessly competitive and immensely 'successful' research groups, which are sometimes held up as the standard bearers of the biomedical research enterprise. By and large, the majority of academic research groups still have reasonably healthy mentor–student relationships, with most supervisors forging a life-long bond with the students who pass through their laboratories. In a comment on Lawrence's essay, submitted shortly before he died, Max Perutz recalls that he did not author some papers with his collaborators when his contribution was not critical; but he says 'I had my reward in their lasting respect and affection, and it did not damage my scientific career' (*Nature*, 2002, **415**, 819). While most group leaders may not follow Perutz's path, the accepted norms for scientific authorship require at least some measure of contribution from all listed authors, most importantly from the head of a group.

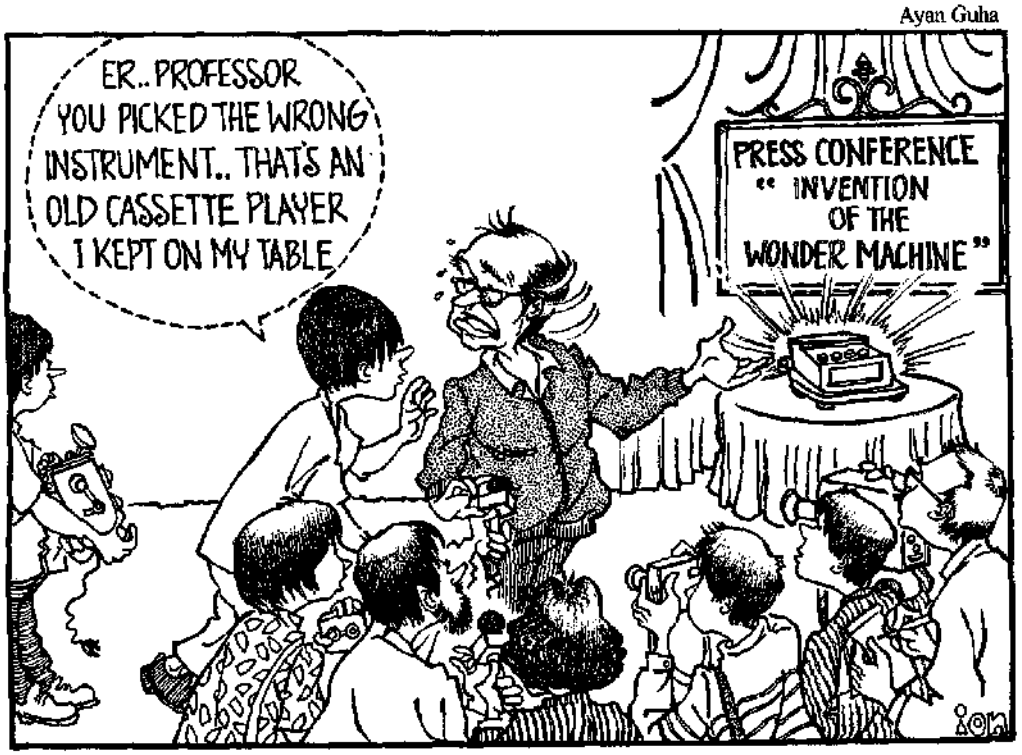
Is Lawrence voicing a predominantly Western concern or is there need to look at our practices in India too? Using the history of the discovery of HIV, the virus that causes AIDS, he argues that there are instances when 'scientific, legal and governmental systems not only failed to curtail but actually rewarded

unethical behaviour'. Our scientific system is particularly vulnerable, when group heads indulge in ethically questionable practices. The structures of our institutions are built to favour those who hold responsible positions, irrespective of their conduct. There are few mechanisms, even in the best of places, to redress difficulties faced by beginning researchers, who find it difficult to adjust to the sometimes unreasonable demands of over-ambitious supervisors. The cancer of the impact factor measurement has spread so widely in our academic evaluation system that we are now confronted in lectures by projections of publication lists, with journal impact factors conveniently displayed. We might do well to read Lawrence's lament: 'We should stop measuring success by where scientists publish and use different criteria, such as whether work has turned out to be original, illuminating and correct'.

Lawrence's central thesis that 'the misallocation of credit is endemic to science' is overstated. There have been a few celebrated examples of egregious error and he cites the case of Selman Waksmann, who received the 1952 Nobel prize for Medicine for his discovery of streptomycin. The antibiotic was really the product of work done exclusively by a student, Albert Schatz, but the professor quickly created a myth which had little place for a co-discoverer. And, as Lawrence notes 'the scientific community . . . helps to ensure that credit always flows up the ladder of rank'. But, we must also see the other side of the coin. Science progresses by the smooth linkage of generations, the torch passing from mentors to students in unobtrusive fashion. The relationships are not always smooth, a feature not uncommon in all human affairs. There are celebrated instances where students taste the highest scientific success, their mentors fading into the background.

Lawrence has, justifiably, stirred a hornets' nest. With crass competitiveness and increasing commercialism fuelling intellectual dishonesty and promoting exploitative practices in research laboratories, the scientific community needs to discourage many 'disturbing trends and practices' that Lawrence has highlighted. Left unchecked, these trends will bring discredit to science.

P. Balaram



CURRENT SCIENCE

Volume 82 Number 7 10 April 2002

EDITORIAL

Retirements and extensions

In a terse and typically, obscurely worded notification, the Government of India recently armed itself with the power to extend the age of retirement of scientists to 64 years. The operative clause states: 'Provided also that an eminent scientist of international stature may be granted extension of service up to the age of 64 years, if such extension is in the public interest and the grounds for such extension are recorded in writing'. Clearly, if the gazette notice of 27 February 2002 is to be literally interpreted, there must be a confluence of scientific eminence, international acclaim and public interest in the case of those individuals on whom the government bestows its munificence. A few years ago in the aftermath of the Fifth Pay Commission the retirement age for academics in many national institutions was raised by two years to 62, although some major scientific organizations like the Department of Atomic Energy have yet to follow suit. A few Central Universities have had an internally accepted procedure to 're-employ' their faculty up to the age of 65 years; an age also adopted by several prestigious national laboratories, set up under the peculiar device of a 'registered society', as the age of formal retirement for scientists, who direct these institutions. But, the vast majority of scientists and academics who work for government, autonomous institutions and universities retire at the age of 60 or 62. The government's, apparently unprovoked, initiative raises several questions. Indeed, the sensitive problems of retirement itself may be considered in the light of some broad issues. How should retirement ages be determined? Is uniformity desirable or is it important to retain wide discretionary limits? Is mandatory retirement desirable in an era of enhanced life expectancy and improved health standards?

Retirement marks a particularly difficult transition in life. Oliver Goldsmith eulogized retirement 'as friend to life's decline', but he also portrayed an idyllic, pastoral setting, which offered a retreat from a life of hard labour:

'How happy he who crowns in shades like these,
A youth of labour with an age of ease;
Who quits a world where strong temptations try,
And, since 'tis hard to combat, learns to fly!'

(*The Deserted Village*)

In the modern world, retirement for many means to be cut adrift from one's moorings, to float uncomfortably in an unfamiliar setting, as age takes its toll. The transition can be painful for those who have developed few outside interests; wrenching for those who have become accustomed to the perquisites of administrative power; devastating for scientists suddenly cut off from their research, which sometimes transforms unobtrusively from a professional activity to an impossibly, addictive hobby. In the West, retirement ages have generally been higher than in India, with scientists in Europe holding formal appointments till the mid to late 60s. In the US the retirement age for University faculty was 70 until 1994, when Federal laws prohibiting mandatory retirement on the basis of age, came into force. Since then, US universities have wrestled with the problems that have arisen once the age of retirement became a matter of personal choice. With the abolition of the 'retirement age', universities have had to re-examine their tenure policies to introduce mandatory review for senior faculty and to enhance the attractiveness of retirement packages.

Given the conditions prevailing in India, mandatory retirement from government employment must be seen as an absolute necessity. Retirement, undoubtedly, is a critically important parameter for institutional renewal. Since the concept of a 'permanent job' is inextricably associated with government employment, no mechanisms exist within academic institutions for easing out non-performers. Retirement, indeed, is the only way out for jettisoning deadwood and transferring salary burdens to the task of recruiting fresh entrants, who offer hope for a better future. Within the cloistered precincts of academia, many professors have productive research programs and laboratories that still attract students, as they approach retirement. Quite often, many academics establish independent careers almost on the verge of middle age and appear to have got set to play a good innings, just as retirement approaches. In the world of academia, most professors do not just pack up their bags and go home on the day of retirement. They, in fact, go about their normal business, much as though nothing has changed; gradually fading, as the loss of official status slowly erodes the functioning of their laboratories. Emeritus appointments

and sponsored research projects keep the flame flickering, but official retirement for most scientists, marks the end of a research career. It sometimes seems a shame to put out to pasture, individuals whose scholarship is undeniable; to lose teachers, whose talents can hardly be replaced. But, age does take its toll. Few individuals manage to maintain a high level of scientific productivity as they approach the age of retirement. Indeed, true academicians, with limited managerial skills, do poorly as they age; they do not successfully build large groups, with a structured chain of command, which can camouflage the slow decline of individual performance, behind a facade of collective activity.

Many institutions in India also face a fresh problem, as retirements begin to outstrip recruitments in several areas of science and engineering. The uneven rate of recruitment over a long period of time, partly the result of lethargy and administrative inaction in many organizations, has left departments top heavy, with the prospect of bunched retirements in the future. The loss of experienced teachers for many specialized courses in institutions offering advanced degrees is a matter of concern. Faced with these problems, institutions may, in the future, have to turn to devices like re-employment, extensions and recruitment of retired professionals on short-term contracts. But, at present the 'extension of service' seems aimed selectively at an important breed of scientists in India – the science administrators. Disturbingly, in government circles there is an almost complete lack of appreciation for the ground realities of the practice of science in laboratories; the administrators of science appear to be vested with an 'academic eminence', which has little to do with continuing scholarly achievement. The consensus appears to be that generalist administrators may be more important to furthering the development of science, than specialist researchers, who work unobtrusively, but successfully in their laboratories. Indeed, the best way for senior scientists to extend their careers appears to be to laterally transfer themselves into positions of administrative authority. Strangely, the label of 'irreplacebility' rests more comfortably on administrators than on scientists with specialized skills. Over the last few years support for emeritus scientist schemes has been diminishing, with the Indian National Science Academy downsizing and modifying its programs, that cater substantially to its fellowship, which presumably includes many of our most active scientists. In this context, the new spectacle of the extension of service to 'eminent scientists of international stature' is intriguing. This appears to be, clearly and unabashedly, a move to further perpetuate the notion that extensions of service for high-level managers of science are critically important in 'the public interest'. Unsurprisingly, even in the best of our scientific institutions the occupants of administrative positions are often associated with an aura of generally, exaggerated scientific eminence. If science in India is struggling it is not for lack of administrators; rather we need to maintain and enhance the pool of productive scientists by vigorously promoting recruitment and by introducing new, innovative schemes to tap the potential of retiring scientists. Enhancing inter-institutional mobility and removing administrative bottlenecks in making short-term contract appointments may facilitate continuation of active researchers. Unfortunately and unavoidably, it is administrators who rule the roost. In the true traditions of bureaucracy, even science administrators have learnt to feather their own nests; public interest and self-interest merge seamlessly. Louis XV of France, in the run-up to the French Revolution said famously: 'Apres moi le deluge' (translated crudely, 'after me the flood'). This is surely a sentiment to which most of our administrators of science, subscribe.

P. Balaram

Volume 82 Number 8 25 April 2002

EDITORIAL

Technology-driven science

The physicist Freeman Dyson is reported to have remarked that 'scientific revolutions are more often driven by new tools than new concepts'. This sentiment must seem like heresy to the many, who have a glamorous view of science as an enterprise, driven by an urge to understand natural phenomena, with periodic revolutionary upsurges when new concepts and insights appear to form in the minds of the most gifted practitioners of science. The quantum revolution in the early part of the last century and the dramatic upheaval in structural molecular biology in the 1950s, appear as marvellous examples of the way in which science progresses. Technology is seen as a fall out of science; the transistor, the laser and the integrated chip are corollaries of the spectacular march of fundamental physics; an extraordinary spectrum of pharmaceuticals and materials, molecular diagnostics and vaccines appear to be the consequences of over a century of unimpeded progress in chemistry and biology. Useful technology clearly follows major scientific developments. But, can technology drive science, as Dyson implies? Are there branches of science where technological developments direct the path of inquiry; where the motivations for research are shaped by the tools available?

Modern biology inevitably springs to mind as a discipline, where the march of technology has changed the face of biological research. In his characteristically acerbic style, the molecular biologist, Sydney Brenner reflected on the issue of 'hunters and gatherers': 'I was taught in the pre-genomic era to be a hunter. I learnt how to identify the wild beasts and how to go out, hunt them down and kill then. We are now, however, being urged to be gatherers, to collect everything lying about and put it into storehouses. Someday, it is assumed someone will come and sort through the storehouses, discard the junk and keep the rare finds. The only difficulty is how to recognize them'. (*The Scientist*, 2002, **16**, 14). Brenner targets here the genomics programs, which seek to sequence as many genomes as possible, cataloguing the data, with no immediate questions in mind. Rather than 'hunt' in the traditional manner for disease genes, the new approaches involve sifting through reams of sequence data which have been 'gathered', in the hope that subsequent analysis will reveal the genes of interest. Brenner's heart appears to lie with the 'hunters', practitioners of classical 'hypothesis-driven science'; his ire directed at the 'gatherers', votaries of the rapidly advancing area of technology-driven science.

Biology has been transformed over the past twenty years by an array of powerful technologies, most notably the techniques to manipulate, clone, amplify and sequence DNA. Indeed, *Time* magazine was probably not guilty of its customary hyperbole, when it described the new DNA technologies as 'the most awesome and powerful skill acquired by man since the splitting of the atom'. Max Perutz describes Fred Sanger's pathbreaking work on DNA sequencing, thus: 'Sanger did not work *à la* Popper by formulating hypotheses and then performing experiments to test them by falsification. Instead he invented new chemical methods capable of solving problems that no one else had even approached, since it was believed they would defy solution. He did not measure his discoveries against existing paradigms, because they opened new worlds where no paradigms existed'. (Max Perutz in a review of P. B. Medawar's *Advice to a Young Scientist*). Sanger and indeed all the great analytical and synthetic chemists over the ages have worked in a manner that defies a Popperian classification; their science clearly not hypothesis-driven, but nevertheless wonderfully creative and inventive. Perutz adds: 'The process of invention is imaginative, but so far as I know no philosopher has thought it worthwhile to analyse it, because the mind's creative process is impenetrable'. DNA sequencing has come a long way from Sanger's original laboratory methods; it is, today, an immensely powerful technology. Indeed, in the sequencing centre that bears Sanger's name, array upon array of marvellous machines churn out genome sequence data that are automatically stored in banks of computers. The 'gatherers' are at work here and in many superbly organized facilities across the world. As genome after genome succumbs to the relentless onslaught of organized sequencing, the science of biology stands at the crossroads. Will the 'gatherers' transform into 'hunters'? Or will a new breed of hunters evolve, who like hyenas will emerge from the shadows to rip out the most succulent parts of fallen genomes. There is already some evidence for the emergence of a new species; bioinformaticists, who hope to sift through

the mass of genome sequence data, to reveal the foundations on which biology's future advance will rest.

The study of genome sequences is not the only area where technology holds sway in biology. The developing science of genomics has two distinct components; functional genomics which aims to bridge the gap between genotype and phenotype and structural genomics, which promises to determine the three-dimensional structure of all the protein products, that result from the translation of information encoded in genes. Protein structures, in turn, may reveal the nature of their functions, providing another path towards understanding the control and flow of information in biology. Central to the area of functional genomics are powerful, automated methods for analysing simultaneously the expression of hundreds of genes, based on micro-array technologies. Structural genomics is fuelled by high throughput X-ray crystallography, with synchrotron beamlines and robotic crystallization procedures providing some of the tools which will hasten the process of structure determination; an activity that has traditionally been slow and laborious. High-field NMR spectroscopy with its monstrously expensive instrumentation is also being recruited to the task of determining protein structures, albeit limited by molecular size, but offers the advantage of bypassing the uncertainties of crystal growth. With biology awash in data, computers and computer scientists have become central to the field, a development largely unanticipated even thirty years ago. The new biology has been described as 'discovery science', requiring 'large-scale facilities for genome-wide analyses, including DNA sequencing, gene expression measurements and proteomics'. (Aebersold, R., Hood, L. E. and Watts, J. D., *Nature Biotechnol.*, 2000, **18**, 359). 'Discovery science' appears to be a term that finds favour in industry, promising as it does the prospect of new and patentable discoveries. Hypothesis-driven science, in comparison, seems a curiously inefficient, esoteric, academic exercise.

As the tide of technology-driven science rises, particularly in biology, we need to consider mechanisms for the support and conduct of research in areas, which depend heavily on the use of expensive and sophisticated equipment and, sometimes on alarmingly expensive consumable reagents. Not too long ago, biology laboratories in our institutions housed little by way of expensive equipment. Today, the demands of biology outstrip most other sciences. With growing awareness of new technologies, funding organizations in India face a new problem; requests for acquisition of extremely sophisticated research tools, by institutions with limited capacity to maintain and use such equipment. A new breed of scientists is beginning to emerge, 'the acquirers', whose sole aim, sometimes, seems to be the acquisition of sophisticated technology, which is often neither used nor maintained. Abetted by agents for companies which market the new technological tools of biology, institutions are being driven to purchase expensive equipment, which they do not always require. The 'acquirers' also betray a technical innocence, which can be quite disconcerting; they seriously believe that once the technology is in place, good science will follow. The alarming shortage of scientists and expert technicians in India, who are trained and comfortable with the cutting edge technologies of biology, must be addressed, if we are to effectively exploit the explosive development of new techniques. Acquiring instrumentation alone may hardly be adequate.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The recesses of the mind

So at the present time I seem to be thinking rationally again in the style that is characteristic of scientists. However this is not entirely a matter of joy as if someone returned from physical disability to good physical health. One aspect of this is that rationality of thought imposes a limit on a person's concept of his relation to the cosmos.

—John Nash
Autobiographical Essay
Le Prix Nobel, 1994

The Oscars are to Hollywood and films, what the Nobel prizes are to science. There are, of course, more Oscars; even awards for the best supporting actors and actresses, costume designers, screenplay writers and music directors, unlike in science where the major prizes go to the big stars, with the supporting cast generally forgotten. This year's Oscar ceremonies were especially significant for Indians, who, by and large, were rooting for *Lagaan* and Aamir Khan. As the interminable Oscar show on TV dragged on, I was struck by the number of times the movie, *A Beautiful Mind* was mentioned in various categories. A few weeks later, I accidentally ran into a prominent Indian sportsman, who on hearing that I was a scientist immediately asked: 'Have you seen the lovely movie, *A Beautiful Mind*?' By this time I knew that this was a movie about a scientist. A few days later I acquired a copy of Sylvia Nasar's book *A Beautiful Mind*. And, it is this book which has since occupied my thoughts.

Nasar chronicles the life and times of John Nash, the mathematician who received the Nobel prize (or more correctly, the Nobel Memorial Prize, instituted by the Bank of Sweden) for Economics in 1994. Until the prize, Nash was a forgotten genius; but the call from Sweden catalysed not only Nasar's remarkable biography, but also the Hollywood movie. The Princeton University Press, undoubtedly driven by the surge of public interest, has produced *The Essential John Nash*, edited by Harold Kuhn and Sylvia Nasar. The story of Nash's life is riveting; a compelling mix of madness and pure creativity. It is the story of genius trapped in the mists and shadows of schizophrenia. It is the story of a deeply troubled man, who laid the foundations of game theory, developing concepts that have had a far-reaching impact on disciplines as diverse as economics and evolutionary biology.

Nash did his remarkable work as a graduate student in the Department of Mathematics at Princeton University, in a spectacular creative burst in 1949–50. His first paper appears to have developed from an undergraduate course in international trade, that he took at Carnegie Tech in 1948, 'simply to fulfil degree requirements'. His analysis of 'The Bargaining Problem' may not have excited pure mathematicians, but it signalled the beginning of a new approach to economic analysis. It also heralded the arrival of an original thinker, with an ability to make the intuitive leaps, which separate men of genius from others. Soon after, Nash laid the cornerstone of modern game theory, when he defined the concept of an equilibrium, which would eventually bear his name. Today, the Nash equilibrium is an idea which influences not only the applications of the theory of games in economics, but its imprint has extended to the idea of an 'evolutionarily stable strategy' in biology. Nash's seminal work led to an extraordinarily slim Ph D dissertation of only twenty-seven pages. Unaware of the impact that his work would eventually have, Nash himself seemed untouched by success in pointing towards strategies for solving practical problems; he appeared more consumed by the need to prove himself as a 'pure mathematician'. It is in the years after his extraordinary period as a graduate student at Princeton that the first signs of the tragedy that was to follow become evident. Nasar's biography paints a haunting picture of his period at the RAND Corporation, a symbol of the collective paranoia of the early years of the Cold War. Nash's period on the faculty of MIT marked his major success as a 'pure mathematician' in attacking the 'embedding problem' in differential geometry. It also marked, as his biographer notes, the beginning of his 'slide from eccentricity to madness'. Only thirty when he experienced the first crippling episode of paranoid schizophrenia, Nash's academic career lay ruined. From 1959 to 1970 Nash led a tortured existence and from the 1970s until the Nobel prize, Nash was a 'phantom' who haunted the campus of Princeton University. Nasar's

description of the Swedish Academy's deliberations, before deciding to honour a man, who had stepped past the borders of rationality, makes gripping reading. There is also a poignant episode. In a move, unprecedented in the Nobel awards process, one of Nash's colleagues at Princeton is forewarned, to prepare both the recipient and his surroundings for the inevitable glare of publicity that follows the prize. In Nash's case in the years that followed, every detail of his tortured years becomes a matter of public record in Nasar's, at times, brutal but riveting biography.

But more than anything else the story of John Nash, is the story of a disease; an illness of the mind that is estimated to affect a staggering one per cent of the world's population—schizophrenia. The term itself was coined by Eugen Bleuler in 1908 and for long has been associated in popular perception, with a 'split personality'. Rather, it is a deep-rooted disorder of the brain, which profoundly affects behavioural patterns. Schizophrenics may have difficulty in distinguishing real from unreal experiences, be gripped by delusions and sometimes incapable of normal emotional responses. As a crippling disease of the mind, the spread of schizophrenia over the past century has been frightening. The evolution of the disease, in history, is shrouded in mystery; its recognition and management using anti-psychotic drugs is of relatively recent origin. The efficacies of treatment regimens are still a matter of serious debate. In the not too distant past, as in the case of Nash, psychotherapy, insulin and electric shock treatment have been advanced as palliatives, although their efficacy remains uncertain. The first major signs of the disease appear in young adulthood, apparently spontaneously, with limited correlation to heredity or environment. The symptoms are disparate. A general consensus that seems to be emerging is that the best course of management may be a combination of anti-psychotic drugs and a supportive environment. Nash's dramatic improvement in the years immediately preceding the Nobel prize may have been facilitated by the 'structured, predictable and supportive' environment provided by his wife, the familiar surroundings of Princeton and the company of sympathetic mathematicians. In current approaches to the management of the illness, family support has acquired a major role.

The 1990s were declared the 'decade of the brain'. But, the decade has long since faded into history. Despite the enormous progress of neuroscience over the past several years and the increasing power of new methods for probing brain function, functional magnetic resonance imaging (FMRI) among them, disorders of the brain remain among the most poorly understood of the illnesses that affect man. The case of schizophrenia clearly indicates that neither genes nor environment, nature nor nurture, can be entirely held to be the sole causative agents of the disease. Like many other poorly understood disorders of the brain, which remain lumped together as mental illnesses, in the case of schizophrenia too, management of the disease may be more accessible than a cure.

The story of John Nash raises many questions about creativity and the mind. In her prologue to her biography of Nash, Sylvia Nasar notes: 'Many great scientists and philosophers . . . have had similar strange and solitary personalities. An emotionally detached, inward looking temperament can be especially suited to scientific creativity.' She notes that exercise of a creative mind may also provide a defence 'against anxiety stimulated by conflicting demands for detachment and human contact'. Nasar recalls Wordsworth's famous lines:

> I was taught to feel, perhaps too much
> The self-sufficing power of solitude.

Indeed, it was Wordsworth who spoke of 'that inward eye which is the bliss of solitude'.

In reflecting on creativity, Nasar notes that 'men of scientific genius, however eccentric, rarely become truly insane—the strongest evidence for the potentially protective nature of creativity'. But on reading *A Beautiful Mind* I came away with a feeling that schizophrenia is an affliction that may defy our understanding for some time to come. Its victims will often continue to be 'incomprehensible and inaccessible' to those around them. While modern science may bring to bear its most powerful weapons to probe the brain, the secrets of the mind may be very hard to prise out. The wellsprings of creativity emerge from the deepest recesses of our minds; but for some of the most gifted amongst us, so too do the demons which haunt the mind.

P. Balaram

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EDITORIAL

Bioinformatics: Blowing up a balloon

A few weeks ago, the trade fair Bio2002 was held in Bangalore. This is becoming an annual event, organized by the Karnataka state government's Vision Group on Biotechnology, emphatically reiterating the conviction that the biotechnology industry will lead the economic boom of the future. Two states in India, Karnataka and Andhra Pradesh, have been particularly supportive of the fledgeling biotechnology industry, taking the initiative to establish 'parks and corridors', welcoming the setting up of new biotech ventures within their boundaries. The thrust in Indian biotechnology comes well over a decade after the gold rush in biology began in the United States. Indeed, the beginnings of the biotechnology revolution can be traced to the early days of DNA cloning in the 1970s, when work done in Herbert Boyer's laboratory at the University of California, San Francisco, led to the establishment of Genentech, signalling the birth of the biotechnology industry. True historians will, of course, note that the 20th century also marked the coming of age of 'classical biotechnology', a subject that traditionally deals with the production of useful chemicals by biological processes. While fermentation, breweries and alcohol have been in vogue for centuries, the use of microbial fermentation for production of antibiotics is clearly a 20th century landmark, hastened by the need for penicillin in World War II. The more modern phase of biotechnology, which gathered momentum in the 1980s, was initially driven by the need to produce pharmaceutical proteins, insulin foremost among them, using the powerful methods of recombinant DNA technology. Despite nearly twenty years of effort, the number of pharmaceutical proteins that are major successes at the marketplace is limited; insulin, erythropoietin, growth hormone, colony-stimulating factors and interferons are some examples. In India the first efforts to produce recombinant insulin began about twenty years ago, with the Department of Science and Technology (the Department of Biotechnology (DBT) was still only a gleam in the eyes of its promoters) playing a highly supportive role. Industry however, resolutely avoided investments in biological research, showing some signs of interest only in the late 1990s. Financial institutions were generally wary of biotechnology ventures; a far cry from the present situation, where both government funds and venture capital are available to start-up companies. Recombinant insulin is still not produced and sold in India; the foreign product rules the roost. But once again there are stirrings in industry and maybe a locally-produced insulin will soon be a reality. The first recombinant DNA-derived products in India have reached the shelves of pharmacies; these are, however, products in which bioprocess development and downstream processing have been the key elements standardized locally. Little or no basic research has been involved in biotechnology product development; an historical course of industrial growth in India, that parallels the rise of the pharmaceutical industry in a legal climate that did not recognize product patents. If organic chemistry and chemical technology were the key disciplines for the pharmaceutical sector thus far, fermentation and biopurification may be the pillars of pharmaceutical biotechnology in the future.

In the last few years biotechnology has expanded immeasurably, fuelled by the spectacular progress in biological research. Its applications have made major inroads into the areas of agriculture and medicine. Transgenic crops and recombinant vaccines are rapidly becoming commonplace. Advances in stem cell research and the prospects of 'therapeutic cloning' may be harbingers of a new biological revolution. But if any single recent event in biology has captured the public imagination, it is the determination of the human genome sequence. To many uncritical observers, it appears that the 'secret of life' has been unravelled. To others the 'book of life' is waiting to be read and interpreted. With biology awash in genome sequences, which need to be annotated and translated into meaningful biochemical insights, computational biology has acquired a new importance. And, in the popular mind a new and important discipline of science, bioinformatics, has emerged. If its prophets are to be believed, bioinformatics is central to biology, today. The nature of biological information that must be stored, catalogued, indexed and understood has grown beyond nucleic acid sequence data; there are growing databases on protein three-dimensional structures, gene expression profiles, two-dimensional protein maps generated by electrophoresis and a bewildering range of information emerging from studies using nucleic acid and protein arrays. A recent commentary is tantalizingly titled: 'Creating a bioinformatics nation' (L. Stein, *Nature*, 2002, **417**, 119).

The rise of biotechnology and bioinformatics has also led to the emergence of a new breed of high profile salesmen for these disciplines, who are motivated by the irresistible smell of commercial success. We are promised 'designer drugs' which will presumably be optimized to suit a patient's 'genetic background'; even as the struggle to develop new drugs for difficult diseases has proved long, costly and often, unsuccessful. But, the promise of biotechnology and bioinformatics has been so widely touted, a new problem has begun to surface in India – the mushrooming of biotechnology and bio-informatics courses, which appear remarkably attractive to students. In the 1980s, in one of its most successful programs, the DBT supported the starting of M Sc (Biotechnology) programs, in a few chosen institutions. These departments were well funded, attracted and recruited good faculty and for a period of several years produced a new breed of biology students, trained in the developing techniques of molecular biology. However, more recently, the number of departments have swelled; many lacking the necessary facilities or faculty. B Sc courses have also been started in colleges. Most disturbingly, in many places, 'biotechnology' departments have been separated from the core disciplines of biochemistry and microbiology. There are a very large number of 'biotechnologists', with Masters degrees, in search of employment. In this setting, the enthusiasm of the Ministry of Human Resource Development (MHRD) for starting B Tech (Biotechnology) programs in the Indian Institutes of Technology (IITs), must be viewed with caution. Many IITs do not have even the minimum number of faculty trained in biology to sustain these courses and may have to resort to the device of inviting 'gues un- happy situation for institutions that have always maintained high standards of rigour in their teaching programs.

Even more alarming than the proliferation of biotechnology degrees, is the veritable explosion of bioinformatics courses. Bioinformatics in India has been conceived as the product of an illegitimate fusion between information technology (IT) and biotechnology (BT). Even as the IT industry experienced a mild slump, the many teaching shops, that cater to the needs of students to learn computer languages, have begun to rediscover themselves as institutions for teaching bioinformatics. In a cursory survey, I discovered advertisements from 'institutions' in the Bangalore suburbs, which offered bioinformatics courses covering an impressive range of subjects in their 'modules'. The topics ranged from *Biochemical and Biophysical Techniques*, *Molecular Biology*, *Genetic Engineering*, *Neural Networks and Artificial Intelligence*; subjects which are taught in universities and advanced institutions. In addition, the advertised modules include, *Chemoinformatics*, *Drug Discovery* and *Combinatorial Chemistry*; courses for which our best institutions would be hard pressed to find willing and competent instructors. These bioinformatics shops, which now sprout in all our major cities, attract unsuspecting students, who pay substantial fees. Typically, the costs can mount to as high as Rs 1,00,000 (over thrice the annual fees for B Tech programmes at IITs), for courses of dubious content and doubtful utility. While some courses list minimum requirements for students, I discovered one 'university', which advertised an M Sc bioinformatics course open to students with an Arts background. This institution had a succinct description of the discipline: *Tools and techniques from computer science, biology and mathematical statistics are used to provide a variety of molecular maps of genomes, including DNA and protein sequences.* The rationale for students to pay for these courses is clearly laid out in another pamphlet: *The trend to out source low-cost research and development in biotechnology is increasing. This provides an immense opportunity for Indian companies for overseas contract research.*

The bioinformatics courses are money spinners for those who run them; attracting parents and students, who sometimes invest unaffordable amounts in the search for a guaranteed future. These courses advertise the fact that they teach students how to use bioinformatics software, all of which are freely available on the internet. There is the seductive promise of wonderful career opportunities for those 'who understand both biology and computers'. Some questions spring to mind. Are teaching shops that have sprung up like pizza parlours, without facilities or faculty, teaching biology so effectively that they transform students, irrespective of background, into bioinformaticists? Or, are we creating a large pool of poorly trained certificate holders, who will struggle to obtain jobs in the promised biotechnology boom?

The rise of the teaching shop is a peculiar by-product of the IT age. There are new and sometimes, disturbing trends that are surfacing. The spectacle, of faculty of publicly-funded institutions teaching as 'guests' in private teaching shops, offering courses which are not available within the precincts of traditional institutions is discomforting. For years, our schools and colleges have faced the issue of conflict between the conventional classroom and the private tutorial class; with teachers often preferring the latter. The commercialization of education, with little control on course content and misleading advertisements, is a sign of our times. The fact that this trend may be tacitly supported by governments is a matter of concern. The balloon of bioinformatics is being assiduously expanded. When it blows up, many unsuspecting students will be poorer.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The poetry and prose of evolution

'There is grandeur in this view of life.'

—Charles Darwin
Origin of Species

'I have found no documents of human thought more exciting than the notebooks that Darwin filled in London as a young man in his late twenties, just returned from five years aboard the Beagle. He had the key to a new way of life, and he knew it. His mind ranged over the entire intellectual landscape, from biology to psychology, morality, philosophy and literature. Evolution by natural selection impinged on everything'.

—Stephen Jay Gould
On Original Ideas
Natural History, 1/83

Our view of Nature and the living world has been profoundly influenced by Darwin's remarkable insights, in the years that followed his celebrated voyage of discovery, aboard the *Beagle*. Darwin's views on the evolution of life and his enunciation of the principle of natural selection, forever, altered the face of biology. In the span of almost a century and a half that has passed since Darwin, his views on man's place in nature have been championed by some of biology's brightest stars, in a lineage that stretches from Thomas Henry Huxley to Ernst Mayr. But, the public perception of the Darwinian world view has been shaped by many powerful writers, who have presented our changing insights on man's origins, to the world at large. Stephen Jay Gould, more than anyone else spread the message of Darwinism, explained 'natural selection' and attempted to educate a world that 'has yet to make its peace with Darwin and the implications of evolutionary theory'. When Gould died on 20 May, at the age of 60, the world of science lost one of its most articulate spokesmen and the fields of biology and palaeontology one of their most distinguished practitioners. Beginning in 1974, for over a quarter of a century, Gould wrote an uninterrupted monthly column in *Natural History*, entitled 'This View of Life', a phrase borrowed from Darwin's concluding sentence of *Origin of Species*. And, in his essays Gould displayed an unmatched ability to make the most abstruse of science intelligible and to convey a continuous and undimmed enthusiasm for the wonders of nature. Gould's writings grew beyond the monthly essay to encompass an astonishing range of books, often collections of his essays. In the last year of his life he produced his final work of scholarship *The Structure of Evolutionary Theory* (Harvard University Press, 2002), which occupies a formidable 1464 pages. For many who have wrestled with the subtleties of natural selection, the role of chance and design in biology, the difficulties of interpreting an imperfect fossil record and of course, the philosophical implications of Darwinian evolution, Gould's writings have served as a beacon. Unmatched in the ability to strip layers of incomprehensible complexity from a problem, Gould developed a large, devoted readership, who will undoubtedly miss him.

Gould's professional research revolved around a concept that appeared at odds with the gradual, purposeless march of conventional Darwinian evolution. Together with Nils Eldredge, Gould advanced the idea of a 'punctuated equilibrium'. The Gould–Eldredge view of speciation was a clear departure from the gradualism implicit in a Darwinian model. In Gould's words: 'Species arise in a geological moment – the punctuation (slow by our standards, abrupt by the planet's). They then persist as large stable populations on substantial geological watches, usually changing little (if at all) and in an aimless fashion about an unaltered average – the equilibrium'. The proclamation of a 'punctuated equilibrium' as an evolutionary device was met with considerable derision by strict Darwinians; unkindly characterized as 'evolution by jerks' by its detractors. Ironically, creationists viewed the Gould–Eldredge suggestions as a sign of the cracking of the edifice of evolutionary theory, a development which required Gould to set the record straight in the 1980 Arkansas creationism trial. Gould was a master of the metaphor; he likened Darwinian gradualism to 'pushing a ball up an inclined plane', while punctuated equilibrium appeared like 'climbing a staircase'. Gould revelled in public debate and controversy and in his last book *The Structure of Evolutionary Theory* he provides a spirited counter-attack on his critics, Richard Dawkins among them. In a 1990 review of Gould's *Wonderful Life*, Dawkins laments: 'If only Stephen Gould could think as clearly as he writes! This is a beautifully written and deeply muddled book'. Ernst Mayr in a tribute marking 25 years of Gould's column in *Natural History* is more charitable: 'Whether right or wrong, Steve (Gould) is always stimulating and this is perhaps where he has made his greatest contribution – in awakening in thousands, if not millions, of his readers an enthusiasm for the secret of this wonderful world of ours'. Gould's long-time colleague at Harvard, Richard Lewontin points to an ironic similarity in the 'Gouldian view of evolution' and a passage from the Ecclesiastes: 'For the race is not to the swift, nor the battle to the strong, neither bread to the wise man, nor yet riches to men of understanding . . . but time and chance happeneth to all'. (*Natural History*, 11/99).

Gould was diagnosed as suffering from a rare and serious abdominal cancer in 1982, 'with a median mortality of only eight months after discovery'. His response was an extraordinary essay 'The Median Isn't the Message'; a wonderfully

positive interpretation of depressing statistical distributions. Gould noted: 'The distribution was indeed strongly right skewed, with a long tail (however small) that extended for several years above the eight month median. I saw no reason why I shouldn't be in that small tail . . . .' To Gould this was 'a personal story of statistics, properly interpreted, as profoundly nurturant and life-giving'. Gould's essay must undoubtedly be the most persuasive argument for the importance of carefully analysing statistics and a fitting rejoinder to sceptics like Hilaire Belloc, whom Gould quotes: 'Statistics are the triumph of the quantitative method, and the quantitative method is the victory of sterility and death'. Gould walked the tightrope between the twin careers of a professional scientist and popularizer of science, particularly a view of life that emerges at the crossroads of biology, geology and palaeontology. His opening sentences in a tribute to one of the 20th century's greatest public expositors of science, Carl Sagan, underline the difficulties of this dual role: 'As Saul despised David for receiving ten thousand cheers to his own mere thousand, we scientists often stigmatize, for the same reason of simple jealousy, the good work done by colleagues for our common benefit. Because we live in a Philistine nation filled with Goliaths, and because science feeds at a public trough, we all give lip service to the need for clear and supportive popular presentation of our work. Why then do we downgrade the professional reputation of colleagues who can convey the power and beauty of science to the hearts and minds of a fascinated, if generally uninformed public.' (*Science*, 1997, **275**, 599).

Gould's ability to integrate seemingly unrelated subjects in explaining Nature's grand design was one of the most engaging features of his writing. And, of course, he frequently turned to poetry. In wondering about the amazing organization of life, Gould was often confronted (and indeed, all of us are), with the Darwinian problem of ascribing an important role to chance in the evolution of life. Biology's greatest paradox is the emergence of organized life from chaotic randomness. In wrestling with this problem Gould noted Alexander Pope`s 'facile solution` which 'simply banished the very idea of chance by a decree of faith':

All nature is but art unknown to thee;
All chance, direction which thou canst not see;
All discord, harmony not understood;
All partial evil, universal good.

Gould, while musing on the teaching and learning of science noted that 'most quotations are fabricated' and added that 'we know many great literary lines by a standard misquotation rather than accurate citation`. As an example he chose the standard misquote, 'a little knowledge is a dangerous thing' and went on to analyse Pope's famous lines:

A little learning is a dangerous thing;
Drink deep, or taste not the Pierian spring;
There shallow draughts intoxicate the brain,
And drinking largely sobers us again.

Gould, disarmingly, notes that a turn to an encyclopedia and the venerable Oxford English Dictionary established the meaning of 'Pierian spring`, allowing him then to expand on the theme of learning.

But, my favourite in the Gould collection is his analysis of Tennyson's famous line often used as a 'canonical descriptor' of Darwinian evolution: 'Nature red in tooth and claw'. Ironically, Tennyson's *In Memoriam*, a long and as Gould notes, generally unread poem, appeared in 1850, while Darwin chose to unveil *The Origin of Species* only in 1859. But, Tennyson's ode to his friend Arthur Hallam, had a remarkably prescient view of science:

A time to sicken and to swoon
When Science reaches forth her arms
To feel from world to world, and charms
Her secret from the latest moon?

Behold, we know not anything;
I can but trust that good shall fall
At last – far off – at last, to all
And every winter change to spring.

Gould notices a forerunner of the Darwinian world:

Are God and Nature then at strife
That Nature lends such evil dreams?
So careful of the type she seems,
So careless of the single life.

(A poetic allusion to the stability of species, while unmindful of individual deaths.)

The pitiless and often misleading metaphor of natural selection as 'survival of the fittest' found its echo in Tennyson:

Who trusted God was love indeed
And love Creation's final law –
Though Nature, red in tooth and claw
With ravine, shrieked against his creed.

(S. J. Gould, *Natural History*, 1992, **11**, 14.)

Discussions of creation, nature and evolution abound in poetry. My own favourite from William Blake might serve as a tribute to Gould:

Tiger! Tiger! burning bright
In the forests of the night,
What immortal hand or eye
Could frame thy fearful symmetry?

When the stars threw down their spears,
And water'd heaven with their tears,
Did he smile his work to see?
Did he who made the Lamb make thee?

Stephen Gould did not worry about 'rhyme or meter' in his writings. But, if we were to anoint a poet laureate for science it would be Stephen Gould. At times, his prose was sheer poetry.

P. Balaram

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EDITORIAL

The two faces of simulation

Simulation is a respectable scientific activity. Even since computers swept the world of science, simulation has been a favoured pastime of engineers, physicists, chemists and even biologists. It is comforting, and often comfortable, to work with mathematical models, which will hopefully, predict the behaviour of real world systems. Increasingly, powerful, number crunching, computers are being used to predict the course of monsoons, the properties of atoms and molecules and to chart the spread of pathogenic organisms that cause infectious disease. The protein folding problem, in structural biology, is a popular windmill; many simulators, like Don Quixote set their own targets and claim success. Computer simulations are at the heart of subjects as diverse as fluid mechanics and computational biology and central to every aspect of engineering design. Simulations, sometimes disguised as projections, enter our daily lives whenever we have elections; forecasts, apparently supported by quantitative analysis, both right and wrong, abound. In modern science, simulations are ubiquitous. It was therefore with some amusement that I greeted the news from the football World Cup, that the FIFA's disciplinary committee had decided that their target was *simulation*. The immediate provocation was the faking of an injury by the Brazilian striker Rivaldo in the match against Turkey. The act, which attracted a red card for a Turkish player and a post-match fine for Rivaldo, when the FIFA committee recognized his simulation of injury, is a commonplace occurrence on soccer fields. When I turned to the dictionary it became clear that the common meaning of *simulate* is *to make a pretense of*. Indeed, the term 'simulation' often appears to be used in a pejorative sense and as in the case of football, it simply appears to be another description of the act of faking. Simulation appears widespread in sport and is at its most obvious in some arenas, like the wrestling circuses on television. Sometimes, simulation is more sophisticated and hard to detect, as in the case of fixed cricket matches. But, as Rivaldo must have realized, the resolution of the close-up video camera shot can distinguish a genuine injury from a clever simulation. Reflecting on Rivaldo's predicament, I came to the inescapable and obvious conclusion that the word *simulation* must have different meanings in sport and science.

But, does simulation, in the sporting sense, occur in science? Faking is not an entirely unknown phenomenon in science, although the term 'simulation' is almost never used to describe the concoction of fraudulent data. In thinking about fraud in science, several questions spring to mind. Is dishonesty rare in science? Are there many forms of deviant behaviour amongst scientists? Does right always triumph over wrong in the world of science? Many years ago, David Hull drew upon the emerging field of sociobiology, to conclude that unlike many others, 'research workers police themselves coldly, dispassionately, almost cruelly' (*Animal Behaviour*, 1978, **26**, 685). Hull came to his rather naive conclusion, even as science was poised to move explosively forward across a broad front of disciplines. In a critique that appeared a few years later, a more realistic assessment was forthcoming: '... while science does have certain means through its norms, to encourage honesty, it must also reflect the nature of the society in which it is nurtured. A corrupt society will produce a corrupt science' (Manwell, C. and Ann Baker, C. M., *Search*, 1981, **12**, 151). The issue of misconduct in science has been openly debated in the United States and Europe over the past several years; these discussions have been fuelled by many famous cases of publication of doctored data, with the John Darsee and Mark Spector cases from Harvard and Cornell attracting much attention. The Baltimore–Imanishi Kari imbroglio lasted for what appeared to be an eternity, throwing up several questions on the tactics of investigators, who sometimes turn a legitimate inquiry into a witch hunt. Many of these cases were from the field of biomedical research, where competition is intense, the rewards for success immense and the pace of advance frenetic. To scientists in distant disciplines, these cases of data fabrication and manipulation seemed to be determined by the mores of a field, that appeared to be advancing with reckless abandon. Although there have been a fair number of frauds in more sedate fields, these have rarely been characterized by the imagination and effrontery displayed by the 'simulators' in biomedical research. Physics and the burgeoning disciplines of 'nanomaterials' and 'nanotechnology' have therefore been shocked by a new, high profile case of apparent 'simulation'.

In a remarkable period between 2000 and 2001, researchers at Bell Laboratories produced one spectacular result after another, in what appeared to be a major revolution in molecular ('nano') electronics. The successes of

the Bell Labs group ranged from the development of organic photovoltaic diodes, field effect transistors made of films of pentacene, observation of superconductivity at 117 K in a doped fullerene and most dramatically, the announcement of the single molecule transistor. Most of the papers reporting these findings appeared in *Science* and *Nature* attracting widespread acclaim. The path to Stockholm appeared clear until allegations of data faking appeared in editorial offices. The 23 May issue of *Nature* (p. 367) displays two figures from separate articles in *Science* and *Nature*; the experimental curves appear identical down to the last wiggle of noise, although the experiments are different. And, as more people scrutinize the prolific outpouring from the Bell Labs, more worms appear to be surfacing. In the eye of the storm is the young star Jan Hendrik Schön, who at 31, has an extraordinary output of over 100 papers, many of them in high impact journals. In the background are senior and respected colleagues and coauthors. For a while it seemed that Schön had the Midas touch; but now his world appears to be crumbling as questions of reproducibility and authenticity of data are raised by colleagues and competitors (*Nature*, 2002, **417**, 367 and 473; *Science*, 2002, **296**, 1376 and 1584; *Physics World*, June 2002, p. 5). Are the Bell Labs results a case of 'simulation'? The final verdict must come only when a full-fledged inquiry report becomes public. But, one might recall a famous allegation made in 1986 that Stanley Prusiner's claim of an infectious protein particle, the prion, was questionable (Taubes, G., *Discover*, 1986, **7**, 28; Garfield, E., *Current Contents*, 1987, **14**, 88). Prusiner, of course, established his claims to the satisfaction of the Nobel committee, which awarded him the Medicine or Physiology prize in 1997.

Allegations of 'simulation' are often hard to substantiate and in many cases can do irreparable damage to the accused. In an extraordinary episode, the Austrian biologist Paul Kammerer was suspected of faking the data he produced, demonstrating the inheritance of acquired characteristics (Lamarckian evolution) in the amphibian *Alytes obstetricans*. Kammerer's work attracted attention in the early 1920s, resulting in remarkably vituperative attacks in the columns of *Nature*. He shot himself, 'on an Austrian mountain path', in 1926. Arthur Koestler's narration of the life and times of Paul Kammerer (*The Case of the Midwife Toad*, Random House, 1972) must remain the most compelling investigation of a case of alleged misconduct. But, we might do well to recall one of the obituary references to Kammerer that Koestler quotes:

'In diesem Lande genial zu sein
Ist von der Kirche und dem Staat verboten'
(To be a genius in this country
Forbidden is by Church and State.)

In considering misconduct and dishonesty in science, researchers and managers of science in India might also introspect. Are cases of plagiarism, 'simulation', false claims and misappropriation of credit investigated, when allegations are made and evidence produced? Or is there a general tendency to push all disturbing issues under the carpet, pretending that 'simulation' rarely happens? Is there a general reluctance to pursue purposeful enquiry, where *prima facie* cases have been established? Even more disturbingly, do we reward and encourage unethical practices by pretending that they do not occur; on occasion, elevating and celebrating 'simulators'? Do we train students to have sufficient respect for the methods of science and to value integrity above success? In an environment where corruption seems to be endemic, scientific institutions need to be watchful.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The science of engineering

Attending good lectures is one of the most painless, and probably most pleasant, ways of acquiring information in science. At a recent meeting of the Indian Academy of Sciences, I was a fortunate spectator at a marvellous presentation of a metallurgist's view of quasicrystals, the problems of filling space with atoms of differing sizes and the connections between the concerns of materials science and the apparently disparate fields of physics, chemistry, biology, mathematics and metallurgy. The speaker, a colleague of mine, S. Ranganathan, began with Johannes Kepler's musings on the shapes of snowflakes, moved to Linus Pauling's classic determination of the structures of intermetallic compounds, discussed the early ideas of J. D. Bernal and Alan Mackay, described Roger Penrose's, now famous approach to the tiling problem, before presenting his own work, and that of colleagues, on the problems of fivefold symmetry and quasicrystals. On the way, the speaker alluded to Dmitri Mendeleev and his periodic table, but provided a view of materials that seemed to rely on the tenets of metallurgy, with the inevitable invocation of the name of William Hume-Rothery. In the enthusiastic discussion that followed physicists, chemists and biologists seemed to identify themselves with the problems of filling space with atoms; geometry, chemistry and nature seemed to be working in concert. At the tea, to which the audience adjourned, I encountered another colleague, a professed solid state chemist who remarked: 'The metallurgists' approach to the problem of quasicrystals and interatomic interactions is intuitive and empirical.' He seemed to imply that physicists (and presumably the more rigorously minded chemists) would address the same issue with greater theoretical finesse. In thinking about this rather casual, offhand comment, I came to an obvious conclusion; disciplines and their contours have a long evolutionary history, which shapes the thinking of their practitioners. But, more pertinently, the speaker who belongs to a Department of Metallurgy is categorized in our surroundings as an engineer, while the vast majority of the audience consisted of those who would label themselves as 'pure' scientists. Metallurgy, of course, has been traditionally considered as an engineering discipline; metallurgical engineers have, in the past, been much sought-after by heavy industry, particularly those dealing with steel and aluminium. Metallurgy also has a hoary history as an empirical science; the Iron Pillar in Delhi providing a spectacular example of the state of the subject nearly 1600 years ago in India (Balasubramaniam, R., *Curr. Sci.*, 2002, **82**, 1357). But, metallurgy's face has been transformed by the remarkable growth of materials science, a field that lies at the confluence of chemistry, physics and metallurgy. Biology too, threatens to provide insights into the construction of the materials of the future. The obvious applications of new materials lie in every area of engineering and will undoubtedly catalyse the next technological revolution.

Science and engineering, upon careful reflection, appear to be seamlessly connected; but, in reality, in our institutions, they are sharply divided. In the Indian Institutes of Technology (IITs), the flagship engineering institutions in India, the science departments, mathematics, physics and chemistry, are intended to provide basic background in these disciplines for a large undergraduate population, while maintaining research programs, catering to prospective Ph D students. The engineering departments have very large teaching commitments, with generally fewer research students. At the Indian Institute of Science (IISc), the absence of an undergraduate program was presumably intended to enhance the focus of research in both science and engineering. Indeed, when students first entered IISc in 1911, two years after its founding, they were greeted by two departments, representing distinct streams, General and Applied Chemistry and Electro-Technics (renamed two years later as Electrical Technology). But, as the institution grew in size, the perceptions about science and engineering seemed to diverge; while, science departments appeared to have research as a dominant commitment, engineering sections acquired a greater role in teaching, training and industrial consultancy. Curiously, when departments in different areas of engineering were grouped for administrative convenience, the resultant groups were christened as Divisions of Mechanical and Electrical Sciences. The term 'sciences' appeared to be a larger umbrella, capable of accommodating departments in diverse (but at times, perilously close) disciplines of engineering. Undoubtedly, even the most thoughtful of academic administrators must have difficulties in resolving internal conflicts about the terms science and engineering.

Simply, the common (and admittedly, prejudiced) view may be stated thus: 'Academic scientists do research, publish papers and train more scientists; Academic engineers

train other engineers, who do useful work and sometimes do apparently useful work themselves, through their interaction with industry and government'. This stereotypical definition is changing, as more scientists, particularly chemists and biologists, move towards closer interaction with industry. But, one message seems clear; research appears to be an activity of limited import on the academic engineering scene. There are, of course, many celebrated researchers in the engineering departments of our high profile institutions, many of whom are indeed scientists of great distinction; their work often closely overlapping with the interests of colleagues in allied areas of science. There are some subjects which run as a cementing thread across the disciplines; fluid dynamics, for example, can bind researchers of distinctly different persuasions.

In trying to find a clear definition of *engineers* and *engineering*, I turned to Eugene Garfield (*Essays of an Information Scientist*, Vol. 12, pp. 304–309, 1974–1976), who in the early days of scientometric analysis, had indeed struggled to define these terms. He points out that Webster's dictionary yields the following definition of engineering: '. . . the science by which the properties of matter and the sources of energy are made useful to man in structures, machines and products'. Garfield then, ruefully, adds that 'Leonardo DaVinci would have readily accepted this definition as descriptive of his main interests and employment'. He inevitably, moves to identifying engineers and their research interests by examining publications and citations. Using data for 1969, Garfield compiled two lists: A set of 50 journals, which were most often cited by a group of engineering journals and a list of 50 titles, which most often cited his chosen journal group. Looking through these lists even I recognized some perennial favourites of academic engineers; *J. Fluid Mechanics*, *J. Acoustical Soc. America*, *J. Applied Mechanics*, *Chemical Engineering Science*, *AICHE Journal* and as many as 12 titles of the Institute of Electrical and Electronics Engineers (IEEE). There were, of course, several science journals common to these lists; *J. American Chemical Society*, *Physical Review* and *J. Applied Physics* appeared, an indicator that engineering and science are inextricably linked. Over 30 years later academically successful engineers might still like to publish their results in many of the journals on Garfield's lists. But, it does seem that in the academic engineering departments in India, publication of research does not seem to be a critically important activity. To a great extent, this situation is a consequence of the diminishing quantity and deteriorating quality of students, who choose to obtain research degrees in engineering. Ironically, while there is a mad scramble to enter engineering courses after high school, students leave academia with undergraduate degrees, often deserting the discipline in which they have been trained. The prospects of well-paid employment in the areas of management, software and 'information technology' prove irresistably seductive. This state of affairs appears to have led to a widely held perception in India, that research may not be a central activity of an academic engineering department.

The boundaries between science and engineering are sometimes sharply drawn. In reporting on a millennial symposium of the National Academy of Engineering USA, celebrating the greatest engineering achievements of the 20th century, *Physics Today* (May 2000, pp. 48–49) charged the engineers with neglecting to highlight the fact that many of the most spectacular technological advances had their roots firmly grounded in physics. Electrification topped the engineers list; *Physics Today* was quick to remind its readers that the roll call of honour would be led by the physicists Michael Faraday, James Clerk Maxwell, Hans Christian Oersted, Alessandro Volta, Georg Simon Ohm, Joseph Henry and Nikola Tesla. Further down the list was electronics, and here the physicists claimed John Bardeen, Walter Brattain and William Shockley, inventors of the transistor and more recently, Jack Kilby, who developed the integrated chip. In arguing about the distribution of credit for technological advances, both engineers and scientists skate on thin ice. One cannot do without the other. Scientists in all areas, physics, chemistry, biology, geology amongst them, rely on precision engineering to produce the instruments that advance their fields. Engineers play a critical role in transforming basic advances in science into the technologies of daily use. Several years ago, on the threshold of the digital revolution, W. O. Baker considered the issue of 'concurrency on the future of science and engineering' and its portents for the future (*The Bridge*, National Academy of Engineering USA, 1986, **16**, 19). In addressing the future possibility of a 'non-traditional juncture of science and engineering', Baker advanced the cause of reforming science and engineering education. Unfortunately, in our environment, instead of promoting a coming together of the disciplines, our pattern of education has tended to insulate subjects and departments.

Innovative technology, which politicians and policy-makers clamour for, can only be born by a fusion of good science and engineering. The rapidly emerging fields of nanotechnology and biotechnology may be advanced only by a 'broad coalition' of scientists and engineers. For this to happen there must be a greater appreciation of science in our departments of engineering, while students of science must be aware of the enormous gulf that engineers bridge, when they transform a scientific advance into a technological success. This coalition must be spearheaded by interpreters who speak both languages. I suspect that it might be easy to find the 'scientists' amongst engineers, who can indeed forge such a coalition, the need for which is being more widely felt in the best of our institutions. The science of engineering requires greater exposure. The lecture which catalysed this essay is a stellar example.

P. Balaram

EDITORIAL

Editorial discretion and indiscretion

Towards the end of last year a paper, conservatively titled 'Transgenic DNA introgressed into traditional maize landraces in Oaxaca, Mexico' appeared in the literature (Quist, D. and Chapela, I. H., *Nature*, 2001, **414**, 541). A quick glance at the paper reveals illustrations and protocols characteristic of any contemporary paper in molecular biology; photographs of DNA gels, descriptions of polymerase chain reaction (PCR) procedures, nucleotide sequencing and the somewhat less widely used, inverse PCR reaction. To a casual reader, uninterested in transgenic crops only the uncommon words 'introgressed' and 'landraces' may have attracted attention. But the opening sentence is a giveaway: 'Concerns have been raised about the potential effects of transgenic introductions on the genetic diversity of crop landraces and wild relatives in the areas of crop origin and diversification, as this diversity is considered essential for global food security.' Towards the end of this rather terse communication is another carefully worded sentence: 'The intentional release of large amounts of commercial transgenic seed into the environment since the mid-1990s represents a unique opportunity to trace the flow of genetic material over biogeographical regions, as well as a major influence on the future genetics of the global food system.' But masked in the measured and at times, impenetrable language of the scientific paper was a clear message; the Quist–Chapela experiments suggested that 'transgenes' from genetically modified, industrially produced maize appeared to have integrated into the genomes of wild relatives in the neighbourhood. The authors concluded that there is a 'high level of gene flow' and raised questions on the origins of the 'introgressed DNA', since Mexico has had a moratorium on the planting of transgenic maize since 1998. Somewhat disturbingly, this study also suggested that in some samples 'the transgenic DNA construct seemed to have become re-assorted and introduced into different genomic backgrounds, possibly during transformation and recombination'. As *Science* noted in a sensationally titled news item 'Has GM corn "invaded" Mexico', 'a gene's behaviour depends on its place in the genome, the displaced DNA could be creating utterly unpredictable effects' (Mann, C. C., *Science*, 2002, **295**, 1617). The Quist–Chapela paper appeared to be providing clear scientific support for the vocal anti-GM crop groups, around the world. It also seemed to signal a setback for the powerful multinational companies, that would like to cash in on the ongoing revolution in agricultural biotechnology.

The formal counter-attack was not long in coming. In April this year two 'brief communications' appeared unambiguously entitled 'Suspect evidence of transgenic contamination' and 'Maize transgene results in Mexico are artefacts' (Metz, M. and Fütterer, J., *Nature*, 2002, **416**, 601; Kaplinsky, N. *et al.*, *Nature*, 2002, **416**, 601). Their import was clear. The first communication emphatically stated that 'there is no evidence of transgenes fragmenting and scattering through genomes'. The second noted that 'transgenic corn may be being grown illegally in Mexico, but Quist and Chapela's claim that these transgenes have pervaded the entire native maize genome is unfounded'. The authors concluded that 'it is important for information about genetically modified organisms to be reliable and accurate, as important policy decisions are at stake'. These exchanges might have been dismissed as yet another example of the bitter differences between proponents and opponents of genetically modified crops, but for the appearance of an 'editorial note' in *Nature* (2002, **416**, 600), which 'concluded that the evidence available is not sufficient to justify publication of the original paper'. *Nature*'s editors were publicly backtracking, although the Quist–Chapela paper had passed the journal's apparently rigorous peer review, and the authors had obtained further data and stood by their results. In similar situations the journal (and indeed most journals) would have maintained a discreet silence, allowing readers to judge the science, uninfluenced by editorial judgements. In the 27 June 2002 issue of *Nature*, the battle expanded with correspondents questioning the motives of both the journals and the critics of the Quist–Chapela paper. The fact that the authors of the two published critiques had 'part of their research funded by the Torrey Mesa Research Institute (TMRI), an offspring of the agricultural biotechnology company Novartis (now Syngenta)', was seen as a significant indicator of bias. The 'strategic alliance' between TMRI and the University of California, Berkeley, home to both Quist and Chapela and many of their critics, muddies the waters even more. Even more pointedly, the correspondents, many of whom are at Berkeley, note that the *Nature* Publishing Group actively integrates its interests with those of companies invested in agricultural and other biotechnology, such as Novartis, AstraZeneca and other 'sponsorship clients', soliciting them to 'promote their corporate image by aligning their brand with the highly respected *Nature* brand. In this growing controversy even the journal *Nature* seems to have left itself open to a charge that its editorial actions were driven by a 'partisan agenda' to promote commercial interests in agricultural biotechnology. There is an element of irony in this accusation against the journal; indeed *Nature*'s publication of the note 'Transgenic pollen harms monarch larvae' in May 1999, triggered an upsurge of activity by the anti-GM

technology activists, with many observers questioning the journal's motivation in publishing an apparently preliminary report, whose interpretations were debatable.

The debate on genetically modified foods has left its imprint in other editorial offices. The publication by *Lancet* of a paper entitled 'Effects of diets containing genetically modified potatoes expressing *Galanthus nivalis* lectin on rat small intestine' (Ewen, S. W. B. and Pusztai, A., *Lancet*, 1999, **354**, 1353), unleashed a wave of anti-GM food attacks. A blue-ribbon Royal Society panel found the research flawed, but 'five out of six' reviewers for *Lancet* had approved publication of the paper. The *Lancet* editor's motives in promoting the Ewen–Pusztai manuscript were discussed and *Nature*, in a rare moment of introspection, worried about the 'dangers of over-dependence on peer-reviewed publication'. Few would argue with the conclusion: 'The journals should be expected to maintain their standards in publishing valid, if occasionally credibility stretching science. But the ever-increasing reliance on them for quality control has disadvantages that should be countered by adequate provision of time and resources for independent assessment and, in the midst of controversies, publicly funded agencies providing comprehensible, reliable and prompt complementary information over the networks' (*Nature*, 1999, **401**, 727).

Looking back on the 1999 controversy, *Lancet*'s editor, Richard Horton noted that such 'arguments... threaten the fragile remnant of trust that remains between the public and scientists'. Horton is quick to point out the drawbacks of peer review, which may sound familiar to most authors of rejected manuscripts: '... the system of peer review is biased, unjust, unaccountable, incomplete, easily fixed, often insulting, usually ignorant, occasionally foolish and frequently wrong' (Horton, R., *The Medical Journal of Australia*, 2000, **172**, 148). But in reflecting on the GM food battle, Horton highlights the fact that the Royal Society claimed in its report that 'genetically modified food offers one solution to a projected global famine'. He counters by quoting Amartya Sen's conclusion that 'access to food depends far more on a complex mix of economic, social and political factors' and that, as we all know too well, 'a person may starve in the face of plenty'. Horton's conclusion is bound to raise many hackles when he says: 'seeking a technological food fix for world hunger may be not only the biggest scientific controversy of 1999, but also the most commercially malevolent wild goose chase of the new century'. While dependence on peer-reviewed publications may be an uncertain guide to formulating public policy, it is important to recognize that the conflicts of interest that sometimes arise in government and publicly funded agencies can be even more disturbing and dangerous.

The turmoil at editorial offices at the most prestigious scientific journals has not been restricted to the area of biotechnology. In March, *Science* published a paper entitled 'Evidence for nuclear emissions during acoustic cavitation' (Taleyarkhan, R. P. *et al.*, *Science*, 2002, **295**, 1868). Its claim was startling. The authors reported the detection of nuclear fusion in a table top experiment, raising the ghosts of the 'cold fusion' episode. While the jury still seems to be out on the experiment, the debate on whether *Science* should have published a paper, whose results were in doubt, has raged for several weeks. Commenting editorially on the controversy, the editor of *Science*, Donald Kennedy notes that the history of the paper by Taleyarkhan *et al.* 'has exposed some of the more unusual challenges that can arise in the publication processes'. Pressure mounted on the journal not to publish the paper as physicists aware of its findings grew sceptical of the conclusions. Organizations like the American Physical Society seemed to publicly discredit the report even before its publication. At this stage, Kennedy correctly concluded that 'publication is the right option, even – and perhaps especially – when there is some controversy'. The journal's mandate, he notes, 'is to put potentially important science into public view after ensuring its quality as best as we possibly can. After that, efforts at repetition and reinterpretation can take place out in the open' (Kennedy, D., *Science*, 2002, **295**, 1793).

The pressure on high profile journals has increased even as the public expectations of the fruits of science and technology have grown. In many areas like biomedicine the pages of the top journals are scanned carefully to spot the signs of impending breakthroughs, which may affect the lives of millions of people worldwide. There are many red herrings, which have their day in the limelight, as the popular press blows the findings of scientific papers out of all proportion to their true import. There are publicity-seeking authors who can trumpet a scientific paper most effectively for the cause of self-advancement, before apparently sensational findings are washed away by the growing avalanche of scientific literature. For the 'highest impact' journals to stay on top, they must ensure that they snare the best papers. In order not to lose out to the competition, risks must sometimes be taken. The flood of misconduct cases that seem to have hit the best journals in recent times is an indication of the pressures on authors, reviewers and editorial offices. The most recent cases in the physical sciences, notably the problems at Bell Laboratories (Brumfiel, G., *Nature*, 2002, **418**, 120) and the even more recent retraction of the discovery of elements 116 and 118 at the Lawrence Berkeley Laboratory (*Phys. Rev. Lett.*, 2002, **89**, 039901), suggest that science may be approaching a new crisis. In arguing that complacency on misconduct issues may be dangerous, *Nature* notes somewhat ominously that business leaders had often felt 'that markets would correct for dishonest practices, in the fullness of time' (*Nature*, 2002, **418**, 111). The growing range of financial scandals demonstrates the impotence of markets to police themselves. Will this also be eventually true of science?

Journal editors can, if they so desire, wield complete control on the material they publish. But, realistically no editorial office can function without the unflinching and most often, selfless service of peer reviewers. At times, editors must exercise judgement and their decisions will anger either authors or reviewers. In most cases, the benefit of doubt must go to authors, in the hope that publication of results that fail to stand up to subsequent scientific scrutiny, is a lesser evil than the prospect of suppressing an interesting, but controversial finding, that may advance the course of science. In recent times the growing impact of the findings of science on public policy has made the exercise of editorial judgement, at times, a difficult and even professionally hazardous process. Editors have the right of exercising discretion, but inevitably they must be allowed a margin of indiscretion.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science in India: Signs of Stagnation

Ranking individuals, institutions or nations is a dangerous pastime. The parameters which permit differentiation and classification must be carefully defined. In sport, rankings are easily achieved. Competition, with clearly defined objectives, permits an unambiguous conclusion. Even as the results from the Commonwealth Games in Manchester pour in, it is clear that India might finish third or fourth in the medals tally, a result that will undoubtedly gladden the hearts of all Indian sports lovers. But, there are other lists. In the FIFA rankings, the Indian football team is listed at a lowly 124th position in a collection that includes 203 teams, including those from tiny countries and islands, a few of which may be unknown to many readers. In football India is outclassed by Libya, Estonia, Cyprus and Iceland among others. Brazil, the newly crowned champion, tops the list and the Republic of Korea which did so well is, as high as, 22nd on the list. In this season of lists, the United Nations Development Program (UNDP) has released a ranking of countries based on the human development index (HDI). The HDI is calculated using a complex formula, which factors in parameters like life expectancy at birth, adult literacy rate, GDP per capita and an education index. And, in the HDI ranking for 2001, India appears unhappily, at the 115th position among 162 countries. Predictably, there have already been critical comments in the press on the manner in which the HDI is calculated. There is, however, some cheering news. Bangalore, home to this journal, ties for the 4th position with four other cities, among the 46 top technology hubs; Silicon Valley in the US, of course, heads the list. On an index for technology achievement, India occupies the 63rd position, with China at 45, Brazil at 43 and Korea at 5.

In thinking about ranking and lists, I must turn inevitably, to science. In assessing scientific productivity quantitatively, the simplest index is the total count of published papers. The database that is most widely used is the *Science Citation Index* (*SCI*), produced by the Institute for Scientific Information, Philadelphia. Although the *SCI* is necessarily selective in covering Third World journals, it still includes all scientific periodicals above an acceptable threshold of quality and frequency. Counting papers and ranking nations is, thus, a common activity amongst scientometrists. Nearly six years ago in a letter entitled 'India's declining ranking', N. Raghuram and Y. Madhavi pointed out that the country slid from 8th position in 1980 to the 13th position in 1995, in a list that ordered countries based on their output of published papers (*Nature*, 1996, **383**, 572). In commenting on the apparent decline of India's world ranking in science, the authors noted that 'lack of motivation, a feudal work culture and absence of dynamic and inspiring leadership' may be contributory factors. They also pertinently noted that 'what is most striking is the total lack of monitoring of Indian science publishing trends as an input to policy and planning'. After a brief outcry at the 'negativism' implicit in the Raghuram–Madhavi analysis, the matter was conveniently forgotten. More recently the ghosts have come back to haunt us. In a letter entitled 'Is science in India on the decline?', published in this journal, S. Arunachalam has returned to the arena of scientific ranking, armed with data for two decades, 1980–2000 (*Curr. Sci.*, 2002, **83**, 107). India is now at position 15, while China and South Korea occupy the 9th and 16th ranks respectively. But, the statistics are telling. In 1980, India accounted for 14,983 papers in the *SCI*. By 2000, the number had fallen to 12,127. China moved upwards from 924 in 1980 to 22,061 in 2000. In the same period South Korea jumped from 175 to 12,013, Brazil from 2215 to 9565 and Israel from 5733 to 9292. Although the share of the developing countries in the world's scientific output is still very poor, there are clear signs that some countries are moving forward at a fast pace. Korea and Brazil appear to have focused on improving both their soccer and their science. India, on the other hand, appears to have reached an uncomfortable, equilibrium position. If the published output remains stagnant or declines over the next few years, there is little doubt that we will slip further down these rankings.

Is the number of papers published annually an adequate index of the health of the country's scientific enterprise? Many would argue that few conclusions can be drawn from such numbers, which probably hide more than they reveal. But how do we assess an individual scientist, a

department or an institution? More often than not, such evaluations are based on the number of published papers as an index of performance. When the numbers are small more detailed analyses, which introduce a 'quality criterion', are possible. It is here that the journal impact factor begins to rear its head. But, at the level of nations a total count may be a moderately reliable index. Why then is India struggling to improve its rate of scoring, while others seem to be accelerating rapidly? The very premise that science in India is in a rut will be strongly contested. Are there not many signs of visible, technological progress? The successful nuclear explosions, the satellite launches and the production of indigenous missiles are a tribute to the organized arms of strategic research and development. Well-defined 'missions' can undoubtedly be accomplished in chosen areas of technology development. But, when published papers are counted we are really assessing academic science; traditionally research carried out in universities and national laboratories, which are not part of a grand strategic design.

For over three decades after independence academic science remained a low-key activity, attracting little by way of major support from government. The winds of change were evident in the late 1970s, when the National Committee for Science and Technology began to function. The early 1980s saw an intensification of support for academic science by the Department of Science and Technology (DST), following the Baroda (now Vadodara) seminars, which formulated the 'Thrust Area' programs. Subsequently, government funding for science increased steadily, levelling a little in the late 1990s. Ironically, the decline of scientific productivity coincides with enhanced inputs into scientific research. This apparently paradoxical situation merits analysis. One possibility is that the universities, which used to contribute substantially to published output, have been declining alarmingly. While many new, and sometimes embarrassingly well-endowed, national institutions have been created since the mid-1970s, the academic science departments in most universities have been rapidly plunging downhill. The major academic centres of the 1950s and 1960s, the universities at Calcutta (now Kolkata), Madras (now Chennai), Delhi, Allahabad and Banaras are no longer the foci of academic science. Plagued by problems of political interference, diminishing resources, declining faculty quality and a rapid deterioration of the academic ambience, the contribution of Indian universities to the country's scientific output is undoubtedly falling. With the exception of a couple of Central universities, the status of science in university departments is hardly encouraging. There are no solutions on the horizon, which will address the inexorable decline of our universities. On the contrary, several national laboratories and central institutions now have excellent research facilities, increasing numbers of Ph D students and appear to have significantly enhanced the quantity and quality of their published scientific output. However, the national institutions have been buffetted by the winds of change. Their research is gradually being driven, by the carrot of large funding, into areas of apparently applicable research, with the focus shifting to areas of 'national priority', 'networked projects' and 'missions' of various kinds. While considerable financial inputs are generally provided for these enterprises, publications are often not the expected outcome of these projects. In many cases the output of these researches is hard to measure. The camouflage of patents and protection of 'intellectual property rights' is often used to mask the fact that many major 'research projects' have yielded far fewer interesting results than promised. A new breed of scientist is now emerging; the 'managerial scientist', whose success is determined by the ability to accumulate funds and facilities, with a diminishing emphasis on published output.

While mere numbers of papers may not tell the whole story, it is worth recalling that one of the first analyses of Third World science output, using the 1973 *SCI*, put India at the 8th position, preceded only by the US, UK, USSR, West Germany, France, Japan and Canada (E. Garfield, *Science and Public Policy*, 1983, **10**, 112). Interestingly, in the period for which Arunachalam has produced statistics (1980–2000), the number of universities and deemed universities has grown from 128 to 231. R&D expenditure has risen in this period from Rs 760.5 crores to about Rs 13,000 crores; figures adjusted for the falling value of the rupee. From 1985–86 to 1998–99, the R&D expenditure has hovered around 0.8% of the Gross National Product (*Research and Development Statistics*, Department of Science and Technology, May 2002). It does not require a great feat of interpretation to conclude that all indicators suggest that scientific output is on the decline. An uncomfortable facet of counting numbers of papers is that quantity is no guarantor of quality. A few years ago an analysis of the *impact* of papers published between 1992 and 1996 was carried out, using the ISI's databases. Nations were ranked by citations per paper. Switzerland topped the list, with the US coming second. Brazil appeared at position 30, Argentina at 26, while China and India were conspicuously absent (*Science Watch*, 1997, **8**, 1). Here, we must remember that a large number of poorly cited papers reduce the citation frequency. Scientometric analysis, carefully done and conservatively interpreted, can be valuable in establishing the status of academic science in institutions and countries. A hard look at incontrovertible facts may help policy makers. The tools for assessment exist and they must be cautiously used. We must also pay attention to the problem of evaluating scientific projects which do not seem to yield publications. However much one would like to disbelieve statistics, the writing is on the wall. Science in India is in danger of stagnating.

P. Balaram

EDITORIAL

Environmental politics: The Asian brown cloud

'Until recently, North America and Europe dominated the use of fossil fuels, resulting in strong carbon dioxide emissions and global warming. The fossil energy-related CO_2 release per capita in Asia is nearly an order of magnitude smaller than in North America and Europe. However, Asia is catching up.' These are the somewhat ominously worded opening sentences of a paper by an international cast of authors (*Science*, 2001, **291**, 1031), summarizing the results of the Indian Ocean Experiment (INDOEX) which highlights 'widespread air pollution from South and Southeast Asia. Using data gathered 'during the dry monsoon season in January–March 1999' the authors conclude that 'the pollution build-up will be strongest in the winter monsoon under large-scale subsidence and cloud-free conditions'. While the references to the 'dry' and 'winter' monsoons are calculated to bewilder ordinary readers, the concluding sentence is unambiguous, clear and forbidding: 'Unless international control measures are taken, air pollution in the Northern Hemisphere will continue to grow into a global plume across the developed and the developing world'. The authors note that 'the growing pollution in this region gives rise to extensive air quality degradation with local, regional and global implications, including a reduction of the oxidizing power of the atmosphere'. The appearance of a well-documented article, in a highly visible journal, authored by an impressive group of authors from over half a dozen countries including three from India appears, at first glance, to be a major contribution to the assessment of environmental pollution in the Indian Ocean region. But the interpretation of the results of the INDOEX campaign acquires a new dimension following a poorly worded news release of the United Nations Environment Programme (UNEP), which begins melodramatically: 'A vast blanket of pollution across South Asia is damaging agriculture, modifying rainfall patterns including those of the mighty Monsoon and putting hundreds of thousands of people at risk a new study suggests'. The press release quotes the Executive Director of UNEP as saying that 'the haze is the result of forest fires, the burning of agricultural wastes, dramatic increases in the burning of fossil fuels in vehicles, industries and power stations and emissions from millions of inefficient cookers, burning wood, cow dung and other biofuels'. To add an alarmist note, presumably directed at a Western audience, the UNEP official is quoted as saying: 'There are also global implications not least because a pollution parcel like this, which stretches three kilometres high, can travel half way round the globe in a week'. Coming on the eve of the World Summit on Sustainable Development due to begin in Johannesburg on 26 August, the UNEP's public interpretation of the INDOEX studies may be dictated as much by political compulsions as scientific facts. The science of the UNEP report, 'The South Asian Brown Cloud', still needs to be assessed; but the international agency's popular interpretations may be completely ill founded. A general reader of the press release cannot but help come away with the feeling, that unless checked, the burgeoning human populations of South and Southeast Asia constitute the most important current threat to the global environment.

Understanding the factors that damage our environment is a legitimate concern of science. The development of technologies that mitigate, minimize and even abolish the effects of environmental pollution are undoubtedly necessary and desirable. But efforts to control and limit environmental degradation acquire complex dimensions because of the imperatives of local and international politics. The issue of 'global warming' provides an excellent example of the science and politics of the environment. Discussions of the 'greenhouse effect' began over a century ago, when the Swedish chemist, Svante Arrhenius, better known for his theory of electrolytes, worried about the 'influence of carbonic acid in the air upon the temperature of the ground' (Arrhenius, S., *Philosophical Magazine*, 1896, **251**, 8; Rodhe, H., Charlson, R. and Crawford, E., *Ambio*, 1997, **26**, 2). Crudely put, as the concentration of infrared absorbing gases (Arrhenius considered only carbon dioxide and water vapour) increases the atmosphere becomes warmer.

In the years since Arrhenius, the number of greenhouse gases has increased; methane, nitrous oxide and ozone among them. Growing populations and the dra-

matic changes in human activity, fuelled by successive technological revolutions, have increased the 'carbon burden' of the atmosphere. The most imaginative doomsday scenarios predict a melting of the polar ice-caps and a rise of the ocean levels, in an apocalyptic future. At the other end of the spectrum are the 'conservatives', who dismiss all environmental concerns as vastly exaggerated. The global warming discussions are further muddled by suggestions that a focus on carbon dioxide emissions may be misplaced, with non-CO_2 greenhouse gases and aerosols being ascribed a more critical role (Hansen, J. *et al.*, *Proc. Natl. Acad. Sci. USA*, 2000, **97**, 9875). But, regardless of the precise causes and mechanisms, there is a general perception that CO_2 emissions must be cut worldwide. Carbon is burned in many facets of human activity and some off-the-cuff estimates suggest 'that the average American is responsible for about 25 times more CO_2 than the average Indian' (Gelbspan, R., *Sierra Magazine*, May/June 2001). Clearly, the major contributions to the global atmospheric carbon burden must come from the developed world. But 'progress' will ensure that poorer countries enhance their contributions to the CO_2 load. Reductions in emissions must then be accompanied by some level of 'national austerity' and self control; a path which appears completely unacceptable to the United States. Indeed, the Kyoto Treaty, so emphatically repudiated by the US, was remarkable for the unanimity of opinion it engendered amongst American politicians. The mandatory ceiling on carbon emissions by developed countries, suggested by the Kyoto protocol, was clearly perceived as a major threat to the US economy and way of life. No ceilings were imposed for developing countries under the Kyoto agreements, an acknowledgement that the path to a higher standard of living, applying existing technologies, must necessarily demand a higher carbon emission threshold. The global politics of the environment clearly involve complex issues of national self-interest and global equity. Even within nations, and India is a prime example, uneven development leads to stark disparities. Preaching austerity and self-restraint to the economically powerful is hardly likely to be a rewarding exercise.

The science and politics of environmental pollution can be complex and puzzling to casual observers. The Swedish Academy, almost exactly a century after Arrhenius, recognized the field of atmospheric chemistry, particularly 'the formation and decomposition of ozone', by awarding the 1995 Nobel Prize in chemistry to Paul Crutzen (coincidentally one of the discoverers of the Asian Brown Cloud), Mario Molina and Sherwood Rowland. The Nobel committee hailed the recognition that the ozone layer, 'the Achilles heel of the biosphere' is sensitive 'to the influence of anthropogenic emissions of certain compounds'. Chlorofluorocarbons (CFCs), better known as 'freons', primarily used in refrigerants, were identified as major culprits. The scientific work was published in the early 1970s but international agreements to restrict, limit and eventually ban CFCs came into being with the Montreal protocol of 1987. Here again, the total ban on the 'most dangerous' gases requires the use of CFC substitutes; with the developed world at a clear advantage in being able to switch to substitutes at little economic cost.

From Montreal to Rio de Janeiro, to Kyoto, to Bonn to Johannesburg, the conferences on environmental politics circle the globe at a much faster rate and with a greater longevity than the Asian Brown Cloud. Viewed in the context of the impending Johannesburg summit, one cannot escape the feeling that the UNEP's sudden concern for the Asian Brown Cloud and its global implications may be motivated more by politics than science. It is also a matter of some concern that the scientific study upon which the conclusions are based, INDOEX, is the product of an international collaboration involving a sizable participation of scientists from India (*Current Science*, Supplement, 10 April 2001). Several conclusions drawn in the UNEP press release may not stand scientific scrutiny. The transportation of aerosols across the globe, the existence of the 'brown cloud' as a permanent feature and the cause–effect relationship between the haze and an apparently failed monsoon, may not be supported by hard evidence. While the motivation for the timing of the UNEP press release is obscure, it is evident that politics takes precedence over science in discussions of the environment. There is little doubt that every effort is needed across the country in cleaning up our environment. But, the agenda for action must be based on careful assessments made within the country. Like in many other areas, where science and technology impinge on international politics and business, the advice of international agencies must be carefully weighed and viewed through the, sometimes distorting, prism of national self-interest.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Philanthropy and the funding of science

'And gradually there is coming into being a new branch of science – molecular biology – which is beginning to uncover many secrets concerning the ultimate units of the living cell. . . .

Among the studies to which the Foundation is giving support is a series in a relatively new field, which may be called molecular biology, in which delicate modern techniques are being used to investigate ever more minute details of certain life processes.'

Warren Weaver
The Natural Sciences Report
Rockefeller Foundation
1938, 203–204

Molecular biology is central to all modern research in biology. The discipline was born in a report of the Rockefeller Foundation, when the director of its program in natural sciences, Warren Weaver, coined the term 'molecular biology' in arguing for directing funding towards a new, reductionist approach to biology. Weaver's intentions were to promote a new 'intellectual ferment' in biology, turning to physics and chemistry, for attacking the central issues of cellular processes. Weaver was spectacularly successful, overseeing grants of over $ 90 million in the period between 1932 and 1959. The Rockefeller Foundation supported a stellar cast, Hermann Muller, Max Delbrück, T. Caspersson, E. Hammarsten, Linus Pauling, Robert Corey, T. Svedberg and W. T. Astbury, who would transform biochemistry and genetics in the years between the World Wars; setting the stage for the Watson–Crick double helix to emerge in 1953. The birth of modern biology was truly midwifed by the Rockefeller Foundation. Both Lawrence Bragg and Max Delbrück, two of the field's most influential founding fathers, have acknowledged their indebtedness to the Foundation, while Linus Pauling's seminal work on the magnetic properties of hemoglobin was catalysed by the carrot of support offered by Weaver (*The Path to the Double Helix*, Robert Olby, Dover, New York, 1994). The Rockefeller Foundation's spectacular success in supporting biological science came at a time when government funding for research in the West was modest. Indeed at the beginning of the 20th century the Rockefeller and Carnegie foundations, started by two of America's richest businessmen, John D. Rockefeller and Andrew Carnegie, had begun a new experiment. 'Foundations with large established endowments were to supply venture capital for the common good. And giving should be strategic – philanthropy should not simply respond to needs but should look for investment opportunities that yield significant returns' (Fitzpatrick, S. M. and Bruer, J. T., *Science*, 1997, **277**, 621). Can there have been a more successful 'venture' in directing basic research than the effort of Warren Weaver at the Rockefeller Foundation?

In the 50 years since the double helix and, of course, the Sputnik, public funding for science has grown enormously in the West, particularly the United States. The breathtaking advance of contemporary science and technology, on an extremely broad front, can be traced to the substantial public investment in science. At the peak of the boom in public interest in science in the US even private companies invested significantly in apparently 'basic research', although in more recent times long-term investments in science have become much less attractive to industry. In analysing trends in the US, N. Myhrvold, in an essay in *Science* (1998, **282**, 621) notes: 'At the government level science has not been recognized as the wellspring of the technology miracle, and as a consequence support is cut, or worse, is subjected to a protracted dissection and review to see if it is "relevant" to short-term economic goals. This puts government funding bodies in the awkward position of second-guessing both the research and the market place.' Ironically, in discussing the drop in defense funds, which most notably supported the long-term development of ARPANET, the precursor of the Internet, Myhrvold ruefully adds: 'Peace has not been good for science'. But, even as discussions on the directions for public funding of science have continued in the West, private philanthropy has been on the rise. About three years ago, *Science* carried a news focus entitled 'Philanthropy's Rising Tide Lifts Science' (*Science*, 1999, **286**, 214). This report placed the annual funding by private foundations worldwide at about

$ 2 billion on science, out of a total spending of $ 20 billion. Over the period 1990–1998 spending by private foundations doubled. While the funding by the US National Institutes of Health (NIH), the National Science Foundation and Department of Energy is almost 10 times larger for science, the philanthropic bodies appear to be gaining importance in their abilities to direct and promote research in specific areas.

For a little over a decade, the Howard Hughes Medical Institute (HHMI) has been a major force in funding biomedical research in the US. Awash with funds generated by the sale of stocks in the Hughes Aircraft Company, HHMI may be the most lasting legacy of the famously reclusive and eccentric aviator, Howard Hughes. In 1999, the assets were estimated at over $ 12 billion, with over $ 400 million distributed as direct science funding to researchers carefully chosen for their excellence in diverse fields of modern biology and medicine. In the UK the Wellcome Trust, established by a pharmaceutical company founder, Henry Wellcome, has become a major force in science funding as the charity's assets have touched nearly $ 20 billion. Focused largely on biomedical research, Wellcome has been a driving force in propelling genomics research, by its investments in the Sanger Centre. The most recent, high profile entrant into the area of private, philanthropic support for science is the Bill and Melinda Gates Foundation, whose 1999 assets are estimated to rival those of its better-known predecessors. The list of foundations and charities which promote science, some in specialized niche areas, in the US is impressive. The Rockefeller Foundation, which played such a key historical role in charting the course for private philanthropy, focuses only modestly on direct research support in areas like reproductive health, agriculture and vaccines. In addition to direct funding of research, Foundations play an important role in providing fellowships, travel support and international exchange and in 'linking policy leaders with the scientific community to consider the factual and analytical basis for decision making in science policy' (Hamburg, D. A., *Science*, 1999, **284**, 259). Private donors have also contributed enormously to the growth of American universities, many of which have launched major new programs using resources generated by campaigns to add privately to their coffers. The long-term partnership between public organizations and private philanthropy has served to establish academic science in the West on a firm footing.

Private enterprise and individual vision catalysed the founding of two of India's most prestigious institutions, the Indian Institute of Science (IISc) in 1909 and the Tata Institute of Fundamental Research (TIFR) in 1945. In the case of IISc, the catalysing vision was that of Jamsetji Tata, while the practicalities of formation required not only the initiatives of his successors but also involvement by the governments of India and the State of Mysore. For TIFR, the initial seeds were sown by the Sir Dorabji Tata Trust and the Government of the Bombay province. Both the institutions today are funded almost entirely by government sources, with little by way of private, philanthropic support for science. The almost complete dependence of our universities and research institutions on government support has resulted in a mushrooming of a number of public agencies and departments which dispense research support. The bureaucracy has now acquired a stranglehold on the functioning of departments charged with the responsibility of nurturing science education and research. Mechanisms for funding individual investigators have become ponderous and inefficient. Government agencies are often at odds with one another in futile games of bureaucratic one-upmanship, which leave scientists bewildered and confused. Even simple issues like fixing fellowships for research students and post-doctoral associates (*Curr. Sci.*, 2002, **83**, 358) have become contentious matters, as concerned Ministries and agencies work at cross purposes. In this environment, foreign funding agencies like the Wellcome Trust have begun to support a few chosen investigators. These organizations will identify people and projects based on their own perceptions and needs, sometimes tying researchers to areas and problems in long-term commitments, which may conflict with local imperatives. Private funding offers considerable operational flexibility, making this kind of support, eminently desirable. At present, private funding for research and development in India is limited to direct 'consultancy' interactions between individual scientist and private industry. There is little by way of broad based, visionary private support. Earlier this summer, one of India's most successful entrepreneurs, K. Anji Reddy addressed an evening gathering at the mid-year meeting of the Indian Academy of Sciences. His title was provocative: 'Science for profit is profit for science'. His thesis was clear; research and innovation could pave the way for great financial success. When he concluded, a questioner asked whether he would support basic research from a share of the profits generated, which were, presumably, the fruits of science. The answer was quick in coming: 'If it tickles my fancy'. Perhaps the time may be ripe to promote new initiatives for supporting science and the institutions where it is practised. In a country, which is slowly emerging into the brave new world of a liberalized economy, the creation of a foundation based on a partnership between government and private bodies may provide a means of nurturing science, unencumbered by the constraints that seem to be shackling government agencies. But, we might reflect that the Rockefeller Foundation's spectacular early successes relied not only on the munificence of its donor but also on the commitment to the public good of men like Warren Weaver.

P. Balaram

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EDITORIAL

The Council of Scientific and Industrial Research

'We can see today the dim lights of a new dawn in the distant horizon of Indian progress. These faint radiations are not the vanishing streaks of our glorious past; they are the sure signs of a new birth full of promise and glory for the future. This dawn represents the birth of the industrial movement in India.'

— Shanti Swarup Bhatnagar
Lecture delivered at Bombay in 1938; *S. S. Bhatnagar on Science, Technology and Development 1938–1954*, ed. Krishna, V. V., Wiley Eastern, New Delhi, 1993.

1942 was a turbulent year. The Second World War had reached its bloody midpoint. The turning points of the war, the battles at El Alamein and Stalingrad would happen only as the year drew to a close. In the east, the Japanese armies had overrun Malaya, Singapore and Burma. The eastern seaboard of India appeared vulnerable. On 8 August 1942 the Congress adopted the Quit India resolution; the sun had begun to set on the British Empire. In these difficult and trying times, plans for the reconstruction and development of independent India had already been drawn, by men of vision and conviction. In 1939, even as the exigencies of war led to the demise of the Industrial Intelligence and Research Bureau, Ramaswamy Mudaliar was lobbying for the creation of a Board for Scientific and Industrial Research (BSIR), which came into being in April 1940. Shanti Swarup Bhatnagar was its Director and its charter was to advise the government and to support research on a broad front. In September 1942, the Council of Scientific and Industrial Research (CSIR) came into being; an autonomous body, constituted as a Society, which now acquired a new mandate to facilitate 'the establishment, maintenance and management of laboratories, workshops, institutes and organizations to further scientific and industrial research and to exploit any discovery or invention likely to be of use to Indian industries'. Mudaliar and Bhatnagar were the men who persuaded the government of the times to establish the CSIR. Today, the CSIR is 60 years old, celebrating its 'diamond jubilee' year, in an environment which is undoubtedly much less turbulent than the year of its birth.

Bhatnagar, the founder of CSIR was by all accounts, a remarkable and indefatigable organizer and builder of institutions. In the brief period between 1947 and 1954, as many as a dozen national laboratories were started; among them the National Physical Laboratory, Delhi; the National Chemical Laboratory, Pune; the National Metallurgical Laboratory, Jamshedpur, the Central Drug Research Institute, Lucknow, the Central Leather Research Institute, Chennai (then Madras) and the Central Food Technological Research Institute, Mysore. Bhatnagar's institutions were spread across the length and breadth of newly independent India. They covered in their disciplinary spread almost every area of science and technology, having an immediate bearing on national development. Supported completely by Jawaharlal Nehru, Bhatnagar's pace of institution creation appears breathtaking, in retrospect. His goals were clear; the tools of science must be brought to bear on the problems of development in a post-war, post-independence period. The struggle of the early phase of the building of CSIR is best illustrated in Bhatnagar's address as President of the National Institute of Sciences (now the Indian National Science Academy) at Patna in 1948: 'With the great change in the outlook of political India with the advent of Independence, events in the world of science are bound to march forward. It is no doubt true that the political developments and rioting which followed the partition of India brought to a standstill the programme of development which was in hand, particularly in the northern regions. Our construction programmes of the National Physical Laboratory, National Chemical Laboratory and the Central Glass and Ceramic Research Institute were completely stopped. With great difficulty we are restarting, though slowly. Nevertheless progress has been spontaneous in many directions.' Bhatnagar did not live to see the full flowering of the institutions that he had conceived. He died in 1954, on the eve of a new year, at the age of 60.

Bhatnagar was not without his critics. The idea of building a chain of national laboratories, separated from the academic science of the universities, attracted strong criticism from some of the most prominent and accomplished scientists of the times. Meghnad Saha advanced the view that the national laboratories would drain the universities and compete for scarce resources: '... the National Laboratories which you have erected will not satisfy our needs.... You must gird up your loins and find out money so that we render sufficient assistance to the

universities and revitalize their activities' (op. cit., Krishna, V. V., p. 17). C. V. Raman was characteristically more acerbic: 'Shah Jahan built the Taj Mahal to bury one of his favourite women. The National Laboratories were built to bury scientific instruments' (quoted in *Journey Into Light*, G. Venkataraman, Indian Academy of Sciences, 1988, p. 517). Even Homi Bhabha, a confirmed builder of institutions was critical of the CSIR: 'All these laboratories were brought into existence in the same way. A Planning Officer was appointed for planning the work and building of each laboratory. The plan was usually drawn up on the basis of the work of similar laboratories abroad, divided into divisions and sections, and an estimate of the staff required made on this basis. An attempt to fill the posts was then made on the basis of advertisement...' (*Journey into Light*, p. 463).

Sixty years after its founding the CSIR consists of a network of 40 laboratories spread across the country, from Jammu in the north to Thiruvananthapuram in the south and from Bhavnagar in the west to Jorhat in the east. This network of largely, scientifically autonomous laboratories, is welded together by a centralized headquarters in Delhi. In keeping with the times, a visit to the CSIR website will reveal that the office in Delhi is called the 'Corporate Headquarters'; a clear reflection of the changing perceptions of the organization in a rapidly changing world. With a total strength of about 22,000 people, the CSIR must undoubtedly have surpassed the size that Bhatnagar envisioned for his organization. During the last few decades of its development, CSIR has been the public face of organized, government-supported science. Other organizations like the Department of Atomic Energy or the Defence Research and Development Organization have been sheltered from public scrutiny by the cloak of strategic concerns. The Indian Space Research Organization has had a sharply defined focus and objectives that readily receive public approbation. In contrast, CSIR with its chain of widely dispersed laboratories, separated both by geography and disciplinary interests, comes across as an organization which is vastly overstretched in its objectives. CSIR's laboratories must interface with industries as diverse as pharmaceuticals, leather and food on the one hand and aerospace, electronics and heavy engineering on the other. The wide-ranging involvement of CSIR with all aspects of science and technology development in India, has often resulted in CSIR bearing the brunt of public criticism when the fruits of technology do not materialize. In reflecting on the ups and downs of this mammoth institution, it is worth recalling the events of the late 1970s, when a move to transfer some laboratories to 'user ministries' gathered strength. These storms have been weathered and CSIR appears to have undergone a transformation in recent times, corporatising its image and emphasizing the importance of laboratories earning a significant portion of their budgets. But, even as the ship has steadied there are many disturbing signs. Recruitment of scientists has been slow, promotions have been effected with little discrimination, resulting in laboratories which are top heavy with senior scientists, who are often uncomfortable with the practicalities of everyday science. The laboratories have also differed widely in their output, with the excellent performance of some institutions compensated by the indifferent results from others.

In its early years, CSIR appeared to be confirming the worst fears of its detractors; pulling scientific talent away from academic institutions. But, over the last four decades or so, CSIR has been a major supporter of academic science. The organization, through its extra-mural arms has consistently supported individual-investigator driven projects across the country, provided national level fellowships for research students and postdoctoral associates and provided the platform for publication of some of the best scientific journals in this country. More recently, CSIR has attempted to promote the forging of inter-institutional collaborations focussed on finding solutions to key issues of local importance, through the New Millennium Technology Initiative (NMITLI). It is these wide-ranging programmes that have permitted CSIR to build extensive links with academia. For many scientists working in CSIR however, the national laboratories appear to have periodically changed their attitudes towards in-house basic or fundamental research. There is no denying the fact that the laboratories of CSIR were created to address the many practical issues of national development. Bhatnagar was unequivocal: 'Generally speaking universities are concerned mainly with fundamental research, while the activities of national laboratories lie essentially in the domain of applied research, though these laboratories are not precluded from taking up investigations of a fundamental character' (op. cit., Krishna, V. V., p. 17). In the absence of well-orchestrated applied projects with defined targets, large numbers of scientists in national laboratories have drifted into areas of research, which are neither productive in basic science nor hold any potential for application. This situation has arisen largely because of a peculiarly hierarchical ambience that prevails in many national laboratories, which encourages a top-down approach to science.

The CSIR has been the cornerstone for the development of science and technology in independent India. In moving towards its platinum jubilee and centenary, the CSIR may eventually have to consider extensive restructuring, shedding much of the flab that it has accumulated over decades. This is a feature common to old, established institutions. When that happens, a new phase in the scientific and technological development of India would have begun. Shanti Swarup Bhatnagar would be proud of the status that his creation has achieved over six decades. In thinking of the future we might well recall the Christian dictum, which may be as applicable in judging institutions as individuals: 'Not what thou art, nor what thou hast been, ... , but what thou wouldst be'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Malaria: Marking Milestones

It is our hope and expectation that researchers around the globe will use the information and biological insights provided by complete genome sequences to accelerate the search for solutions to diseases affecting the most vulnerable of the world's population.

M. J. Gardner *et al., Nature*, 2002, **419**, 498

For his work on malaria by which he has shown how it enters the organism and thereby has laid the foundation for successful research on the disease and the methods of combating it.

Citation, 1902, Nobel Prize for Physiology or Medicine awarded to Ronald Ross

A 105 years have passed since Ronald Ross published his famous paper 'On some peculiar pigmented cells found in two mosquitoes fed on malarial blood' (*Br. Med. J.*, 1897, **2**, 1786). Ross made his epoch-making observations in Secunderabad on 20 August 1897. The human malarial parasites *Plasmodium vivax* and *Plasmodium malariae* had been identified in Rome by Camillo Golgi in 1886. In 1890 Italian physicians described *P. falciparum*, the most lethal of the parasites causing human malaria. Ross conclusively established that the mode of transmission of the disease required the intermediacy of the *Anopheles* mosquito. He decisively identified the presence of the parasite in the guts of mosquitoes fed on malarial blood of a patient, Husein Khan, who was paid one anna for each bite. As late as 14 August 1897, Ross had reported little success, confessing in a letter to his wife: '. . . perhaps (I) am not using the proper kind of mosquito'. In an historical analysis, K. Rajakumar and M. Weisse note: 'He hired three native men to help find mosquitoes, adults and larvae. As if the Lord were answering his plea, an 'angel of fate' put into the hands of one of his 'mosquito men' some larvae that hatched out a different dappled-winged mosquito, with which he began experimenting, hoping for success (*Southern Medical Journal*, 1999, **92**, 567). The 'hatched-dappled' mosquitoes, *Anopheles*, were the vectors which harboured, nurtured and transmitted the malarial parasite. Ross moved to Calcutta in 1898 and established the cycle of avian malaria. The first major step in understanding a disease that has been with humans for thousands of years had been completed. Ross' discovery, which has been repeatedly chronicled, provides a fascinating example of the early successes of tropical disease research, in a historical setting, where the imperatives of empire building and colonial expansion provided a compelling rationale for the studies on malaria. In introducing Ronald Ross at the Nobel ceremonies in December 1902, the Rector of the Royal Caroline Institute, K. A. H. Morner noted that, of the 178,000 men of the British Army in India, 76,000 suffered from malaria in 1897, the year of Ross' success. The estimated mortality in the civilian population was, of course, much higher. There was the inevitable element of serendipity in Ross' encounter with the *Anopheles* mosquito; his 'native' assistants might have been acute observers, a thought that has been played out as fantasy in Amitav Ghosh's novel, *The Calcutta Chromosome* (Ravi Dayal Publisher, Delhi, 1996).

Exactly a century after the award of the Nobel Prize for Ross' work establishing the integral connectivity between the mosquito, the parasite and the human, come the announcements of the completion of the sequencing of the genomes of the parasite, *P. falciparum*, and the mosquito, *Anopheles gambiae*. The parasite genome and related studies appear in the October 3 issue of *Nature*, while the work on the mosquito is described in the October 4 issue of *Science*. As in the case of the report on the human genome, the conquest of the parasite and vector genomes has been a media event, with both journals orchestrating a publicity blitz, which once again focuses public attention on malaria. Ross would be surprised by the fact that over a century after his work the disease still flourishes. As the authors of the *P. falciparum* genome sequence paper emphasize: 'Approximately 40% of the world's population lives in areas where malaria is transmitted. There are an estimated 300–500 million cases and up to 2.7 million deaths from malaria each year. The mortality levels are greatest in sub-Saharan Africa, where children under 5 years of age account for 90% of all deaths due to malaria' (M. Gardner *et al.*, *Nature*, 2002, **419**, 498). In the long battle against malaria the first signs of a possible treatment appeared in the 17th century, when Jesuit priests brought back to Europe the knowledge that the bark of the cinchona tree appeared to alleviate the symptoms of the disease. The alkaloid quinine, was the first widely used antimalarial. Chloroquine,

in many respects a wonder drug, appeared much later, but in 20 years after its introduction, resistant strains of the parasite began to emerge. Intriguingly, chloroquine's successors sulfadoxine–pyrimethamine were universally effective in the field for a shorter period; the parasite acquired resistance in less than 5 years. Coincidentally, the issue of *Science* that reports the genome sequence of the *Anopheles* mosquito also carries the results of an important study that probes the dynamics of drug resistance in the parasite (A. B. S. Sidhu *et al.*, *Science*, 2002, **298**, 210). In a *Perspectives* essay entitled 'A Requiem for Chloroquine' I. M. Hastings *et al.* note: 'Now it appears that the application of modern genetic technology may enable chloroquine to leave one more valuable legacy – a detailed genetic, clinical and epidemiological epitaph that can be used to inform the deployment of its successors' (*Science*, 2002, **298**, 75). There are, in fact, few new drugs on the horizon, which will effectively target all strains of the parasite, including those that have learned to withstand the onslaught of chloroquine and its successors. Ross, however, firmly held the view that the key to malaria control and eradication lay in the ability to destroy the vector, the *Anopheles* mosquito. For a while it appeared that the insecticide DDT would win the war against the mosquito. But, DDT-resistant strains surfaced quite soon. Clearly, both the parasite and its mosquito vector were more than a match for the weapons produced by science.

In this background we might well ask: 'What do the genome sequences of *P. falciparum* and *A. gambiae* offer in the fight against malaria?' The answer seems to come from the sequencers themselves: 'In the short term, however, the genome sequences alone provide little relief to those suffering from malaria. The work reported here and elsewhere needs to be accompanied by larger efforts to develop new methods of control, including new drugs and vaccines, improved diagnostics and effective vector control techniques. Much remains to be done' (M. J. Gardner *et al.*, *Nature*, 2002, **419**, 498). Despite this sombre assessment, the completion of the genome sequences of the parasite and its mosquito vector marks a milestone in the long struggle against malaria. The genome sequences are a remarkable resource for biologists attempting to unravel the details of parasite biology and biochemistry, which may be essential for developing rational methods of control. In war, it is essential to understand the enemy; *Plasmodium* has proved a sophisticated and devious foe. The life cycle of the parasite requires two distinct hosts, the mosquito and the human. Even in the human, the parasite resides at different stages in two distinct cells, the hepatocytes in the liver and the erythrocytes in the blood. The parasite reveals itself only fleetingly in the bloodstream, possessing an uncanny ability to vary its exterior; a skill invaluable in evading immune surveillance. *Plasmodium* also prefers to multiply in the confines of the erythrocyte, largely hidden from a potentially hostile environment. It is this remarkable biological sophistication that has made the development of diagnostics and vaccines so difficult. The parasite's ability to learn to defuse drugs directed against its metabolic machinery makes it a truly formidable enemy. The genome sequence of *P. falciparum*, translates into about 5300 predicted proteins. At present about 60% of these do not bear 'sufficient similarity to proteins in other organisms to justify provision of functional assignments'. Put simply, our ignorance of the details of parasite biochemistry is substantial. But gene sequences are only the tip of the iceberg. It is essential to identify the protein products of expressed genes, at every stage of parasite development. Here the emerging tools of proteomics, high resolution protein separation coupled with mass spectrometric analysis of sequences, hold the key. The first major steps in analysing the *Plasmodium* proteome, which may eventually provide valuable clues on antigen variation, have just been reported (L. Florens *et al.*, *Nature*, 2002, **419**, 502; E. Lasonder *et al.*, *ibid*, 2002, **419**, 537).

The publicity over the announcement of the genome sequences of the parasite and the mosquito has turned the spotlight, albeit briefly, on malaria. Research on the disease is, however, poorly funded with an annual worldwide investment of $ 200 million per year; an amount unlikely to permit major advances based on the bridgehead of the genome sequences. Support for malaria research has largely come from charities like the Wellcome Trust and, more recently, the Gates Foundation. The disease is not a subject of major interest for the giant pharmaceutical companies, who prefer to invest their research resources on more lucrative areas. The estimated market for a malaria vaccine is small, less than half a billion dollars annually. This figure may be unattractive for companies which are loath to enter a race for products that have a potential market of less than a billion dollars, annually. Malaria, is truly an 'orphan disease' with potential drugs and vaccines aimed at the poorest populations in the world. With the departure of colonizing armies, the incentives for major investments by the developed countries on the fight against malaria are not readily apparent. Ronald Ross was moved to poetry when he saw the malarial parasite in the stomach wall of the mosquito. His verse has been widely quoted.

This day relenting God
Hath placed within my hand
A wondrous thing; and God
Be praised. At his command

Seeking his secret deeds
With tears and toiling breath
I find thy cunning seeds
O million-murdering Death.

I know this little thing
A myriad men will save.
O death, where is thy sting
Thy victory, O Grave?

After the blaze of publicity over the genomes dies down, present-day malaria researchers may be more circumspect.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Oxygen, Lavoisier and Revolution

Sune Kallstenius: You always complain about the time spent on Nobel Committee business.
Bengt Hjalmarsson: All I seem to be doing is reading other people's papers.
SK: How else can we come up with a list of candidates?
BH: What about my important work?
SK: Most Swedes would be proud to pay that price.
BH: I'm tired of paying it! No wonder Swedish chemists don't win the real Prize!
.....
SK: So resign.
BH (Grins): From the committee? I like the power... and the gossip.
SK: Now you have double power: Picking regular Nobel prize winners as well as retro-Nobelists. First the living... now the dead.
BH: The dead don't repay favours.

Oxygen
A play in 2 acts
C. Djerassi and R. Hoffmann

October is the season for the Nobel prizes. For a brief period of a week, the daily announcement from Stockholm focuses attention on science and scientists. The Nobel awards for physics, chemistry and medicine have a degree of credibility, unmatched by the prize for literature and the award for 'peace', announced from Oslo, which quite often is conferred on politicians of questionable merit. The economics prize, an after thought instituted by the Bank of Sweden, has not gained in prestige by continual awards to American economists, for work in an area which has been characterized as a 'dismal science'. The Nobel prizes in the sciences, however, have acquired an enormous distinction in the century that has passed, since the first awards were handed out in 1901. As Harriet Zuckerman notes: 'At the outset before the prizes had acquired their present standing, the Nobel Foundation honoured itself in the course of honouring scientists of great stature. In effect, the Nobel prizes received annual infusions of prestige borrowed from the esteem long accorded the eminent scientists who agreed to accept a prize. This process of prestige accumulation by the Nobels meant that later, when awards were occasionally made to scientists of considerably lower standing, they would acquire a worldwide eminence largely derived from their having been named Nobel laureates' (*Scientific Elite*, The Free Press, New York, 1997, pp. 35–36). Today the awards instantly elevate their recipients to an 'aristocracy in science'. Last year the Nobel Foundation marked the centenary of the prizes, whose stature has remained undimmed by the passage of time. The Swedish government, which evidently considers the Nobel awards as its most distinctive export, even used the celebration as an occasion to promote Swedish business overseas.

In thinking about the Nobel centenary, I chanced upon an extraordinary play, *Oxygen*, authored by two of the most distinguished chemists of our times, Carl Djerassi and Roald Hoffmann (Wiley-VCH, Weinheim, 2001). Djerassi, the moving spirit behind the steroid oral contraceptive, a prolific contributor to the chemistry of natural products, is the recipient of both the National Medal of Science and National Medal of Technology. Hoffmann, a leading theoretical chemist, was a Nobelist in 1981 for his work on the conservation of orbital symmetry in certain classes of chemical reactions. Djerassi and Hoffmann are also distinguished by their literary talents; the former authoring as many as six novels in addition to poetry, drama and serious works on science and policy, while the latter has written three books of poetry and has been at the forefront of efforts to enhance the public perception of chemistry. In their short and gripping play, *Oxygen*, Djerassi and Hoffmann suggest an interesting way of celebrating the Nobel centenary—have the Nobel Foundation award retro-Nobel prizes for the science of the 18th and 19th centuries. The prefix *retro* is an old favourite with chemists; retrosynthesis, for example, signifies the process of deconstructing a target molecular structure, as the first step in devising a strategy for its construction from simple building blocks; the process of synthesis. But, in pondering about retroactive awards, the task of choosing the first recipients appears impossibly complicated. Djerassi and Hoffmann imagine the predicament of a Nobel committee in 2001, worrying about the possible recipients of the first retro-Nobel in chemistry. In thinking about the origins of modern chemistry one name must spring to mind, Antoine Lavoisier (1743–1794), who led the revolution that established modern

chemistry as a quantitative science. Lavoisier's *Elementary Treatise on Chemistry* was published in February 1789. On 14 July 1789, the Parisian mobs stormed the Bastille, signalling the beginning of the French Revolution. A month later, almost in the early weeks of the revolution, Lavoisier was nearly 'lynched by an angry mob of Parisians, during the course of his duties for the government of the times. Lavoisier ignited the sparks of the chemical revolution in the midst of one of history's most dramatic political and social revolutions, in which he was centrally involved. On 8 May 1794, Lavoisier was guillotined at the Place de la Revolution during the infamous Reign of Terror that ruled Paris in the frenzied days of revolution (*Antoine Lavoisier, Science, Administration and Revolution*, A. Donovan, Cambridge University Press, 1993). Several decades later Adolph Wurtz extravagantly claimed: 'Chemistry is a French Science; its founder was Lavoisier of immortal fame'. (quoted by D. Knight in the preface to Donovan's biography).

Lavoisier brought to chemistry the approach of the experimental physicists of his time; a belief that a rigorous, quantitative, experimental analysis of chemical phenomena would eventually provide the basis for a unifying theory of chemistry. Lavoisier had an extraordinarily successful career in the years preceding the revolution, as a 'tax farmer' and financial administrator. He brought the philosophy of the careful accountant to the study of chemical reactions. Lavoisier, more than anyone else, established the importance of the conservation of mass in chemical transformations. Lavoisier's remarkable insight that combustion involved combination of materials with oxygen, provided the decisive evidence against the 'phlogiston theory', which held that when substances burned, a mysterious entity, 'phlogiston', escaped into the air. To Lavoisier, combustion involved the process of chemical combination with oxygen, a component of air.

Djerassi and Hoffmann, through the fictional retro-Nobel committee ask: 'Who should get the award for the discovery of oxygen'. The candidates are, of course, Lavoisier, Joseph Priestley an English clergyman and Carl Wilhelm Scheele the Swedish apothecary. The play flits back and forth between the Nobel Committee in 2001 and Stockholm in 1777, where the three contenders have assembled at the invitation of King Gustavus III to resolve directly a dispute on the priority for the discovery of oxygen. Even as the drama moves across the centuries, scientists will quickly appreciate the emotions that accompany scientific discovery and the often dominating desire to be first and of course, to be publicly recognized for the achievement. The fictional conversation in *Oxygen* between the Nobel committee members say it all:

Ulf Svanholm: Because science is done by scientists... not machines... and scientists crave recognition.
Astrid Rosenqvist: Science is done by humans... humans are competitive... scientists are even more competitive... and they want to be rewarded for being first.
Sune Kallstenius: Of course! But we still haven't agreed what 'being first' means: is it the initial discovery... the first publication... or full understanding?

Djerassi and Hoffmann have woven a fascinating drama around the discovery of oxygen. Scheele was the first man to make oxygen but did not publish his results, but did write to Lavoisier providing a recipe for its synthesis. Priestley made oxygen in 1774 but, like Scheele, he did not fully recognize the significance of his discovery for chemistry. Lavoisier, integrated oxygen into his view of combustion, discrediting decisively the phlogiston theory, which Priestley espoused for several years after it became clear that it had no basis in experimental fact. As the playwrights note in their foreword: 'Scheele and Priestley fit their discovery into an entirely wrong logical framework—the phlogiston theory—that Lavoisier is about to demolish. How does Lavoisier deal with the Priestley and Scheele discoveries? Does he give the discoverers their due credit? And what is discovery after all? Does it matter if you do not fully understand what you have found? Or if you do not let the world know?' The history of science is replete with examples of independent discoveries making apportioning of credit a difficult and often, contentious process. As the fictional Nobel committee discovers in *Oxygen* the selection of 'retro-laureates' may prove as difficult as deciding on contemporary awards.

But in the end we must return to Lavoisier. In his career as an administrator Lavoisier was a conservative. As a scientist he was a true revolutionary, prepared to vigorously challenge prevailing dogma. Ironically, Priestley, a whole-hearted supporter of the French revolution, while living in royalist England, was conservative in his view of chemistry. Donovan's biography highlights in a footnote a remarkable communication from Lavoisier to Priestley: 'In July 1791 Joseph Priestley organized a celebration in Birmingham to mark the anniversary of the fall of the Bastille. Later that day a Church and King crowd destroyed his home, library and laboratory'. Lavoisier's letter to Priestley evokes a wonderful image of the times: *'As a Citizen, you (Priestley) belong to England and it is for her to atone for your losses; as a Scholar and as a Philosopher you belong to the entire world; you belong above all to those who know how to appreciate you, and it is we, united in agreement, who vow to restore to you the instruments which you have employed so usefully in our instruction. We have therefore resolved to reestablish your Cabinet, to raise again the Temple which ignorance, barbarity and superstition have dared profane. What more important service can we render to science than to place in your hands the instruments necessary for its cultivation?'*.

A study of Lavoisier and his times may benefit all those who try to understand the imperatives that drive both scientific and social revolutions.

P. Balaram

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EDITORIAL

Drugs control: A slippery slope

Controversies surrounding the drugs and pharmaceuticals industry are not uncommon. The widespread and often, indiscriminate use of pharmaceuticals fuels a worldwide demand, which is serviced by a ruthlessly competitive industry. There is little doubt that nearly a century of pharmaceutical research has contributed spectacularly to improvement in human health and quality of life. Chemotherapy has its origins in the, now classic, work of Paul Ehrlich. The battle against bacterial infections was truly joined when Gerhard Domagk discovered 'prontosil', introducing the era of sulfonamide therapies. Alexander Fleming's, historic and serendipitous discovery of penicillin in 1929, was to be followed by the insights of Howard Florey and Ernest Chain and the exigencies of World War II, to herald the age of antibiotics. Rene Dubos' work at Rockefeller on gramicidin is sometimes overshadowed in popular histories of the discovery of antibiotics; the glamour of the penicillin story invariably proving irresistible. Drug research in its classic phase has been invested with an aura of romanticism, rarely associated with many other areas of science. Accidental discoveries which save human lives abound in the literature of classic medicinal and pharmaceutical chemistry. An interesting, relatively recent example is George Lesher's discovery of the antibacterial agent, nalidixic acid during a synthesis of the antimalarial, chloroquine. Serendipity, has been more often associated with drug discovery than rational, purposeful design.

A remarkable range of drugs, to combat infectious disease, control cardiovascular ailments, treat cancers and provide palliatives in the case of central nervous system disorders, have completely transformed the practice of medicine over the past half a century. Together with significant advances in diagnostics and vaccines, for the early and definitive detection of disease and the prevention of infection, the progress of drug research has contributed to the advancement of human health care in both the developed and developing countries of the world. But, the growth and increasing financial power of the major pharmaceutical companies, the escalating costs of drug research, rapidly changing business strategies and the growing complexities of international patent regimes, have brought in their wake many problems which merit careful analysis.

The recent controversy on the indiscriminate use of the drug nimesulide ('Nimulid') for treatment of pain and fever has focussed on the possible side effects, particularly the induction of liver complications in children. Nimesulide, a non-steroidal anti-inflammatory agent has rapidly become a 'best-seller', with estimated annual sales of over Rs 200 crores in India. Several Indian companies manufacture the drug, although multinationals have not yet marketed the drug in India. Interestingly, nimesulide competes with common drugs like paracetamol (acetaminophen), ibuprofen and aspirin for treatment of the most common of ailments, fever and pain. Critics of nimesulide point out that the drug was never approved by the US Food and Drug Administration (FDA); approval for use in the United States is often considered as the most stringent test that a new drug must pass. While, the drug was initially introduced in the 1980s in Italy, the European countries have recently been alert to possible complications following its use. In India, however, the use of nimesulide has been growing; doctors often tending to prescribe a drug, which is aggressively marketed. Since fever and pain are invariably treated by general medical practitioners or by self medication by patients, marketing strategies are likely to win over any rational assessment of efficacy versus toxicity, in a real clinical setting.

One of the harsh realities of routine medical practice in India is that for many medical practitioners, the sole source of information about the pharmaceuticals they prescribe are the ubiquitous sales representatives, who can be found hovering about in any neighbourhood clinic. Pharmacology and toxicology are not important subjects in the curriculum of medical courses; the continuing education of doctors, particularly the general practitioner, is not usually a matter of concern. The responsibility for ensuring, to the extent possible, the safety of widely used drugs rests with the Drugs Controller of India, whose office falls within the broad sphere of

responsibility of the Ministry of Health. Does the Drugs Controller's office have the necessary wherewithal to make scientifically valid decisions on drugs? In an area whose technical complexity grows with each passing day, can an office functioning under a ministry, not noted for its scientific strengths, efficiently and credibly discharge its mandate? Can an office steeped in a 'ministerial culture' resist the pulls and pressures of competing pharmaceutical houses? The present attack on nimesulide and the publicity given to reports of its liver toxicity in children must undoubtedly have its origins in the strategic marketing wars that drug companies are prone to wage. After all the literature reports on the adverse reactions of nimesulide and other non-steroidal antiinflammatory agents have been known for some time. If there was reason for concern it is incumbent on the Drugs Controller to make these public, rather than to respond only when the popular press raises the issue, stridently. It is also necessary for manufacturers of nimesulide, particularly those with the muscle of research and development departments behind them, to provide convincing data that toxicity in local populations is not significant. Interestingly, many of these questions would not have arisen if nimesulide was freely marketed in the United States, a country known for its stringent drugs approval procedures and a legal system which provides rapid redressal for wronged consumers.

The nimesulide story brings to mind one of the most famous tragedies of drug research, the introduction of thalidomide for use in controlling 'the symptoms of morning sickness and nausea in pregnancy', in Germany, Britain and several other countries in the late 1950s. The first dreadfully deformed babies were born in the 1960s, although a toxic effect in the users, peripheral neuritis, was detected much earlier. The drug was withdrawn only slowly, by which time the damage was widespread; there are some estimates that as many as 10,000 babies were born deformed. While the effect of thalidomide in causing human foetal abnormalities was clearly established, it is not clear if the use of animals to test the drug's teratogenic properties would have prevented its use. Once again the FDA had rejected the application to market thalidomide in the United States; an alert watchdog worrying about the drugs ability to induce peripheral neuritis. Despite its notoriety, thalidomide is back; new uses having been discovered for this old drug. For sometime, thalidomide has found use in treating inflammatory conditions, particularly in the case of leprosy patients with painful skin nodules. The ability of thalidomide to act as an immunomodulatory agent, its property of inhibiting cell movement and its capacity to hinder the growth of new blood vessels, angiogenesis, promise many new clinical applications, including in cancer and metabolic wasting, cachexia, seen in AIDS.

These remarkable properties of thalidomide, a deceptively simple chemical entity, appear to have allowed the drug to resurface. The drug has now been cleared for use in specific situations like leprosy and possibly, cancer, in India. Predictably, questions have been raised in the press on the wisdom of having a potent teratogen freely available on the shelves of pharmacies throughout the country. Pharmaceutical companies are reluctant to introduce drugs, whose potential markets do not ensure a significant turnover. The commercial motivation behind the introduction of new drugs (and old drugs for new applications) casts a shadow over the processes of drug regulation and prescription.

Regulatory issues in the area of drugs control will acquire a new dimension of complexity as biotechnology products, particularly recombinant protein pharmaceuticals become more widely used. The issues of quality and identity will be added to the problem of toxicity evaluation. Whether the Drugs Controller's office is equipped to address these increasingly difficult technical problems must be a matter of public concern.

P. Balaram

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EDITORIAL

Chemistry: In search of icons

'Our old chambers had been left unchanged through the supervision of Mycroft Holmes and the immediate care of Mrs Hudson. As I entered I saw, it is true, as unwonted tidiness, but the old landmarks were all in their place. There were the chemical corner and the acid-stained deal topped table.'

The Adventure of the Empty House

'A remarkable wine, Watson.... Might I trouble you to open the window, for chloroform vapour does not help the palate.'

His Last Bow
Arthur Conan Doyle

A few weeks ago a forum of chemistry teachers in engineering colleges invited me to speak at the inauguration of a workshop on the 'revised syllabus in engineering chemistry'. They were clear and direct in their approach, telling me of their fears that chemistry would slowly be squeezed out of the engineering curriculum, rendering the teachers superfluous, unless they slowly moved towards teaching 'biotechnology'. My mandate, then, was to present a talk extolling the importance of chemistry in the education of engineering students, with the unrealistic hope that the assembled administrators would be influenced by the brief that I presented in the defence of chemistry. Like all such meetings, this one too passed off uneventfully; almost certainly chemistry would continue its inexorable slide in the esteem of the designers of engineering curricula. Coincidentally, not long after, my attention was drawn to a news report that the Royal Society of Chemistry had awarded an 'Extraordinary Honorary Fellowship' to Sherlock Holmes, the immortal and fictional detective, who was not only an amateur chemist, but also one who saw clearly the value of chemistry in the detection of crime; forensic science may have been born on the pages of Arthur Conan Doyle's engaging fiction. The resurrection of Sherlock Holmes (Conan Doyle fans would of course, recall that the author killed his most famous creation in a plunge to death over the Reichenbach Fall, while grappling with the infamous Prof. Moriarty in 'The Adventure of the Final Problem', but resurrected him in 'The Adventure of the Empty House') by the Royal Society of Chemistry might have been motivated by a desire to boost the public image of chemistry, a subject that seems to be increasingly in the grip of an identity crisis.

Not long ago, *Nature* in an editorial characterized chemistry as 'a discipline buried by success'. The journal went on to note: 'The subject's reputation is not a trivial issue. If chemistry is to achieve half as much in this century as it did in the last, it must fight to attract the brightest young scientists. And to do that, image is important'. (*Nature*, 2001, **411**, 399). Indeed, in these columns, some two years ago, I had wondered 'where has chemistry gone', noting that 'the richness of chemistry clearly lies in its role as a central science' (*Current Science*, 2000, **79**, 927). A news feature that appeared last year however begins: 'Chemistry likes to style itself as the "central science", but perhaps bridesmaid science would be more appropriate'. The article goes on to argue that many new developments in chemistry, 'from the discovery of life-saving drugs' to the 'work on carbon nanotubes ... often seem to end up being appropriated by other disciplines' (*Nature*, 2001, **411**, 408). Ironically, with over 160,000 members, the American Chemical Society counts itself as the 'world's biggest scientific organization'. The declining image of chemistry was highlighted by a recent survey in the United Kingdom, which raised an alarm about the dramatic decline in the number of students entering university chemistry departments, leaving 'up to a dozen chemistry departments in England vulnerable to closure' (*Nature*, 2002, **416**, 777). Chemistry is, of course, not alone in its struggle to attract students to its fold; physics has been struggling for sometime too, but that hardly seems a consolation for a discipline, which considers itself as critical for the advancement of science on a broad front, ranging from biology to materials science. The *Nature* report rubs salt into the wound by quoting 'a department head, whose physical chemistry laboratory is being handed over to sports science' as describing the move as one of 'redistributing resources'. With university funding in the UK being allocated on the basis of a 'research assessment exercise', departments with falling graduate student populations face grant cuts.

What is the status of chemistry in India? The view from the colleges is unencouraging. Majoring in chemistry is hardly an attractive option, with most undergradu-

ate courses having remained almost frozen in time, reflecting little of the phenomenal progress the discipline has made over the past fifty years. Since engineering and medical courses skim off the cream and commerce, management and computer applications appear to offer better employment opportunities, it is only the most highly motivated or those who have little choice that turn to science degrees. The amazing, and at times frightening, rise of undergraduate biotechnology courses, with little substance but full of the false promise of future opportunity, has further diminished the demand for undergraduate degrees in chemistry and physics. The increasing competition from foreign universities for students in these areas, both at undergraduate and postgraduate levels has not helped. At the level of research institutions the entry of students to Ph D programs in the physical sciences is also on the decline; all except the most sought-after institutions are hard hit. The situation in some colleges and universities for chemistry courses is alarming; empty laboratories and idle faculty may soon be the norm. Interestingly, chemistry is still a subject which provides employment for its practitioners. The chemical industry in India is well established and pharmaceutical R&D is growing rapidly, creating a constant demand for well trained organic, medicinal, analytical and polymer chemists. This may be contrasted with the prospects for 'biotechnology' graduates, who specialize only cursorily in a specific area and are dependent for employment on an industry, which appears to be more a creation of writers for business magazines and the publicity arms of government departments. Curiously, the biotechnology industry may eventually need more chemists and chemical engineers, than 'biotechnologists', if marketable products are to emerge in profusion.

Indian chemistry has also been characterized by an extremely conservative approach to emerging disciplines and by an inability to restore old and dying areas, which have acquired a new dimension because of advances in related fields. Almost up to the 1960s, the chemistry of natural products, primarily of plant origin, was a subject practised with considerable success. But, over the years the area has fallen out of fashion, sadly enough at a time when instrumental methods for separation and analysis of complex molecular mixtures has reached an unparalleled level of sophistication. The present day talk of exploiting biodiversity and traditional knowledge for discovering new molecular entities as the pharmaceuticals of the future, will remain idle, committee room discussion if a new generation of natural products chemists are not created. Chemists in India have also been slow to recognize opportunities at disciplinary boundaries; many chemistry departments in the US have reinvented themselves as departments of chemical biology, a signal that chemistry's frontiers must transcend traditional borders. Even in the area of materials science, physicists and engineers appear to have occupied the middle ground; preparative chemists have been left to graze on the fringes. The preparation of new and novel materials must be central to chemistry, but the composition and structure of most of our chemistry departments would hardly suggest that this is the case. The greatest blow to experimental chemistry, and here my prejudice must show, has been dealt by the overemphasis on computational and theoretical chemistry. Our major institutions abound with theoreticians and students often acquire a slanted view of, what is undoubtedly, a subject which thrives on experimentation. In our surroundings computational work appears to be a more attractive option; *Gaussian* sometimes seduces even the talented experimentalist. My suspicion is that 'biotechnology' too will go the chemistry way, if students are taught that bioinformatics is the true wave of the future. Chemists in India also cling too tightly to their disciplinary affiliations; the lines between organic, inorganic physical and theoretical chemistry sharply drawn. Subjects like analytical and polymer chemistry are practised by too few to be noticed. The reactions of most chemists I encountered after the award of this year's Nobel prize in chemistry are illustrative. While the Nobel committee appeared to be recognizing the prime importance of analytical methods in chemistry, most chemists appeared to regard Kurt Wüthrich, who successfully demonstrated the application of new NMR methodologies to the structure determination of large molecules as a 'structural biologist'. Very few seemed to regard the soft ionization procedures in mass spectrometry developed by John Fenn and Koichi Tanaka as critical to the development of chemical analysis. In all three cases the extraordinary applicability to molecules of biological interest seem to divorce these techniques from the realm of classical chemistry. This ability to look constantly inward and to exclude important areas appears to be a trait of chemists, worldwide. As *Nature* notes '....eye catching interdisciplinary research offers chemistry a chance to polish its image. But first, chemists must learn to celebrate and promote their contributions' (*Nature*, 2001, **411**, 399).

Chemistry's public image often suffers in comparison with the sister disciplines of physics and biology. It does not have the glamour of the competing disciplines. Physicists can promise that their subject will cast light on the origins of the universe or eventually construct a 'theory of everything'. Biologists can revel in the mysteries of heredity or the grand philosophical implications of Darwinism and discuss the origins of man. Chemistry in its entirety has its roots firmly in the ground. It is a mundane, but critically important, science.

But, at the end I might ask, after Sherlock Holmes can chemistry recruit another icon to embellish its image? One possibility would be to turn to Isaac Newton who for almost six decades, spanning the late 16th and early 17th centuries, relentlessly practised alchemy (Dobbs, B. J. T., *The Janus Faces of Genius*, Cambridge University Press, 1991). With Newton and Holmes, promoting the discipline's public image, chemistry's star must surely ascend.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Requiem for a missing generation

You are a lost generation.

Gertrude Stein to Ernest Hemingway

One generation passeth away and another generation cometh: but the earth abideth forever.

Ecclesiastes

The American author and connoiseur of the arts, Gertrude Stein (1874–1946) coined the phrase 'lost generation' to describe the band of American authors, which included Ernest Hemingway and F. Scott Fitzgerald, who settled in Paris in the years between the two World Wars. The American expatriates were to produce a remarkable body of literary work, one of the earliest novels being Hemingway's, *The Sun Also Rises*. In the preface the author quoted Stein, immortalizing the phrase 'the lost generation'. He also went on to quote from the *Bible*; a reflection on the transition between generations. My search for Stein and Hemingway was catalysed by a recent 'News Focus' piece in *Science* entitled 'Missing generation leaves a hole in the fabric of research'. Here was an article, datelined New Delhi, which attributes to some of India's scientific leaders the phrase 'missing generation' to describe the absence of 'leaders in the age group of 45–55 years' (*Science*, 2002, **298**, 733). The fact that an entire generation of scientists in India is devoid of men and women of substance and accomplishment appears to have been made by the most prominent members of a generation of scientists and science administrators, who have had the opportunity to guide the destinies of institutions, academies and government science departments, for much of the past quarter of a century.

How did a group of men, so widely experienced, so remarkably accomplished and so politically astute come to such a clear, and apparently, unanimous conclusion? The evidence on which the 'missing generation' has been postulated, appears to be the inability of search committees to find scientists in India, suitable to head the Tata Institute of Fundamental Research (TIFR) in Mumbai. The choice of a scientist, who has spent almost his entire research career in the United States as the new TIFR Director and the attendent discussion in the popular press appear to have provided the immediate provocation for the news report in *Science*. Even earlier, yet another scientific leader was imported from the United States, for the Harish Chandra Research Institute in Allahabad (formerly the Mehta Research Institute), curiously enough an organization which, like the TIFR, falls under the ambit of the Department of Atomic Energy, whose leaders never tire of trumpeting the importance of indigenous scientific and technological development.

Science in its report notes that the 'phenomenon' of the 'missing generation' takes many forms including the 'appointment of the first outsider to lead one of the country's flagship labs. . . . It's also contributed to a graying at the top of the scientific hierarchy'. In a damaging commentary on the state of science in India, the report quotes a member of the Council of TIFR as saying 'that the search committee looked high and low for domestic talent even perusing the membership lists of the country's scientific academies', but drew a blank everywhere. The irony will not be lost on those who know that we have a profusion of 'Academies' and that many of India's scientific leaders, who are quoted in this report have presided over these organizations, whose most important objective seems to be the election of new members. In rationalizing the move to look elsewhere for leadership we are told that we must 'ruthlessly go for the best'. Indeed the private sector and the Board of Control for Cricket in India would applaud this sentiment; the latter relying on foreign coaches and trainers for some time. The 'go for the best' strategy is unexceptionable and it would be interesting to apply this principle across the board. We would do well to ask: Are the heads of administrative departments that control science the very best? Are the councils of management that govern our institutions the very best? Do we have mechanisms in place that dispassionately assess the heads of institutions and scientific departments? The answers to these questions might all be negative, leading to the conclusion that a new era has dawned, where India's long enduring scientific leaders have suddenly decided to be 'ruthless', in their relentless search for new leadership, at the frontlines of battle.

The current controversies do highlight some important issues; considering a few of them may be instructive. Many of our autonomous institutions are governed by Councils of Management, whose constitution leaves much to be desired. Very often the Chairmen (and there are very few women in these positions) of these Councils have little more than a passing interest in the institutions under their charge. Their tenures are long and their disinterest or self-interest most manifest when there is a

change of executive leadership in the institutions. Councils have a limited role when institutions are run by strong executive heads and have in place fair and robust internal mechanisms. However, they have a pre-eminent responsibility when there is a change of leadership, in identifying and recruiting new heads for the organizations, over which they are expected to exercise a gentle, supervisory role. I must hasten to add that the problems of institutional councils are not only a present-day phenomenon. In 1936, a Quinquennial Review Committee (the Irvine Committee, under Sir James Irvine) was appointed to review the Indian Institute of Science (IISc). The institution was then in a turbulent state. C. V. Raman was to resign as its director in 1937. In 1939, this journal noted in an editorial: 'We doubt whether the Institute really suffers from any clash of personalities, but we are prepared to admit that there is almost an irreconcilable war of ideals, arising from two academic luxuries which the Institute can hardly afford. The first of them is apparently a body of elderly authorities composing the Governing Council, whose imagination has become by routine administration as rigid as perhaps their arteries, on account of their advancing age: the other is a group of ardent young students and members of the staff who, inflated with noble ideals, are impatient to dash forth to win their spurs in extending the frontiers of knowledge. . . . The former is as dangerous a symptom as the latter is a healthy sign, for a generous enthusiasm and sterling ambition, when wisely harnessed and judiciously directed, must at all times constitute an invaluable asset for the Institute' (*Curr. Sci.*, 1939, **8**, 197). The editorial written over six decades ago goes on to question the constitution and responsibilities of the Governing Council of IISc; questions which remain valid and relevant for most of our autonomous institutions, including TIFR, to which the report in *Science* drew attention.

A second issue which may be useful to address is whether the carrot of administrative and executive authority the sole means by which accomplished Indian expatriates are attracted to return to our institutions? While many schemes for attracting early career scientists to return to India have been advanced, there has been little success in luring more established and most importantly, visible and active scientists of Indian origin to return to this country. Many countries which would like to advance rapidly in an increasingly competitive world like China, have attempted to put in place new mechanisms to reverse the direction of flow of scientists to the West. The inducements to return have included the creation of elitist laboratories with exceptional facilities (*Nature*, 2002, **420**, 257). In this context we may recall the abortive, contentious, but nevertheless stimulating, proposal to establish a National Science University as an essentially non-resident Indian initiative, with substantial government support. In his devastatingly worded critique of our scientific structure as it stood in 1994 (and little has changed since then) the author, Swadesh Mahajan said: 'It is not science that brings glory, fame or fortune; it is scientific administration. It is not excellence in scientific research that makes one a leader, but the administrative title of 'Director' (or an equivalent) that legitimately confers leadership upon a person. The director is not only absolutely powerful but must also be the most important scientist when he is appointed director and for all times to come. No one may have a greater scientific stature or ambition than the director whose specific interests take precedence over everyone else's' (*Curr. Sci.*, 1994, **67**, 503). Mahajan summed up a perception of Indian institutions and their leadership which was, and is, widely shared. Unsurprisingly, it is difficult to attract senior scientists of accomplishment to our institutions unless they are vested with administrative authority. We might also expect that leaders of institutions have a clear vision of the immediate future of their organizations. This is rarely the case; most often, the wand of executive authority is used to facilitate a personal scientific agenda.

The report in *Science* ends on an ominous note, emphasizing that the secretaries of seven major departments have exceeded their terms. The case for extensions becomes strong since we are now clear that there is a 'missing generation'. For good measure the article ends with an implied threat, quoting yet another senior scientist, that career bureaucrats may step into the line of succession at the 'science ministries'. While there must be every effort to facilitate the research and academic work of productive, serious and scholarly, senior scientists beyond the age of formal retirement, the practice of extending executive and administrative authority is a pernicious one. Political skills are often more important than administrative talent, leaving many of our institutions and departments irretrievably damaged after extremely long tenures of their administrative heads. There are, of course, honourable exceptions; but we must be wary of administrators whose attachment to their chairs exceeds their commitment to science and their institutions. In thinking about the many difficult, and at times unpleasant, issues addressed in the *Science* report, I was reminded of Oliver Cromwell's famous exhortation to the Long Parliament: 'You have sat too long for any good you have been doing. Depart, I say, and let us have done with you. In the name of God, go.' Cromwell was famously quoted by the Conservative MP Leo Amery, in the British Parliament in May 1940, when he rose to demand the exit of the Prime Minister, Neville Chamberlain, in one of the defining moments of the early days of World War II.

I shall end as I began with Gertrude Stein. In her Paris salon, she was among the first to recognize the talents of Pablo Picasso and Henry Matisse. And, of course, she befriended the young Hemingway. She identified a generation of artistic and literary talent. They were 'lost' only from their moorings in post-World War I America. The men who rule India's science have coined a remarkable phrase, which reminded me of Stein, to arrogantly dismiss an entire generation of scientists in India; many of whom have laboured diligently, with extraordinary commitment to their chosen disciplines, silent and at times forlorn, witnesses to deteriorating standards of decision making. This essay might serve as a requiem for the 'missing generation'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The mores of publishing in science

Mores: *Folkways of central importance embodying the fundamental moral values of a group.*

– The Random House Dictionary

Mores: *Customs or conventions regarded as essential to or characteristic of a community.*

– Oxford English Dictionary.

Scientists function as a tribe. In the modern world, the mores of science transcend national boundaries and the barriers of culture and language. Journals and publications bind peer groups in diverse disciplines. Thousands of scholarly papers are written, refereed, edited and published every year; an exercise which involves the voluntary and enthusiastic participation of members of a far flung community, held together by tribal bonds, which few outsiders can completely comprehend. Scientists love to see their names in print; authoring a publication is akin to a rite of initiation.

'His name is Greek, his nationality is French and his history is curious. He is one of the most influential mathematicians of the 20th century. The legends about him are many, and they are growing day by day.' This description of *Nicolas Bourbaki*, a 'collective pseudonym used by an informal corporation of mathematicians', who set out to write a comprehensive treatise on mathematics appeared over 45 years ago when the Bourbaki legend began to grow (Halmos, P. R., *Sci. Am.*, May 1957, pp. 77–81). Here was a group of influential and enormously accomplished mathematicians publishing their scholarly work under a pseudonym derived from the name of a none too distinguished general, who played a minor role in the Franco-Prussian War, in the second half of the 19th century. Publishing under a pseudonym is not widely practiced in science, where authors clearly like to be identified and counted. It is, of course, a more common practice in literature; in English, Lewis Carroll (Charles Dodgson, a mathematician) of *Alice in Wonderland* fame and George Eliot (Mary Anne Evans), among whose works *Silas Marner* is probably widely known, consistently used a 'pen name'. The use of pseudonyms is common among authors writing in Indian languages. But, pseudonyms are an exception in the world of science, although Halmos in his article draws attention to the 1 April 1948 article in *The Physical Review*, which appeared under the byline of Alpher, Bethe and Gamow. The fictitious 'first author' was invented by his famous co-authors. Not too long ago, Carl Djerassi used pseudonymous authorship, as a tool to highlight in fiction the passions and jealousies that accompany the publication of path-breaking scientific work, accomplished by scientists working as a group (*The Bourbaki Gambit*, Carl Djerassi, Viking Penguin, 1996).

The authorship of publications has become of paramount importance in the life of modern, practising scientists. With the exception of scientists who work in industry or in government directed strategic projects, publications are the sole means of garnering peer recognition. Scientists have devised elaborate methods for ranking journals and the pernicious practice of quantitative assessments of publication lists have become commonplace; in India the disease of averaging journal impact factors has spread like a cancer. In every assessment committee publication lists of prospective candidates are carefully scanned; journals sifted according to their perceived ranking and in the case of multiple authors, personal prejudice and the position of the author's name on a long list are used to apportion credit. The 'first author' is often deemed to have been mainly responsible for the published work, while the 'corresponding author' whose name sometimes is placed at the end, like the anchor in a tug-of-war, is presumed to be the guiding force in the published research. As multi-author papers are the norm in the literature of modern science, the task of assigning credit (and at times, responsibility) has become increasingly difficult.

A new practice has sprung up. Author bylines sometimes carry a footnote which states: 'The first two (or on occasion, three) authors contributed equally to this work'. Symbols that appear as superscripts allow members of a group to stake a special, quantitative claim for their contribution to a collective enterprise. The best of journals have succumbed to pressure from authors to allow these footnotes; this journal is no exception (cf. Arun Kumar, A. N., Srinivasa, Y. B. and Chauhan, S. S., *Curr. Sci.*, 2002, **83**, 809; Soni, G. V. *et al.*, this issue, 1464). Why is it so important to be the first author or to establish equality with the first author? This is largely because a scientist's worth is most often judged by the

number of times his or her name appears as the 'first author' or the 'corresponding author', who presumably is the general marshalling his armies in battle. As the number of authors on an average scientific paper increases the issue of credit sharing becomes more difficult. In many disciplines several groups, with varied technical skills, collaborate to produce major pieces of research.

In thinking about the practice of assigning authorship credit in science, my attention was drawn to a provocatively titled commentary: 'Who wrote this paper anyway', authored by John Hoey, the Editor of the *Canadian Medical Association Journal* (*CMAJ*, 2000, **163**, 716–717). Hoey draws attention to a famous paper in the journal he now edits entitled 'Pancreatic extracts in the treatment of diabetes mellitus. Preliminary report' (Banting, F. G., Best, C. H., Collip, J. B., Campbell, W. R. and Fletcher, A. A., *CMAJ*, 1922, **22**, 141–146). This was the paper that announced the discovery of insulin, revolutionizing the management of diabetes. In 1922, this was a paper marked for its interdisciplinarity; physiology, biochemistry and clinical medicine had to coalesce for the discovery of insulin to be made. Interestingly, the 1923 Nobel Prize for medicine would go to Frederick Banting and John Macleod. In one of the most celebrated episodes of Nobel controversy, Banting, who felt that Macleod was undeserving shared his prize money with Charles Best, the man who developed 'the initial crude method of extracting the substance that would later be named insulin'. Macleod shared his prize money with James Collip, who 'refined the extraction process'. Discerning readers would note that Macleod's name does not appear on the list of authors of the original *CMAJ* report. In discussing in his commentary a statement on authorship issued by the International Committee of Medical Journal Editors (ICMJE, Vancouver 2000), Hoey notes that Macleod who 'made original contributions to the design and interpretation of the research' should have probably been an author. He goes on to add: 'But the corollary of *credit* is the ability to take *responsibility* for what is written. These are the twin attributes of authorship'.

The twin issues of sharing credit and responsibility have acquired a new dimension in the wake of the series of publishing scandals that have rocked science in recent times; most recently physicists have woken up to the fact that misconduct and deviant behaviour do not recognize national or disciplinary boundaries. The cases of Jan Hendrik Schon at Bell Laboratories and Victor Ninov at Lawrence Berkeley National Laboratory appear to have been resolved as straightforward cases of data falsification. Schon's work on solid-state electronic devices and molecular electronics and Ninov's contributions to the discovery of elements 116 and 118 are generally recognized to have been fabricated. The motivations are, as always, unclear; although the possibility has been advanced that these authors succumbed to the temptation of claiming success for experiments which will eventually work. But individual aberrations aside, the papers of Schon and Ninov carried an impressive and distinguished list of coauthors. The investigating committee examining the Schon affair shies away from questioning the responsibility of coauthors, labelling the problem as 'an extremely difficult issue, which the scientific community has not considered carefully' (*Chem. Engg. News*, 4 November, 2002, 31–33).

Closer to home, the case of B. S. Rajput, a Vice-Chancellor of a University has been widely written about. Extensive plagiarism has been alleged in a series of papers in the area of high energy physics (2002 may be remembered as the year physics lost its innocence). In this case the extraordinary defence has been advanced, that the 'corresponding author' was unaware of the publication. The question of how authorship of scientific papers must be granted is difficult to resolve. But, many unethical practices in our midst must be recognized. The inclusion of 'honorary authors' who hold important administrative positions in laboratories must be eliminated. Sometimes, the sharing of responsibility may act as a deterrent.

But, the changing face of publishing in science is most clearly evident in some of biology's mega-projects. The announcement of the 'Initial Sequencing and Comparative Analysis of the Mouse Genome' carried a simple author byline – *Mouse Genome Sequencing Consortium*. Readers must turn to the end of a long article to find a list of over two hundred authors from 46 different laboratories. Curiously, the paper carries another footnote which identifies 16 authors as having contributed to 'project leadership' (*Nature*, 2002, **420**, 520–562).

Charles Dodgson did not use his famous pseudonym when he wrote: 'Man is an animal that writes letters'. We might borrow his turn of phrase to define scientists as animals who like to publish papers. A defining characteristic of this tribe is their desire to see their names in print. Author bylines which highlight contributions to leadership, experimental work, analysis or even manuscript preparation are beginning to appear. Competing financial interests of authors or their lack must be declared in some journals. Clearly, the mores of publishing in science are in the midst of change.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Academic Appointments: Contracts and Tenure

The University Grants Commission (UGC) appears to have set the cat among the pigeons, by beginning consideration on a proposal to introduce contractual appointments in universities and institutions of higher education. Although no firm directives seem to have emanated from the UGC, the very idea that academic appointments may be imbued with an element of impermanence has provoked considerable discussion in the press. While teachers' organizations have viewed the proposal with suspicion and distrust, editorials and columns in some newspapers have welcomed the move. Viewed from the outside, 'the UGC's proposal to introduce a contract system in the appointment of teachers in colleges and universities is to be welcomed as this plan, if implemented, will help to improve the standards of higher education in this country.' (*Deccan Herald*, 15 November 2002). This expression of effusive support for the UGC proposal is based on a harsh and probably accurate assessment: 'It is well known that the higher education scene in the country is not very inspiring. . . . A teacher once appointed, can continue in his position till retirement, however unsatisfactory his performance is. Security of tenure breeds complacency and inertia.' But, to thousands of academics across the country the UGC's move appears to be designed to create uncertainty and confusion among the ranks of college and university teachers. A contract system selectively applied to academics appears inherently unfair in a system, where accountability is hardly ever demanded of the higher levels of decision making. But, the UGC's proposal may be driven by another motivation; the proposed contract appointments, in some measure, will be financially underwritten by central guarantees, permitting academic institutions to recruit fresh faculty, albeit on a contractual basis. This would address an issue of considerable concern to most university departments – the problem of an aging faculty depleted by retirements. For several years, many universities and colleges have found it difficult to recruit new members of the faculty, burdened as they are by financial problems and directives from state governments, which almost completely prohibit fresh induction. The state universities have been particularly hard hit but some central institutions have also struggled to bear the burden of hiring new faculty. Viewed in this background, the UGC's proposal might be charitably considered as an attempt to facilitate the infusion of fresh blood into our academic faculties.

Why then are contract appointments greeted with such concern by teachers' organizations? The most clearly stated fear is that a contract system would be used by administrators to limit academic freedom and to develop a system of patronage, which would be detrimental to any academic institution. The contract system would also appear to be a difficult option in an environment where job security is prized, mobility is restricted, job opportunities are limited and unemployment benefits and social security are unheard of. However, it must be emphasized that in many institutions in India the first appointments are contractual, with permanence or tenure assured only after an initial period of five years. In these cases, the conversion to a 'tenured' position is often preceded by an academic assessment, but this process is generally perfunctory. Contracts are rarely terminated on the grounds of non-performance.

The situation in India may be contrasted with the academic system in the United States where the concept of 'tenure' has been used to maintain extremely high standards of academic performance in the best of universities. The American research university system has developed extremely robust standards for assessment of research and teaching. The standards expected to attain 'tenure' are generally clear and by and large, the assessment system appears fair and equitable. The American system developed over a century, with the Association of American Colleges and Universities enunciating their views on 'academic freedom and tenure' periodically over the years 1925–1990. As the liberalization and privatization process moves forward in India, it might be valuable to reflect on the present system of higher education in India and to consider strategies for stemming and even reversing the inexorable tide of decline in our universities.

Few will argue with the proposition that universities in India need a new infusion of dynamic and committed faculty. Over the last couple of years both the UGC and the Department of Science and Technology (DST) have

introduced new schemes to support the development of infrastructure for scientific research in university departments. The funding for academic research programs has shown an upward trend, although the number of research projects supported in universities is still relatively small. Unfortunately, these positive developments are unlikely to have a measurable impact, if urgent steps are not taken to recruit faculty to university science departments. The ability of many state governments to bear an increased salary burden is limited, necessitating an infusion of central funds, through the UGC. 'New blood' appointments could be supported centrally, with a measure of control on the quality of the appointed faculty. But it may be necessary to consider centrally funding these positions over an extended period of time, since most state institutions are unlikely to absorb an enhanced financial burden after 3–5 years.

Should such appointments be contractual? It would appear that the time may have come, where a beginning can be made in introducing an element of evaluation, before the grant of tenure. What are the parameters on which an assessment can be made? It is almost self-evident that academics must be evaluated on the basis of performance in research and teaching. The former is particularly important in departments and institutions which have been declared as centres of excellence, but must not be neglected in all centres of post-graduate education. Teaching must also be judged; the spectacle of absent teachers and classes that are not conducted does not bring credit to the community. But, most importantly the evaluators must be seen to be fair; and they must be fair. It is here that the Indian university system faces its severest problems.

Few Vice-Chancellors, Directors or Heads of Department are respected by the communities over which they wield administrative authority. In attempting to democratize the functioning of many bodies in universities, academic anarchy has flourished. In many places, important bodies like university syndicates and academic senates have been corrupted and destroyed. Unsurprisingly, the idea of contracts and tenure in academic appointments is viewed with intense suspicion by teachers and researchers in our universities. The UGC might, in fact, consider measures to tone up the administration of academic institutions, including a measure of external review of high level appointments in universities. It is a welcome sign that money for major research facilities and faculty appointments at universities may, at least in part, be supported by central agencies. But, beyond financial support, it may be necessary to find new ways of ensuring that our universities are able to steadily recruit new faculty, based on performance and promise. A young, dynamic faculty may be a pre-requisite in making an academic department an attractive option for students.

In recent times, the emphasis on science in India has focused primarily on central institutions, national laboratories and the monolithic, strategic research departments. The universities have largely been left to their own devices, buffeted by changing political circumstances in the states. The science departments in colleges and universities have been steadily downgraded, with falling student and faculty strengths. Ironically, the universities with their sprawling and sometimes, desolate campuses invariably host the annual Indian Science Congress, at which the expressions of political will to support science are periodically enunciated. This year was no exception, with Bangalore University's enormous campus playing host to the many luminaries, who represent the establishment charged with the responsibility for fostering science and promoting its development and application. As always, there were visions; this time for a global space community. But, it all seemed far removed from the concerns of the institution that so graciously hosted the Congress. The Indian scientific enterprise necessarily draws its talent from the pool of science students graduating from our universities. It is imperative, therefore, to stem the tide of decline and to plot a strategy for the restoration of science to its rightful place within our system of higher education. Despite the controversies over the modalities of the proposal being considered by the UGC, any step that facilitates new faculty induction, with necessary controls on quality, would be a step in the right direction.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Rise of the Non-Resident Indian

The benchmarks for success, which the Pravasi community has set, are a challenge for us in India. They make us examine why the Indian is so much more innovative, productive and successful abroad than in his own country. They prod us to create a business, investment and economic climate, which is as conducive to success as anywhere else in the world. I assure you we are fully committed to creating such an environment in India.

—Atal Bihari Vajpayee
9 January 2003
Inaugural address at the Pravasi Bharatiya Sammelan, New Delhi

Ye friends to truth, ye statesmen who survey
The rich man's joy increase, the poor's decay,
'Tis yours to judge, how wide the limits stand
Between a splendid and an happy land.

—Oliver Goldsmith

The flight of trained manpower from the developing world, in search of greener pastures in the West, primarily North America, is a phenomenon that is nearly half a century old. The trickle of emigration from the poorer countries has grown into a flood. Many trained scientists and engineers leave in search of advanced degrees and postdoctoral training, only to remain as immigrants, lost to the countries of their origin. This flow of technically skilled manpower from the poorer countries of the 'South' to the richer states of the 'North', has traditionally been termed as the 'brain drain'. (Analysts of development have always used geographical descriptors in a manner designed to cause confusion.) Over the years, India has been a major contributor to the tides of emigration from the Third World, a term that seems to unhappily describe, a vast conglomeration of countries, a majority of whose citizens live under desperately poor conditions. The economic gulf between the First and Third Worlds appears unbridgeable; even more importantly, the islands of affluence in the latter are dramatically isolated from their surroundings.

As the number of migrants to the West from India has swelled, with the United States accounting for a figure of about a few million, greater awareness seems to have spread about the existence of a large community of Non-Resident Indians (NRIs), who are now spread across several continents. In relative terms, the numbers are small; about 20 million NRIs (a count that liberally includes many, who over generations have few connections in India), compared to the over 1 billion, who live within our borders. But, as the financial power of the NRIs has grown, even as the rupee sinks and the dollar rises, governments eager to attract dollar inflows have begun to pay greater heed to NRIs. From the desperate days of the early 1990s, when India's foreign exchange reserves teetered on the brink, the pendulum has swung the other way; the Reserve Bank of India now sits, apparently with some degree of discomfort according to some economic analysts, on a large reserve of 70 billion dollars. To this inflow NRIs, attracted by interest rates and promises of easy withdrawal, have contributed substantially. Emotional attachments must necessarily play a limited role in determining investment directions; economic considerations surely take precedence. But, there have been signs that a greater NRI involvement in Indian development has indeed been driven by a motivation to repay in some measure the institutions in this country, that have contributed to the success of NRIs in the West. Most notably, the Indian Institutes of Technology (IITs) provided the training grounds for many of the highly successful emigrants to the United States, beginning in the 1960s. The IITs have begun to receive over the last few years large endowments from grateful alumni. Interestingly, one of the most successful ventures to be spearheaded by IIT alumni, has been Infosys, a company that has demonstrated that Western markets can be exploited from an Indian base. However, most success stories are firmly rooted in the United States and it is rich, expatriate alumni, who have begun to renew their ties with the institutions that moulded them. In the developing euphoria over the growing private contributions to the IITs, we must not forget that in the fifty years of public investment in the IITs, these institutions have given more to the West, than they are likely to earn through occasional acts of philanthropy. The attitude of many successful alumni towards the institutions and country that shaped them, can sometimes be ambivalent. A proposal to effectively gain management control over the IITs, presumably to globalize technical education, using a proven, successful

platform, was floated sometime ago by a group of influential NRIs. The price tags mentioned in the press were of the order of 1 billion dollars (Rs 5000 crores). While the proposal seems to have run aground, there is little doubt that similar proposals will surface in the not-too-distant future. While disinvestments seems to be the favoured policy for the once powerful public sector, it may be inadvisable to consider a similar sellout by government in technical higher education.

The 'brain drain' appears to have suddenly acquired a new importance. January has seen two large gatherings addressed by the Prime Minister, where references to the flight of talent have been made. The Bangalore session of the Indian Science Congress was the first occasion; the 'Pravasi Bharatiya Divas' (literally the day of the NRI) was the second. While the issue of dual citizenship seemed to dominate the proceedings, the crowning irony appeared in an attempt to distinguish NRIs in the First World, from those who have settled elsewhere. To the government, the lure of the dollar seems understandably irresistible; although we may all feel more secure when our foreign exchange reserves are shored up by exports of Indian technology and products, rather than by remittances from emigrants. But, the Delhi gathering of many eminently successful NRIs (V. S. Naipaul's claim to this status must be tenuous, but is undoubtedly reinforced by the Nobel Prize) did prompt the Prime Minister, among others, to raise the question: 'Why do Indians seem to do so much better, elsewhere?' This is a question often raised in scientific circles. The average NRI scientist seems to perform better in the West. Transplantation to India, very often results in a noticeable decline in scientific productivity. One school of thought, and its influence is growing, argues that the best of NRI scientists are unwilling to return to the conditions that prevail here. Consequently, schemes to attract the expatriate Indian have surfaced periodically. The Swarnajayanthi scheme of the Department of Science and Technology has its origins in the desire to reverse the brain drain; its transformation to a mechanism for supporting 'resident talent' must only be attributed to the fact that the scheme did not originate with restrictive clauses. Undoubtedly, new initiatives to lure non-resident Indian scientists will be hatched by governments in the future, even as the financial and inevitably, political power of the NRIs grow.

In anticipating measures to reverse the brain drain, it is instructive to look at experiments being conducted in China. Faced with the exodus of a generation of young scientists, the Chinese government has put in place an elaborate system of opportunities and rewards to attract overseas Chinese scientists. Special professorships, attractive grants and laboratories and even 'independent institutes', with higher salaries and better facilities have been offered. The proposed National Institute of Biological Sciences in Beijing is expected to pay researchers at levels 'set about halfway between typical levels in the United States and China' (*Nature*, 2002, **420**, 257). Large laboratories, substantial grants and promises of administrative autonomy are the other carrots in the package being put together in China. The *Nature* report quotes an adviser to the project (non-resident Chinese, of course) as saying that 'the investigators will mostly be recruited from abroad', ostensibly 'to avoid accusations of poaching talent from elsewhere in China'. But, the initiatives in China have not been without critics. The special grants given to non-resident Chinese scientists to set up laboratories in China seem to have created a pool of 'absentee researchers'. These scientists have tapped into the scarce pool of research resources, but violate the requirement that they spend six to nine months in China, 'leaving largely unsupervised labs', while the grantees sojourn abroad (Cyranoski, D., *Nature*, 2003, **421**, 3).

The 'Pravasi Bharatiya Divas' is expected to be celebrated on 9 January every year. We will now be treated annually, at the beginning of the new year, to the spectacle of the science congress and the NRI celebration following one another closely. The compulsions of playing to the gallery will grow and new schemes to simultaneously promote both science and the NRI will surface. A large, passive and often indifferent, resident scientific community in India might only watch from the sidelines. Even as the government has driven a wedge between the First and Third World NRIs in the ambiguous offer of dual citizenship, we might worry that government might at some point succumb to the temptation of following the Chinese example, of creating First World science preserves, within our Third World ambience. NRIs would undoubtedly and understandably like to have the best of both worlds. Their participation in India's economic, scientific and technological development would bring to our institutions new dimensions of expertise and experience, which would be immensely beneficial. Unfortunately governments, politicians and their advisers in their euphoria about NRIs, may succumb to the temptation of forgetting their real constituency – the resident Indian.

P. Balaram

CURRENT SCIENCE

Volume 84 Number 3 10 February 2003

EDITORIAL

Measuring and Assessing Science

When the 'Science Citation Index' was first proposed its major objective was to break the so-called subject index barrier. Out of this bibliographic experiment has evolved a historiographic and sociometric tool of major importance. Like most other scientific discoveries, this tool can be used wisely or abused. It is now up to the scientific community to prevent abuse of the SCI by devoting the necessary attention to its proper and judicious exploitation.

Eugene Garfield
Nature, 1970, **227**, 669–671

Measuring and assessing the output of scientists and scientific institutions has never been an easy task. Garfield's path-breaking proposal for a 'citation index' was made at a time when few could have predicted the scope and magnitude of the revolution in computing and information technology. Today the *Science Citation Index* (*SCI*) and a host of specialized databases, which cater to specific disciplines, are readily available, online, to those who can afford them. In India, a number of national laboratories and central institutions are beginning to have access to the resources of the *SCI*; a development which will undoubtedly catalyse many analyses of the quantity and impact of Indian scientific output. This may, therefore, be an opportune moment to consider some issues which have a bearing on the assessment of science.

The journal *Scientometrics* first appeared a quarter of a century ago (Vol. 1, No. 1, September 1978, Elsevier Science Publishing Company, Amsterdam and Akadémiai Kiadó, Budapest). The 'launching of the journal . . . persuaded all those concerned that a self-contained research field under this name really exists' (Schubert, A., *Scientometrics*, 2002, **53**, 3–20). Today, scientometrics or the measurement of science, appears to be a well-developed field on the fringes of science, which may attract professional science watchers, policy makers, sociologists and historians of science. Practising scientists often view the results of scientometric analysis with some degree of self-consciousness and discomfort. The idea of a readily available, 'objective' tool, for assessment of the impact of one's publications, is hardly likely to meet with an enthusiastic reception from the average scientist. Inevitably, much of the published scientific work may appear mediocre. But as Garfield noted many years ago: '. . . the growth of science is dependent upon an accumulation of many "mediocre" results that are produced by hard work'. He went on to add: 'Long live the mediocrities. Without them how could there be geniuses?' (Garfield, E., *Current Contents* Nov. 4, 1970; *Essays of an Information Scientist*, ISI Press, Philadelphia, 1977, p. 131). The *SCI*, citation counts and journal impact factors are here to stay. The quantitative tools for measuring science published in the journals covered by the *SCI* are now widely available. Scientometrics may soon become a sub-field of science which may face greater scrutiny, as the validity of many quantitative analyses and their interpretations is questioned. This will be particularly true in countries of the Third World, India prominently among them, where the interpretations of scientometric analysis will have to be approached with caution. The *SCI* is a marvellous, indeed spectacularly useful tool, for unearthing bibliographic networks and tracking the development of a field. It may be a double-edged sword in the hands of administrators, who seek to use it to assess individuals and even institutions.

Recently, this journal carried the results of a simple analysis; a comparison of the total number of *SCI* indexed papers, which emanated from India over the period 1980–2000, with that from countries like South Korea, Brazil and Israel. The observation was that while India's publication output has held steady over a two-decade span, the other countries appeared to show an appreciable upward trend. The conclusion reached was that India's publication output, and by inference its science, has stagnated; undoubtedly, a worrisome trend (Arunachalam, S., *Curr. Sci.*, 2002, **83**, 107–108). The reactions to this finding have varied widely. To some, this quantitative and limited analysis only confirms their worst fears; science in India is indeed on the decline. To others, optimists, who view science from the perspective of specialized fields, the quality of publications in some highly visible areas has shown a dramatic improvement; they dismiss 'total counts' as misleading. The measurement of 'quality'

is, of course, more difficult; journal impact factors are arguably, an imperfect measure; citation counts are more difficult (and expensive) and, in the eyes of many, a far from ideal yardstick to measure the 'quality' of the average scientific output that results from research in the developing world. Even reasonably good and useful pieces of work from the Third World are cited less than work of a similar calibre appearing from Western laboratories. Citations and co-citations which bind together an invisible, collegial network of scholarship, can in the modern world be driven by many sociological factors, which, sometimes, have little to do with the scientific quality and utility of a published paper. While editors of major journals in the world have steadfastly maintained over the years that there is little evidence for bias against Third World manuscripts at the level of editorial review, there are no easy methods to assess whether there is a 'citation bias', that tilts against authors from the developing world. Despite the many questions that may be raised about the validity of the conclusions drawn by counts of papers and citations using the *SCI* database, it appears that this activity is now on the increase in India. Even more importantly, governmental agencies and administrators seem to be attracted by the possibility of using scientometric analysis as a tool in assessing scientific performance; often unable to resist the temptation of using that most dubious index, the 'average journal impact factor' (a quantity easily obtained by averaging the current journal impact factors on a list of publications) to compare individuals and institutions.

The use of the *SCI* and scientometric analysis in India is hampered by the fact that there are very few analysts, who understand the nuances and limitations of the databases that they use. Even fewer have any appreciation for the fields whose publications output they analyse in quantitative terms. Most disturbingly, many analysts do not bring the methods of science to their practice of scientometrics; they do not check and recheck their search results, they do not do 'control searches' where the answers are already reasonably well known. The 'clients' for whom this kind of analysis is performed, invariably influential government science administrators, appear touchingly innocent of the meaning of a journal impact factor. For them, impact factors irresistibly lead to absurd conclusions; an institution devoted to biology is stated to be 'doing better' than an institution in the area of geophysics – the conclusion being drawn using the ubiquitous 'average journal impact factor'. Many analysts are even unaware of the nature of the journal coverage in the databases they use; convenience and expedience dictate the choice of databases and any appreciation of the dangers of analysing interdisciplinary areas using restricted journal sets is largely lacking. The appearance of citation searching in *Chemical Abstracts* using *SciFinder Scholar* as an alternative to the *SCI* (now incorporated in the beguilingly named *Web of Science*) adds a new dimension to analyses in chemical sciences. Differences in journal coverage can lead to differences in citation counts as pointed out in a recent model study (Whitley, K. M., *J. Am. Soc. Inf. Sci. Technol.*, 2002, **53**, 1210–1215). This study notes that 'academic administrations find it difficult to ignore the tidy citation figures as an achievement measure'.

The citation game and the benefits of large publication lists in career advancement have led to many curious situations. The development of an 'electronic systematic search tool' permitted the estimation of the 'amount of duplicate publications in the 70 ophthalmology journals listed by *Medline*'. The results are striking. Out of 22,433 articles analysed, 13,967 gave a score in a duplicate-detection algorithm, which appeared significant. Manual review of 2210 papers yielded 60 'genuinely duplicate' publications. The authors regard this as the 'tip of an iceberg' (Mojon-Azzi, S. M., Jiang, X., Wanger, U. and Mojon, D. S., *Nature*, 2003, **421**, 209). While duplicate publication may be relatively uncommon, the tendency to fragment publications will undoubtedly increase, as 'total counts' become an easy measure of achievement. Another recent report notes that most scientists who quote a paper are unlikely to have read it; a conclusion hardly likely to surprise practising scientists. The sociological factors that go into the compilation of reference lists in a published paper can hardly be gleaned from the *SCI*. Using mis-citations of a highly regarded paper in condensed matter physics, M. V. Simpkin and V. P. Roychowdhury, conclude that propagation of citation errors suggests that more than three-quarters of authors have not read the cited papers (see report by Ball, P., *Nature*, 2003, **420**, 594). This estimate is undoubtedly inflated; there are many other innocuous explanations for error propagation. For example, an author may read a paper that he cites but use an incorrect reference copied from a previously published list, during manuscript preparation. This could hardly be classified as deviant behaviour; it is merely a casual act of sloppiness. Nevertheless, such studies raise interesting questions about citation practices.

Journals, papers, impact factors and citations might provide a measure of the health of a nation's academic science. The growing interest in the analysis of quantitative science indicators in India is clearly a good sign; inferences based on hard data are preferable to those based on personal prejudice. But, it may be necessary to critically evaluate the approaches used in analysing science in India and to thoughtfully integrate scientometric analysis into the shaping of public policy. An uncritical plunge into the sea of scientometrics may be unwise.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Golden Helix

We believe a gene – or perhaps the whole chromosome fibre to be an aperiodic solid.

—E. Schrödinger
What is Life? (1944)
Cambridge University Press
Canto Edition 1992, p. 61.

If, however, the biologically active substance isolated in highly purified form as the sodium salt of desoxyribonucleic acid actually proves to be the transforming principle, as the available evidence strongly suggests, then nucleic acids of this type must be regarded not merely as structurally important but as functionally active in determining the biochemical activities and specific characteristics of pneumococcal cells.

—O. T. Avery, C. M. MacLeod and M. McCarty
J. Exp. Med., 1944, **79**, 137.

We wish to suggest a structure for the salt of deoxyribose nucleic acid (D.N.A.). This structure has novel features which are of considerable biological interest. ... It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material.

—J. D. Watson and F. H. C. Crick
Nature, 1953, **171**, 737.

The most novel features of DNA are associated with its duplicity, rather than its helicity. Linear polymers rarely form stiff straight rods; folding into coils is the norm. The genetic functions of DNA are inextricably associated with its duplex structure, and hardly at all with its helical shape; this is reflected in the preoccupation of DNA research with its role as an informational molecule.

—J. Lederberg
J. Am. Med. Assoc., 1993, **269**, 1981.

Caught in the midst of a traffic jam on a Bangalore road, my attention was drawn to a bus with garishly painted symbols on its side. Curiously, these symbols seemed strangely familiar; and indeed they were. Decorating the side of this otherwise unprepossessing bus, were the two symbols that are representative of chemistry and biology; the familiar hexagonal image of Kekulé's famous structure of benzene and the intertwined double helix of DNA, proposed by Watson and Crick. Indeed, the side of the bus was decorated with a most faithful replica of the single schematic figure that appears in the 1953 paper in *Nature*, which for many marks the first shot of the modern biological revolution. Even as I sat in the swirling gusts of automobile exhaust fumes, the cacophony of horns making reflection almost impossible, I could not help thinking of the late 1960s, when I first heard of DNA. As a masters student studying chemistry, at arguably one of India's most modern institutions, IIT Kanpur, I had never heard of DNA, until I chanced upon an issue of the *Atlantic Monthly* in the summer of 1968. The magazine serialized a version of J. D. Watson's 'The Double Helix'. The molecule stayed in the background; Watson's compellingly readable account of the race for a structure, which would inevitably lead to scientific immortality and the Nobel prize proved irresistible. Later that year, the Nobel prize for medicine was awarded to Hargobind Khorana for his remarkable synthesis of polynucleotides, which was critical in deciphering the genetic code; an award he shared with Marshall Nirenberg and Robert Holley. Khorana's Indian origins immediately focused local attention on DNA, a molecule which until then had remained divorced from chemistry, at least in my surroundings. But, in 1968, a year dominated by worldwide student unrest, the quickening pace of the Vietnam War and all-round political uncertainty, few could have foreseen the impending biological revolution, which would make DNA the best known molecule of our times.

The DNA story has its modest beginnings in 1869, when Friedrich Miescher isolated 'nuclein', from the nuclei of pus cells. Nearly 60 years were to pass before Frederick Griffith described the 'transformation' of *Pneumococcus*; a living organism's characteristics could be predictably and specifically modified by a 'transforming principle'. Genetics was already a well established science, with the 'gene' as the basis of heredity. Erwin Schrödinger famously described the gene as an 'aperiodic, solid' in his lectures entitled *What is Life?*, at Trinity College, Dublin in 1943 (later published in 1944).

At the same time there appeared a conservatively titled paper 'Studies on the chemical nature of the substance inducing transformation of pneumonococcal types', in the *Journal of Experimental Medicine* (1 February 1944; **79**, 137). The authors were Oswald Avery, bacteriologist and professor at the Rockefeller Institute of Medical Research (now the Rockefeller University) in New York, and his collaborators Colin MacLeod and Maclyn McCarty. Their paper, received by the journal on 1 November 1943, marks the beginning of the transformation of biology. The conclusions seem clear, at least with the advantage of hindsight: 'The evidence presented supports the belief that a nucleic acid of the desoxyribose type is the fundamental unit of the transforming principle of *Pneumococcus* type III'. Put simply, genes were made up of DNA; a conclusion confirmed in the bacterial virus experiments of Hershey and Chase in 1952.

Avery's work invested DNA with remarkable importance; it was not merely another biopolymer playing a structural role. DNA was by now, clearly, the unit of inheritance. The Watson–Crick paper of 1953 contains only 6 references and Avery's paper is not among them. Indeed, the famous 25 April 1953 issue of *Nature* also carries the separate papers by M. H. F. Wilkins, A. R. Stokes and H. R. Wilson and by Rosalind Franklin and R. G. Gosling, none of which refer to any connection between the structure of DNA and its biological relevance (See *Nature*, 23 January 2003 for an extended historical assessment). Writing fifty years after the event, Maclyn McCarty, the last survivor of Avery's group, says of the Watson–Crick paper: 'I certainly grasped the significance of their findings and was pleased to see such illuminating results come from a structural approach. I was not so pleased, however, that they failed to cite our work as one reason for pursuing the structure of DNA' (McCarty, M., *Nature*, 2003, **421**, 406). In the enormous glamour of the story of the DNA structure determination, the origins of DNA and the identification of its role in genetics by Avery have been largely forgotten, except possibly amongst biologists with a sense of history. Gunther Stent, one of molecular biology's foremost chroniclers, has classified Avery's discovery as 'premature'; defining a premature discovery as one whose 'implications cannot be connected by a series of simple logical steps to canonical, or generally accepted knowledge' (Stent, G. S., *Sci. Am.*, 1972, **227**, 84–93). The connection between the Watson–Crick double helix and inheritance was, of course, almost immediately obvious. But, there are other views. Erwin Chargaff, who is quoted in the Watson–Crick paper for his finding 'that the ratio of the amounts of adenine to thymine and the ratio of guanine to cytosine are always close to unity for deoxyribose nucleic acid', has this to say: '... If I, a simple chemist only distantly interested in the mechanisms of heredity was so deeply moved by the sudden appearance of a giant bridge between chemistry and genetics, the practitioners of the latter science would have been alerted even more forcefully'. Avery died in 1955, eleven years after his discovery; his omission from the roll call of Nobel laureates continues to draw the attention of historians of science.

Fifty years after the birth of the double helix, it is time for anniversary celebrations. But, as the historian Robert Olby asks: 'Why celebrate this one discovery'. His answer: 'There is a centrality about DNA that relates to the centrality of heredity in general biology' (*Nature*, 2003, **421**, 402). Francis Crick emphasizes this view clearly when he says: 'I think what needs to be emphasized about the discovery of the double helix is that the path to it was, scientifically speaking, quite commonplace. What was important was not the way it was discovered but the object discovered – the structure of DNA itself'. Crick compares the race for the collagen structure, a triple helix first proposed by G. N. Ramachandran at Madras, which was determined in the years immediately following the DNA triumph: 'Its discovery had all the elements that surrounded the discovery of the double helix. The characters were just as colourful and diverse. The facts were just as confused and false solutions just as misleading. ...Yet nobody has written even one book about the race for the triple helix. This is surely because, in a very real sense, collagen is not as important a molecule as DNA' (*What Mad Pursuit*, Basic Books, Inc. New York, 1988, p. 67). Interestingly, in his book, Crick refers to Avery's work as establishing the importance of DNA in carrying genetic information.

The progress of biology, in the half century since the arrival of the double helix, has been spectacular. Even school children have heard of genes and sometimes, even genomes and genomics. DNA is an abbreviation that never needs to be expanded. The remarkable successes of the many technologies that allow the manipulation of DNA suggest that another biological revolution may begin even before the dust has settled on the ferment that began in 1953. Anniversaries can be a time for some reflection. Both the past and the future merit attention. But, it is clear that whatever course biology takes in the years ahead, the double helix will remain one of the most enduring icons of the 20th century.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Indian Institutes of Technology

'Here in the place of that Hijli Detention Camp stands this fine monument of India, I.I.T., today representing India's urges, India's future in the making.'

Jawaharlal Nehru at IIT Kharagpur
21 April 1956.

The Hijli Detention Camp in Midnapore district of Bengal was the place where two unarmed detainees, Tarakeshwar Sengupta and Santosh Mitra, were killed by the police on 16 September 1931. Their bodies were collected by Nethaji Subhas Chandra Bose, in one of the many poignant episodes of India's long and protracted struggle for freedom. In 1946, on the eve of independence, the Nalini Ranjan Sarkar Committee pondered on the issue of setting up 'higher technical institutions for the postwar industrial development' of India. The Sarkar Committee had a vision of four national institutions, geographically dispersed across the country, modelled on the lines of the best technical institutions in the West. In considering the standards to be maintained for undergraduate degrees, the Committee chose Manchester and the Massachusetts Institute of Technology (MIT) as examples; the choice of the former may appear curious in today's context, while few may take exception to the latter as an academic model. The IITs arose out of the recommendations of the Sarkar Committee, with the institution at Kharagpur coming into existence in May 1950. In September 1950, the IIT shifted to its present campus, the site of the Hijli Detention Camp. Almost exactly two decades after the shooting at Hijli, IIT Kharagpur was formally inaugurated on 18 August 1951. The IIT system expanded rapidly in the late 1950s and early 1960s. The institutes at Bombay (1958), Madras (1959), Kanpur (1963) and Delhi (1963) were started with foreign assistance in the early days, providing each with a distinct character, reflecting the academic approaches of the collaborating countries. In time, the IITs grew to develop a remarkable character of their own, transforming the landscape of engineering education in India. The success of the IIT experiment has encouraged the setting up of two new IITs; Guwahati, an institution being developed from scratch and Roorkee, where an existing university has been transformed by decree: an admirable act of administrative alchemy.

IIT Kharagpur celebrated its 'golden jubilee' in 2001 to mark the passage of half a century since its inception. But, in a commendable display of togetherness, IIT alumni used the Kharagpur milestone to celebrate the success of the IITs at a gathering in Silicon Valley in northern California. Ironically, the first celebration of the success of the IIT experiment was in faraway California, where many of its most successful alumni reside. Bill Gates placed his seal of approval on the IIT's academic standing and the CBS television network devoted its 60 Minutes program to featuring the IITs. For those who may be uneasy with this celebration of the success of India's foremost technical institutions, some solace may be derived from the fact that IIT alumni organized an IIT 50 event in Bangalore on 16 February 2003. A-well attended seminar on the 'Role of IITs in Nation Building' marked the occasion, with the discussion initiated by alumni representing the various sections of society into which IIT students had integrated; notably, industry, politics and academia. There was little said about alumni who had moved into areas of wide societal concern like the environmental movement in which the late Anil Agarwal played a stellar role. As an alumnus, albeit an atypical one, with a degree in science, I had the opportunity to participate; although I suspect that many of my perceptions on what the IITs have been and what they ought to be are distorted by the prism of my own long experience in the sheltered and tranquil atmosphere of the Indian Institute of Science.

The global image of the IITs quite clearly derives from the spectacular success of its alumni, who obtained undergraduate (B. Tech.) degrees in various disciplines of engineering. It is these men and some women, who have created technologies, opportunities and wealth, making the IITs synonymous with excellence in technical education. The selection process for undergraduates, through the rigorous Joint Entrance Examination (JEE), is so competitive that only about 2% of the applicants are eventually successful in entering the IITs. With the IITs becoming the institutions of choice for all aspiring engineers, thousands of students flock to preparatory coaching classes for a year or two before venturing to appear for the JEE. This method of selection ensures a certain academic homogeneity of IIT entrants, which may not be

an entirely desirable attribute in an institution of higher learning. Picked for their analytical and quantitative abilities, IIT undergraduates have proved extraordinarily successful when they have moved to fields of management, finance, marketing and business administration. Over the years, the IIT undergraduate programs have been among the major providers of highly trained technical manpower, to the advanced countries of the West. Unsurprisingly, the IIT jubilee is more celebrated in San Jose than it is in India. IIT-trained engineers have not only migrated geographically in large numbers; they have also moved away from the technical disciplines in which they were trained, in even larger numbers. In addition to the flight to business, engineers of all hues have gravitated towards 'information technology'. This 'disciplinary migration' may be of some import in the years to come.

The IITs were born at a time when the Nehruvian vision clearly held that science and technology would dominate the transformation of India. This has indeed happened; but the gains of technological advance have been neutralized by a booming population growth, with an attendant increase in economic disparities. The IITs have flourished and their alumni have been among the leaders in the transformation of at least some sectors of our economy. The IITs have established themselves as the jewels in the crown of technical education in India; they command enormous worldwide respect for the quality of their undergraduates. But, this may be an appropriate moment to reflect on the academic ethos of the IITs; to raise questions on the role of research in these institutions and to reflect on the growing gulf between science and engineering.

The great universities of the West, both in the past and in the present, have been all encompassing in the subjects they teach and research. Even the 'Institutes of Technology', Caltech and MIT amongst them, which formed the model for the IITs, have a broad mandate extending well beyond undergraduate engineering education. It is the breadth and depth of research, which is their defining characteristic. We might then ask: 'where do the IITs fit in the academic scenario in India?'. In addressing this question three models for higher education in an ambience which promotes research, may be considered. First, these are our universities, Banaras, Allahabad, Calcutta, Madras and Delhi among them, which in the early years after independence produced much of the academic output of this country. Secondly, we have predominantly post-graduate research institutions, IISc being preeminent. Finally, there are the IITs. I deliberately omit a growing number of specialized national research laboratories, which have donned the mantle of deemed universities. The three classes of institutions that I list have unique characteristics of their own. But, their status today must be a matter of national concern. The universities have decayed; engineering, science and the humanities have fragmented. The universities which I explicitly list can look back on an historic past, when this country's intellectual resources emerged from their classrooms. They were centres of teaching and research. We must not forget the remarkable engineering departments that grew within our university system; metallurgy in Banaras and chemical engineering at the University Department of Chemical Technology in Mumbai are two examples, which immediately spring to mind. The universities are in a difficult situation today; a matter of concern for the hundreds of thousands of students, who cannot enter the IITs or who wish to pursue disciplines other than engineering.

This may not be the moment to dwell on another model, the Indian Institute of Science (IISc), which is slowly approaching its centenary year. Here engineering and research coexist in an ambience which promotes research. The focus on postgraduate and doctoral degrees differentiates IISc from the IITs, although at some IITs research occupied a more honoured position in the early days of their existence. The jubilee celebration may be an opportune moment for the IITs to reiterate their commitment to research. Thus far the IITs have emphasized quality undergraduate education in engineering. Their success has been predicated on two factors: rigorous selection of students and a proven recipe for training. It is however a matter of some concern that even as the 'brand equity' of the IITs, a term undoubtedly coined in marketing circles, has risen, the country's scientific research output appears to be levelling. And, here I use the word 'science' in its broadest context, encompassing all areas of science and engineering.

Anniversaries and jubilees are useful. They allow us to look back and, sometimes, even prompt us to look ahead. For the IITs this may be a good time to consider emphasizing postgraduate education and research and to lower the rising barriers between science and engineering. There are some welcome signs of change. Biology, long excluded from some IIT campuses has begun to take root. Many of tomorrow's technologies will be seeded by basic research in biology and it is fitting that the subject is introduced at our best technical institutions. The real drivers into the future will be the men and women, who serve on the faculty of these institutions and chart their course. Recruitment policies and programs for promoting research will be critical in these new areas. The growing interest of alumni in the institutions which nurtured them is a heartening sign. The IITs have produced many leaders of business, technology, commerce and government. If some of them were to reflect on the importance of shoring up our academic institutions and chart a course of action, we might begin to see the contours of a renaissance emerge; an academic awakening on a broad front, which would indeed address the theme which the Bangalore meeting wished to consider – 'the role of IITs in nation building'.

P. Balaram

CURRENT SCIENCE

Volume 84 Number 6 25 March 2003

EDITORIAL

Ph Ds: A need for quality control

The Ph D is a passport valid for immigration into almost any academic institution in the world.

—P. B. Medawar
Advice to a Young Scientist
Harper and Row, 1979

The Ph D or Philosophiae Doctor is a traditional credential attribute of an individual awarded by an institute of higher education after the successful defense of a dissertation, recording the candidate's independent and original contributions to knowledge.

—Pjotr Hesseling
Frontiers of Learning
Foris Publications, Dordrecht, 1986, p. 4.

The number of universities and 'deemed' universities in India has almost doubled in the period 1980–2000. Today the number of institutions of higher education, which award doctoral degrees, probably exceeds 250. Statistics for the year 1998–1999 reveal that as many as 10,951 Ph D degrees were awarded by our institutions. Of these, 5608 were in the various disciplines of science and technology, including engineering, medicine, agriculture and veterinary science. A little over a third of the Ph Ds were in the pure sciences, 3836 to be precise (Research and Development Statistics, Department of Science and Technology, May 2002). Although definitive statistics are not available as yet for 2002, it is clear that the numbers must have risen. The number of Ph Ds produced might be useful as an indicator of the growth of the science and technology sector. However, we might well ask: 'Are we producing Ph Ds of an acceptable quality or have falling academic standards permeated the highest levels of our university system'.

The Ph D system developed in the second half of the 19th century; the German research universities clearly dominating the period until the run up to the Second World War. The American graduate school system exploded in the 1950s and 1960s. While the first American Ph D was awarded by Yale in 1861, the number had grown to about 3600 by 1941. Thirty years later, in 1971, the number had risen to a staggering 32,000 doctorate degrees. This coincided with the phenomenal growth period of American science. Two distinct models for Ph D programs emerged in the West: the German–English model, where the doctoral period demanded a complete immersion in research towards a thesis and the American model, where a broad-based program of courses prepared a student for the period of research towards a Ph D degree. Today, the former model is associated with greater specialization and shorter time periods for obtaining a Ph D degree, while the latter promises a more wide-ranging period of training but requires a significantly greater length of time, for completing all the requirements for a Ph D. In India, the academic models vary with institutions; relatively few require any pre-Ph D training program. But, as the number of Ph D degrees awarded by diverse institutions increases it may be necessary to reflect on the quality of our Ph D programs and the doctoral theses that are produced.

An aspiring scientist learns the tools of the trade, during the period of a Ph D program, generally serving as an apprentice to a master. Traditionally, Medawar's 'passport' must be earned after a long period of apprenticeship, which provides an entry to the world of scholarship. But today's reality is different. The Ph D is yet another academic degree, that can often be obtained with little effort by many, who have almost no pretence to scholarship. In large measure, the Ph D is the result of a collaboration between the academic supervisor or 'guide' and the student. Invariably, it is the supervisor who decides when the thesis can be submitted; the arbiter of the quantum of original work that merits award of a doctoral degree. In most of our institutions the academic faculty of a department have an extremely limited collective role in monitoring a student's progress and play no part in any formal process that assures that doctoral theses conform to a minimum standard. In many institutions, there are no qualifying hurdles that an aspiring Ph D candidate must cross. There are no checks that ensure that students have at least a minimal knowledge of the fields in which they propose to specialize. The thesis examination system in

some institutions does not pose a significant academic hurdle; friendly examiners can be found for almost any dissertation. It is therefore unsurprising to find that a large fraction of doctoral theses do not result in any significant research publications, in journals of consequence. Indeed, as the number of doctoral awards in science has increased, the number of papers from India in *SCI* indexed journals has remained stagnant.

In many universities there are no procedures for the admission of registered Ph D scholars; only those who wish to obtain financial support in the form of scholarships pass the national level examinations. There are few limitations on the number of students who can enroll under a single guide; often a complete mockery of the supervisor–student relationship results, with the former having little or no idea of the student's academic activities. Unfortunately academics are often judged by the number of Ph Ds they have produced, resulting in a tendency to lend one's name as a guide, with little discrimination.

Another factor that has led to deteriorating Ph D quality is the requirement that college teachers and technical staff must acquire degrees in order to qualify for promotions. As a result, in many universities there have been a flood of Ph D registrants, whose sole interest is to acquire an academic qualification for self advancement. Several universities also act as agencies which provide Ph D registration for candidates working in national laboratories. Students working in these laboratories have almost no contact with the degree-awarding institutions and often work in an environment, which is rarely stimulating, in an academic sense. To compound this problem, the University Grants Commission has recently been on a spree for creating deemed universities. Many of these institutions are unlikely to set up internal mechanisms which focus on the quality of graduate training as part of a doctoral program. In these laboratories individual supervisors are likely to be the sole judges of academic quality, with no collective institutional academic mechanism in place.

The quality of doctoral theses produced in our institutions varies widely. Those submitted in the best of our institutions match the theses produced at the best universities in the world; however, a very large number of dissertations do not conform to an acceptable standard. The inadequate level of academic training imparted is evident, when these students appear before selection committees for national level research associateships. Very often the candidates are extremely capable individuals, who have received little or no relevant training in institutions with lamentably low academic standards. A common refrain that is heard is that many of our universities lack an adequate research infrastructure. But, in most places what is really lacking is an academic environment, where scholarship and intellectual pursuits are actively encouraged. Examinations, seminars and discussions of science require little by way of resource. Even as we create more universities, deemed and otherwise, it might be worthwhile to consider raising the bar for the award of Ph D degrees. Quality control is as necessary in academia as it is in industry.

P. Balaram

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EDITORIAL

Science, Technology and War

'Genghis Khan was a nomad and the inventor of a powerful war machine – and that conjunction says something important about the origins of war in human history. Of course it is tempting to close one's eyes to history, and instead speculate about the roots of war in some possible animal instinct: as if, like the tiger, we still had to kill to live, or, like the robin redbreast, to defend a nesting territory. But war, organized war, is not a human instinct. It is a highly planned and cooperative act of theft.'

J. Bronowski
The Ascent of Man
British Broadcasting Corporation
1973, p. 64

Jacob Bronowski in his television series 'The Ascent of Man' swept majestically across the centuries as he traced the evolution of science, culture and civilization in the Western world. For Bronowski, 'the largest single step in the ascent of man is the change from nomad to village agriculture'. Civilization grew on the banks of many great rivers and in keeping with his intellectual and religious roots, Bronowski followed the Old Testament and described the rise of agriculture in the Fertile Crescent of the Middle East. His description of Jericho as the 'key that locked' the way of the tribes of Israel 'down towards the Tigris and Euphrates' evokes an image of conflicts for fertile lands. Bronowski estimates that by 6000 BC Jericho was an agricultural settlement and a 'microcosm of history'. Several millennia later the banks of the Tigris and Euphrates are scenes of a war, that may at some future date be marked as a turning point in the history of the West. The war in Iraq, one-sided, pointless and at times, mindlessly brutal, is just over two weeks old. The war's end will be hastened, at least formally, by the enormous technological superiority of the American and British forces, aided by minor participants, who together constitute a 'coalition', which has contemptuously disregarded world opinion, in overrunning a sovereign country. We do not have to look far to seek the causes for this amazing display of arrogance; it is the enormous scientific, technological and economic superiority of the United States, which permits it to act largely unilaterally, in the face of world opinion.

The stated purpose of the war in Iraq has changed gradually over the past few weeks. It began with the perceived threat posed by Saddam Hussein and his 'weapons of mass destruction (WMD)', particularly in the light of the alleged links of the regime in Baghdad with terrorist organizations. But gradually, the focus has changed; liberation of the Iraqi people from a brutal dictator has been trumpeted as the major objective; the war itself being christened as 'Operation Iraqi Freedom'. And, as Iraq is inexorably overrun by vastly superior armies, its enormously productive oilfields have become 'assets' to be secured for the 'future reconstruction' of the country, under American control. The 'weapons of mass destruction' (the definition of 'mass' appears unclear) are presumably, nuclear, chemical or biological weapons. In the prelude to the war, much was made of the use of chemical weapons on earlier occasions by the Iraqis, to quell a Kurdish rebellion and in the Iran–Iraq war. There is, of course, no clear evidence for the use of biological weapons. Nuclear weapons have never been employed in human conflict, after their use by the United States over Hiroshima and Nagasaki, in August 1945.

It is instructive to consider the pattern of usage and development of chemical and biological weapons. Despite the early use of chemical agents in World War I and the systematic use of hydrogen cyanide in German concentration camps during World War II, the widespread use of incendiaries and chemical defoliants reached its peak during the Vietnam War, with the United States resorting to napalm bombs and the spraying of herbicides like Agent Orange (dioxins), with little regard for human toxicity. Recent reports suggest that the US military is contemplating the use of 'chemical calming agents' on the streets of Iraq, in post-war crowd control situations. Clearly, the dangers of such agents on innocent bystanders is emphasized by the casualties sustained amongst hostages during the Moscow theater siege in October 2002 (Knight, J., *Nature*, 2003, **422**, 363). What kind of chemical agents are the Iraqis supposed to be ready to deploy? Many reports suggest that the chemical weapon of choice may be VX, a convenient pseudonym for a molecule that goes by the tongue-twisting chemical name of O-ethyl-S-(2-isopropyl-aminoethyl) methylphosphonothiolate. VX was developed by a British chemist in the early 1950s during a search for new pesticides. VX was found to be too toxic to use and was then the subject of further development by the US military. By the early 1960s full-scale production was possible. Understanding

of its mechanism of action, permitted the development of antidotes as 'auto-injector kits' for the US Army. VX acts as an inhibitor of neural transmission, causes rapid death and is reported to be more effective than its competitor, sarin. Interestingly, the Americans accuse Saddam Hussein of using VX, during the 1988 Halabja massacre, although the element of righteous denunciation appears to have surfaced only long after the event.

Biological weapons do not appear to have been used, thus far, although the 'weaponization' of the anthrax and botulinium toxins was completed by the United States and the erstwhile Soviet Union during the Cold War. The wave of 'terrorist anthrax mailings' in the aftermath of the World Trade Center attacks and the invasion of Afghanistan appear to have been traced to points within the United States. In addition to chemical and biological weapons, Iraq has also been accused of possessing nuclear weapons. It is the search for these 'weapons of mass destruction' that has ostensibly led to the present conflict. Do the Iraqis have the scientific and technological infrastructure to sustain a viable program of weapons development, during a decade-long period of international sanctions and intense surveillance? The view of the former British foreign secretary and leader of the House of Commons, Robin Cook provides a possible answer: 'Iraq probably has no weapons of mass destruction in the commonly understood sense of the term – namely a credible device capable of being delivered against a strategic city target. It probably still has biological toxins and battlefield chemical munitions, but it has had them since the 1980s, when US companies sold Saddam anthrax agents and the then British Government approved chemical and munitions factories. Why is it now so urgent that we should take military action to disarm a military capacity that has been there for 20 years, and which we helped to create?'

In the run-up to the war in Iraq, the worldwide community of scientists, dominated overwhelmingly by the countries of the 'coalition', has been largely silent. One group of US Nobel laureates did address the American president, calling the unilateral move on Iraq as a step that 'would undermine, not protect, US security and standing in the world'. Within the US the powerful scientific community has been cautious in its public statements on the war. Curiously, the flagships of American science have already begun to look for opportunities after the death and destruction. As Donald Kennedy notes in an editorial: 'Finally, there is the task of reconstruction. France and Russia have already sent unfortunate signals that they won't enlist for the peace either. Perhaps others will – but whatever nations and agencies join in, the work will need the full arsenal of science and technology: to repair infrastructure, to deliver aid to civilians, to protect the public health, and to start rebuilding a national economy'. (*Science*, 2003, **299**, 1945). The irony may not have been lost on Kennedy. The full arsenal of American science and technology, laser and satellite-guided smart bombs, cluster bombs and the most powerful conventional explosives available would have been used to decimate Iraqi cities. The battlefield would have also provided experimental tests for the frightening range of military technologies that US research has successfully produced. Both the destruction and reconstruction of a perceived enemy fuel the science, technology and economic enterprise of the United States. Modern war is increasingly an exercise in using technological tools to inflict maximum destruction. Every attempt to control the 'weapons of mass destruction' has found the United States adopt the self-righteous position that all other countries must eschew development of the very same technologies, that it has found so effective in the conduct of international relations. Indeed, the US position on all international treaties, including non-military agreements like the Kyoto Protocol on Climate Change or the Biological Diversity Convention has not been in step with international opinion. An editorial comment in *Nature* (2003, **422**, 359) takes a cautiously optimistic view on the post-Iraq world: 'Instead, we must hope that an international consensus can be rebuilt, if falteringly at first. Scientific organizations have not had much to contribute to the debate running up to this war. But in its aftermath they should speak up loud and clear to press for internationalism in a world that could otherwise veer towards factionalism and further conflict.'

The response of governments across the Third World, and Iraq is indeed a constituent, has been muted. The increasing power of economic and trade sanctions in a globalized economy and the stranglehold that the countries of the coalition and their supporters exercise on international monetary organizations compels silence. The one inevitability of this war is that Baghdad will fall, as Kabul before it. But, students of history may recall another capital and another age. Almost exactly 28 years ago, in the dying days of April 1975, the last Americans were evacuated from the rooftops of their embassy in Saigon, which was almost immediately renamed as Ho Chi Minh city. In that war too, for almost a decade the B52s rained bombs on Vietnam, in an unassailable demonstration of superior military technology. In the end, 'victory' was still elusive.

The 'war on terror' proclaimed in the aftermath of 11 September 2001 is slowly being transformed into a well-coordinated exercise of 'theft' in Bronowski's definition. But, in these turbulent times it may be opportune to reflect on Bronowski's musings: 'We are a scientific civilization; that means, a civilization in which knowledge and its integrity are crucial.' Addressing himself to an undoubtedly Western audience he added: 'Science is only a Latin word for knowledge. If we do not take the next step in the ascent of man, it will be taken by people elsewhere, in Africa, in China. . . . Humanity has a right to change its colour.' Bronowski's conclusions after his broad sweep through the ages may strike a chord: 'We have not been given any guarantee that Assyria and Egypt and Rome were not given. We are waiting to be somebody's past too, and not necessarily that of our future.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Selling science

'I can't believe that! said Alice.
'Can't you?' the Queen said in a pitying tone. 'Try again: draw a long breath and shut your eyes.' Alice laughed. 'There's no use trying' she said. 'One can't believe impossible things.'
'I daresay you haven't had much practice' said the Queen. 'When I was your age, I always did it for half-an-hour a day. Why, sometimes I've believed as many as six impossible things before breakfast'.

—Lewis Carroll
Through the Looking Glass
Chapter V

Scientists spend a great deal of their time trying to convince other scientists of the importance of their work. In the practice of contemporary science, peer acceptance and peer recognition are almost universally sought by scientists. Journals, bulletin boards on the internet, conferences, symposia and seminars form an extended marketplace; an extraordinarily rich and diverse exhibition ground, where scientists market their wares. Can science be sold? Is science a commodity, whose dissemination and acceptability are enhanced by the clever application of marketing strategies? In the fiercely competitive modern world, do the techniques of packaging and advertising decide the acceptability of the results of scholarly scientific research? In thinking about the techniques of marketing, that might be of value in today's science, my attention was drawn to a paper intriguingly titled, 'Is Science Marketing?' (Peter, J. P. and Olson, J. C., *Journal of Marketing*, 1983, pp. 111–125; http://pages.stern.nyu.edu). The authors begin with the problem that analysts of marketing have always wrestled with: 'Is marketing a science?' Their comments on the studies aimed at addressing this question are sharp: 'Although well intentioned, we believe the debate regarding whether or not marketing is a science has been largely unproductive. For the most part, we attribute the current confusion to the somewhat naive conceptions of science that have informed the controversy. In particular, we consider the typical beliefs about how scientists do scientific work and how scientific progress is achieved to be inconsistent with current views about such issues in the disciplines of philosophy, sociology, and history of science'. Peter and Olson then turn the tables on the debate on marketing as a science to ask instead, 'is science marketing?'.

In their long and at times, mildly provocative analysis, they touch upon the many techniques of marketing science, which scientists adopt almost instinctively. They note that: 'in addition to direct advertising via publication in journals, books, and proceedings, a theory can also be promoted through publicity and personal selling'. (The authors use 'theory' in a general sense to signify a scientific finding.) In discussing the key attributes of a 'theory' that impact on its marketing success, Peter and Olson raise many issues, which will touch a chord in most scientists. They note that the 'professional credentials and status of the inventor or borrower' has a direct bearing on marketability of scientific research. 'Scientists who are well known and respected, based on their previous contributions to a field, have better chances of successfully introducing new theories than do less well-known researchers. The credibility of the scientist/marketer may add a halo effect to the theory product. Inventors or borrowers who do not enjoy a strong reputation in their fields (e.g. doctoral students) may need to attract established researchers ("celebrity scholars") to help market their theories'. In a telling comment the authors note: 'Strong empirical support is a highly desirable attribute that will enhance the marketability of a theory. However, strong empirical evidence is neither necessary nor sufficient for the successful dissemination of a theory'. Twenty years after the Peter and Olson analysis, it is clear that scientists in their diverse roles as researchers, authors, journal editors, referees, speakers, listeners, readers, committee members and administrators of science will, almost without exception, endorse the view that marketing is as important for scientific success as inventiveness, commitment, originality, scholarship or even plain good luck.

Publication in journals remains the most reliable way of 'marketing' scientific research, but the process of choosing journals and shepherding a paper past the peer review hurdles and overburdened editors is becoming an increasingly arduous task for authors. Even as the language of business and commerce has begun to dominate

science, arcane scientific journals have transformed themselves to compete in the marketplace. Journals try to claw their way up the 'impact factor' ladder and when they position themselves a rung above their competitors, they are quick to trumpet their success in 'hard to miss' advertisements. And, it is the impact factor that draws authors to exhibit their wares on the pages of a journal. In a recent commentary, Peter Lawrence notes in rather foreboding tones: 'The decision about publication of a paper is the result of interaction between authors, editors and reviewers. Scientists are increasingly desperate to publish in a few top journals and are wasting their time and energy manipulating their manuscripts and courting editors. As a result, the objective presentation of the work, the accessibility of articles and the quality of research itself are being compromised'. As Lawrence notes, with a tinge of regret that we all must share, 'even our language reflects this obsession – we say that Jim Jargon did well as a graduate student because he published a "*Cell* paper", illustrating that we now consider the journal more important than the message' (*Nature*, 2003, **422**, 259). The reference here is to the journal *Cell*, which raised the art of marketing biomedical science (and journals) to new heights, touching off an intense competition with journals like *Nature* and *Science* to lure the most glamorous authors to hawk their sensational findings on the journal's pages. Editors (and their assistants) attend international conferences to recruit authors, with incentives to relax review processes and old-fashioned flattery as weapons of choice (Journals Tussle Over Talent, Sam Jaffe, *The Scientist*, 2002, Oct. 14, pp. 59–60). In an editorial comment, Richard Gallagher of *The Scientist* notes that the 'big three' journals '*Nature*, *Science* and *Cell* undoubtedly have some say in the perception and development of science'. But he goes on to ask: 'Is it now time to dismantle the hegemony?' Gallagher raises the interesting thought 'that too much power is concentrated in the hands of a few individuals; that these individuals do not know what they are doing; or that they do know what they are doing, but operate on standards more akin to Hollywood casting than to standard peer review, giving preference to "star" names'. (*The Scientist*, 2002, Oct. 14, p. 10). As both Lawrence and Gallagher emphasize, scientists flock to high profile journals with an intensity that sometimes borders on desperation. Why do scientists do it? Lawrence provides a view from his vantage point as a researcher and author: 'Evaluations of scientists depend on numbers of papers, positions in lists of authors and journal's impact factors. In Japan, Spain and elsewhere such assessments have reached formulaic precision. But bureaucrats are not wholly responsible for these changes – we scientists have enthusiastically colluded. What began as someone else's measure has become our (own) goal. Although there are good reasons for publishing papers where they are more likely to be read, when we give the journal priority over the science we turn ourselves into philistines in our own world'.

In India the compulsive obsession with journals and impact factors has reached new heights. Like in everything else, the trends are set by administrators, who have uncritically embraced 'quantitative indicators', resulting in what Lawrence refers to as an 'audit society', in which each indicator is invested with a specious accuracy and becomes an end in itself.

The game of marketing science and scientists extends well beyond journals and the world of publishing. The conference and symposia circuit permits constant advertising. In India enormous sums of money, by local standards, are spent in hosting international superstars and meetings of dubious scientific value; undoubtedly an effort to market organizers and the hosting institutions Indian scientists spend considerable sums of money on travel to international meetings, in the hope that personal selling of results might score over anonymous peer review. Symposia are most often used for 'generating contacts', a term that will appeal to marketing executives, rather than to learn of new and useful science. Our evaluation system for scientific scholarship is becoming increasingly skewed in favour of those who would make even aggressive marketing executives blush.

As the marketing of science becomes a commonly accepted practice, new problems have begun to emerge. In industry, false and specious claims are commonly used to push new products. We view with tolerance advertisements for creams that will safely lighten dark skins and for lotions that will allow the bald to sprout hair. Many claims for marketed products are simply untrue. But, in science false claims, marketed aggressively, can be extraordinarily detrimental, as researchers across the world attempt to replicate remarkable results, investing large amounts of time and money. The case of Jan Hendrik Schön, who took the world of nanotechnology by storm, publishing several papers in *Nature* and *Science* in the years 2000–2001, has been a shocking revelation of the infirmities of the peer review and editorial processes. Even more importantly, it raises doubts on the manner in which high stakes research is carried out, and internally assessed by collaborators, in the best laboratories of the world. The spate of retractions published in the 3 October 2002 issue of *Nature* is testimony to a fraudulent marketing plan that went astray.

There are many worrying issues to confront. In countries like India where laboratories, institutions and individuals struggle for success, on a playing field that does not often seem to be level, the danger that marketing may take precedence over science is ever present. In committees which decide large grants on mega projects, it is hard to sift fact from fiction; vigorous marketing can blur the distinction. So much is claimed, on occasion, that listeners must suspend disbelief. Like the White Queen in *Alice*, we may need to believe in the impossible.

P. Balaram

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EDITORIAL

Coexisting with viruses

'*No matter how selfish our motives, we can no longer be indifferent to the suffering of others. The microbe that felled one child in a distant continent yesterday can reach yours today and seed a global pandemic tomorrow. "Never send to know for whom the bell tolls; it tolls for thee*".'

Joshua Lederberg
Medical Science, Infectious Disease and the Unity of Humankind
J. Am. Med. Assoc., 1998, **260**, 684;
Current Contents, 1989, **12**, 145

'The United States should be investing efforts and funds to strengthen the health structures in countries around the world. If we were to help train experts in epidemiology and surveillance, strengthen laboratories in key regions and link them to the best labs in this country and around the world, and support WHO, we would help to create a true global health network. This investment would protect our country and every other against global epidemics, save millions of lives and change the US image from one of self-interest to one of human interest'.

Barry R. Bloom
Lessons from SARS
Science, 2003, **300**, 701

The first descriptions of acquired immune deficiency syndrome (AIDS) appeared in 1981. By 1984 it became clear that the causative agent of AIDS was a retrovirus, christened the human immunodeficiency virus (HIV). In the two decades since its discovery, HIV and AIDS have occupied centre stage in most discussions of public health. At times, the perceived importance of AIDS as a threat to human health has diverted public attention and research funds from diseases which afflict larger populations, exact a heavier human toll, but are restricted to the less developed countries. In a remarkably prescient address at a conference of Nobel laureates in 1988, Joshua Lederberg emphasized that 'the ravaging epidemic of acquired immunodeficiency syndrome has shocked the world. It is still not comprehended widely that it is a natural, almost predictable, phenomenon. We will face similar catastrophes again, and will be ever more confounded in dealing with them, if we do not come to grips with the realities of the place of our species in nature'. Lederberg posed the problem of our relationships with our microbial environment with great clarity: 'Human intelligence, culture and technology have left all other plant and animal species out of the competition. We also may legislate human behaviour. But we have too many illusions that we can, by writ, govern the remaining vital kingdoms, the microbes, that remain our competitors of last resort for dominion of the planet. The bacteria and viruses know nothing of national sovereignties. In that natural evolutionary competition, there is no guarantee that we will find ourselves the survivor'.

Lederberg's concerns voiced even as the AIDS scare peaked in the late 1980s, seem particularly relevant today, as a new viral disease, Severe Acute Respiratory Syndrome (SARS), appears to have emerged from the southern Chinese province of Guandong, with its first public victims having been identified in Hong Kong. SARS is accompanied by the commonplace symptoms of influenza and is an airborne disease. In today's world SARS has spread quickly through the Far East and even beyond; Toronto becoming an outpost in the Americas. The rapid spread of infection has been facilitated by air travel; infected individuals become viral reservoirs, transported in hours across continents. The SARS outbreak appears to be under control, a tribute to the stringent public health measures adopted in several Southeast Asian countries, whose discipline and organization would be hard to replicate in many less developed areas of the world. Even more strikingly, the response of the biomedical research community to SARS demonstrates the enormous power of the tools of modern biological research. The first clinically diagnosed cases of SARS appeared in hospitals in Hong Kong in late February. On 31 March researchers in Hong Kong reported that 'SARS appears to be infectious in origin' and noted that 'the microbiologic origins of SARS remain unclear' (K. W. Tsang *et al.*, *New Engl. J. Med.*, 2003; published at www.nejm.org). A week later, the clinical, laboratory and radiologic features of 138 patients was reported (N. Lee *et al.*, *New Engl. J. Med.*, 2003; published at www.nejm.org on 7 April). Barely ten days later, the causative agent had been identified as a 'novel coronavirus' (T. G. Ksiazek *et al.* and C. Drosten *et al.*, *New Engl. J. Med.*, 2003; published at www.nejm.org on 10 April). One of the authors, Carlo Urbani had already succumbed to the infection. The two simultaneously published papers,

which establish the viral etiology of SARS, have authors with affiliations to institutions in Atlanta, Hanoi, Singapore, San Francisco, Taipei, Hong Kong, Bangkok, Hamburg, Frankfurt, Marburg, Paris and Rotterdam remarkable testimony to the power of purposeful international collaboration. Three weeks after the identity of the SARS agent was established the complete genome sequences of distinct viral isolates were reported by two independent groups (Sciencexpress/www.sciencexpress.org/1 May 2003). The viral genome has a length of about 29,700 nucleotides, a tiny fraction of the over three billion bases that specify our genetic program. The analysis of the SARS virus genome reveals no similarity to other known coronaviruses, promising to keep biologists busy establishing the significance of the many unique features of the organism's genetic makeup.

The progress in understanding the SARS virus has been breathtakingly fast, demonstrating the virtues of networking and cooperation in an increasingly interdependent world. In the case of AIDS, two decades ago, it took about two years to identify HIV as the infectious agent; at that time competition won over collaboration (*Science*, 2002, **298**, 1726–1731). The SARS episode, by contrast has, thus far, been a model of enlightened scientific cooperation.

AIDS and SARS are viral threats that emerged with frightening rapidity, reminding us of the vulnerability of the human species to variant microbes, which can overwhelm our immune defences. Curiously, even as SARS edged Iraq from the headlines, an article in *American Scientist* (2003, **91**, 122) highlighted the problem of influenza, claiming that 'the world is teetering on the edge of a pandemic that could kill a large fraction of the human population'. The authors, R. G. Webster and E. J. Walker, trace the influenza epidemics of the 20th century, noting that the epidemic of 1918–1919 affected '500 million people across the globe and killed at least 20–40 million – more than twice the number who died on the battlefields of World War I'. The influenza viruses that cause human disease have their origin in avian viruses, migrating from birds to humans through intermediate hosts, 'usually domestic fowl or swine'. Webster and Walker note that 'most pandemics originate in China, where birds, pigs and people live in close proximity. Hong Kong's 1997 "bird flu" was an avian influenza virus that probably attained virulence through reassortment of genes from geese, quail and teal'. A rapid response to an emerging infection, by local public health authorities, effectively contained the 1997 influenza outbreak in Hong Kong. The ability of these viruses to mutate and undergo 'antigenic shifts' permits them to evade human immune systems and makes vaccination a poor strategy for combating infection.

The SARS outbreak has focussed attention on the problems of infectious disease. Over the last couple of years there has been much public discussion on the subject of bioterrorism, where a dangerous pathogen may be intentionally released into the environment. The countries with the greatest capabilities in the areas of biological weapons are also the ones who appear to perceive the greatest threats. The report of the United States, Institute of Medicine on *Microbial Threats to Health* (March 2003) begins with these words: 'Infectious diseases continue to be a serious burden around the world, in developing and industrialized countries alike. Whether naturally occurring or *intentionally inflicted* (italics mine), infections can cause illness, disability, and death in individuals while disrupting whole populations, economies and governments. And because national borders offer trivial impediments to such threats, especially in the highly interconnected and readily traversed "global village" of our time, one nation's problem soon becomes every nation's problem'.

The spectrum of biological infectious agents which can emerge from the cauldron of nature's evolutionary processes, pose a continual threat to the well-being of humans. In the struggle between microbes and man, who will prevail? A study of our shared evolutionary past may provide a glimpse of what awaits us in the future. On the broad canvas of a biology driven by Darwinian imperatives, there may be the prospect of a not entirely benign equilibrium. Lederberg strikes a positive note in his essay: 'Many defense mechanisms inherent in our evolved biologic capabilities, thus mitigate the pandemic viral threat. Mitigation is also built into the evolution of the virus: it is a Pyrrhic victory for a virus to eradicate its host! This may have happened historically, but then both the vanquished host and victorious parasite will have disappeared'. Lederberg adds: 'At evolutionary equilibrium we would continue to share the planet with our internal and external parasites, paying some tribute, perhaps sometimes deriving from them some protection against more violent aggression. The terms of that equilibrium are unwelcome: present knowledge does not offer much hope that we can eradicate the competition. Meanwhile, our parasites and ourselves must share in the dues, payable in a currency of discomfort and precariousness of life'.

The public health and biomedical research response to the SARS episode has been spectacular in its speed and efficiency. There has been little doubt about the nature of the disease and the causative organism; a sharp contrast to the 1994 Surat 'plague'. The sudden emergence of new infectious agents may exact a higher toll if the outbreak occurs in the more populous, less organized and underdeveloped parts of world. But even here, nature may have the final say. A constant exposure to pathogens can serve to enhance the level of natural immune competence. Lederberg must have the last word: 'Paradoxically, improvements in sanitation and vaccination sometimes make us more vulnerable because they leave the larger human herd more innocent of microbial experience'.

P. Balaram

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EDITORIAL

Rankings and Records

Cricket and indeed many sports are dominated by statistics. Connoiseurs of the game are quick to cite facts and figures. In today's world, one cannot escape displays of ever-changing rankings of teams and players, based on precise, constantly updated facts. A few weeks ago, the Australian cricket captain, Steve Waugh made yet another century, crossing the legendary Don Bradman's haul, to become his country's record holder for Test hundreds. When asked how he felt while overhauling Bradman, Waugh laconically noted that he had batted considerably more times than his illustrious predecessor. Waugh's message was clear; averages may be more significant than raw numbers. Ever since computers revolutionized the methods of data storage, handling and retrieval, science too has been gripped by the infection of making quantitative comparisons. For a long time after its inception, the weapon of the Science Citation Index (*SCI*) found little application in India. The database was expensive, relatively unavailable and few appreciated its value. Times have changed. The Institute of Scientific Information's (ISI) Web of Science, a beguilingly named network of science databases and connections to the literature, is now more widely accessible from several major institutions in India. Amateur analysts now abound. Total counts of papers from institutions and individuals, indexed in the *SCI*, can be easily computed. Journal impact factors can be used to provide a seductively misleading measure of average quality. With a little more work, citations per paper can be counted, providing yet another parameter for ranking individuals, departments, institutions, organizations and even nations. There is a vicarious pleasure in being able to compare oneself, apparently objectively and quantitatively, with one's colleagues; those on top clearly more comfortable with the derived rankings. In India, where the number of ISI indexed papers is relatively small and the number of citations they accrue is modest, the dangers of misrepresenting and misinterpreting publication statistics is great. Despite these misgivings, the power of the ISI databases for assessing published science cannot be underestimated.

The problem of measuring the impact of science is immeasurably complex. For an economic analyst, the measurement of returns of investment on science may appear hard to quantitate. Bibliometrics, which to the unsophisticated is the field of counting papers and citations, provides one measure of assessment; albeit, largely limited to the area of published, academic science. Some years ago at a colloquium organized by the US National Academy of Sciences entitled 'Science, Technology and Economy', J. Adams and Z. Griliches presented an interesting bibliometric analysis of US science, restricting their attention to five fields: biology, chemistry, physics, medicine and mathematics covering the period 1981–1993. They counted papers and citations, using the citations per paper with five-year windows. Their conclusion, which the authors highlight as 'inconclusive' are interesting: 'From the numbers we have, one could conclude that United States academic science has been facing diminishing returns in terms of papers produced per R&D dollar, both because of the rising cost of achieving new results within specific scientific fields and because of rising competition due to the overall size of the scientific enterprise both within the United States and worldwide, impinging on a relatively slow growing publication outlets universe. In terms of total citations achieved per R&D dollar, the picture is somewhat brighter, indicating a rising quality of United States science in the face of such difficulties, though this interpretation is clouded by the question whether the actual science is better or is it just being evaluated on a larger and changing stage (the growing number of journals and papers in the world as a whole and changing citation practices)' (*Proc. Natl. Acad. Sci. USA*, 1996, **93**, 12664). Any analysis done today in India may also show mixed trends; interpretations must be made circumspectly and cautiously.

There are many situations where the cataloguing of the most highly cited papers provide fascinating glimpses of the growth of a discipline and its connections with sister sciences. The *Journal of the American Chemical Society* (*JACS*), the flagship publication of an organization which often bills itself as the 'world's largest scientific society , has entered its 125th year of publication. A century and a quarter is long enough for a meaningful retrospective. In an editorial filled with facts, the journal's editor, Peter J. Stang provides a marvelous glimpse of the growth of modern chemistry, as mirrored on the pages of *JACS* (Stang, P. J., *J. Am. Chem. Soc.*, 2003, **125**, 1–8). Volume 1 of *JACS* appeared in 1879, at a time when science was dominated by European laboratories and journals. From a mod-

est beginning, 621 pages in Volume 1, the journal has swollen to over 15,000 pages in Volume 124, a growth which will be paralled by comparable journals in other fields of science, where research activity has increased spectacularly over the last century. Biochemists (many who are now transformed into molecular biologists and at times, even biotechnologists) will note the growth of the *Journal of Biological Chemistry* (*JBC*, founded 1905), over the last nearly hundred years. Indeed, chemistry dominated the pages of *JBC* for several decades; its transformation occurring only in the wake of the revolution in molecular genetics and cell biology. Physicists will, of course, correlate the growth of their discipline with the number of papers, pages and subsections in *Physical Review*. In his essay, Stang marks the *JACS* anniversary by listing (and ranking by total citation count) the top 125 papers that have appeared in *JACS*. Few modern chemists can confidently claim that they could have predicted the outcome. Leading the list by a very big margin is the famous paper by H. Lineweaver and D. Burk on 'The Determination of Enzyme Dissociation Constants' (*J. Am. Chem. Soc.*, 1934, **56**, 658). Cricket and chemistry aficionados will note fondly that its year of publication corresponds with one of Bradman's triumphal English campaigns. Lineweaver and Burk, who are now immortalized in a plot that bears their name have thus far accumulated 10,638 citations, placing them far ahead of the competition. In second place is the paper by M. J. S. Dewar and his colleagues; 'AM1: A New General Purpose Quantum Mechanical Molecular Model' (*J. Am. Chem. Soc.*, 1985, **107**, 3902) which has garnered 7623 citations. Will Dewar *et al.* overtake Lineweaver–Burk in the future? Citation frequencies and half-lives will determine the outcome, but in the race for citation stardom scientists, unlike cricketers, continue to have their hats in the ring long after they have retired from the action. Stang's list is interesting and revealing; there are only 5 papers in *JACS* which have accumulated over 4000 citations, with the only representative of synthetic chemistry being R. B. Merrifield's 'Solid Phase Peptide Synthesis. 1. The Synthesis of a Tetrapeptide' (*J. Am. Chem. Soc.*, 1963, **85**, 2149). Curiously, a cursory analysis of the top 25 papers suggests that about half of chemistry's statistical stars are theoretical in nature, while a quarter might be classified as biochemistry. Indeed a careful perusal of the *JACS* 'stars list' immediately identifies the techniques which have had considerable impact on studies of molecules; most prominently nuclear magnetic resonance (NMR). Nobel laureates are well represented in the top 125; R. B. Merrifield, J. A. Pople, L. Onsager, C. J. Pedersen, P. D. Boyer, R. S. Mulliken, E. J. Corey, H. C. Brown, I. Langmuir, R. R. Ernst, K. B. Sharpless, L. Pauling, V. du Vigneaud, R. B. Woodward, R. Hoffmann, P. J. Flory, D. J. Cram and M. Calvin find a place. While *JACS* may not have been the only outlet for 125 years of chemistry, Stang's list provides many insights into the development of a central area of science.

In considering rankings of papers, journals, scientists, institutions and nations it is important to remember that 'there is plenty of room at the top'. It is not difficult to identify truly important and useful papers by citation counts. It may be far more difficult to make distinctions at the bottom of the pyramid of science, where the base is very broad. How many citations must a paper gather before a reasonable degree of significance is attached. One hundred (a century in cricket, too) appears to be a good choice; Stang notes that the author having the most publications in *JACS* with 100 or more citations is E. J. Corey, a finding that should gladden the hearts of synthetic chemists. Records and lists of citations are now regularly compiled by ISI's *ScienceWatch*, investing science with some of the excitement and glamour associated with sporting events and beauty contests (cf. *ScienceWatch*, 1997, **8**, 2, for a list of the most highly cited authors in the Physical Sciences 1990–96).

The ranking of journals by impact factors has also transformed the way scientists choose journals as a vehicle to report their results; the higher impact journals being flooded with submissions. In most local assessments, a poorly cited paper in a high impact journal scores over a well-cited paper in a low impact journal. Most senior (and sadly enough, many younger) scientists in India appear to be innocent of the entire area of bibliometric analysis of science, resulting in the creation of an environment where misconceptions abound. The practice of assessing scientists in committees by quickly computing 'average impact factors' is pernicious; the ready acceptance of this parameter by many science administrators must be vigourously resisted. But, it is not only the average scientist who worries that his or her work may pass unnoticed, another drop in the expanding vastness of the scientific literature. Journal editors are also increasingly self-conscious about impact factors; sometimes pandering to transient fashions in science in order to enhance the immediacy of their journals. A thoughtful commentary in the *Journal of Bacteriology*, celebrating the centennial of the American Society of Microbiology, outlined an editorial life 'beyond the impact factor'. The essay noted that the impact factor assesses only a fraction of a journal's impact on the development of a scientific discipline' (Walker, G. C., *J. Bacteriol.*, 1999, **181**, 1). In the specific case illustrated, only 17% of the citations to the journal were used in the impact factor calculation, which is a good measure of current interest in an area. But, for conservative, professional journals a long range impact assessment may provide a better guide. In assessing journals and scientists, the dangers of cross-disciplinary comparisons are self-evident. Publication and citation practices vary and specific yardsticks are needed, before jumping to judgemental conclusions.

There is an element of compelling human interest in generating rankings of individuals and institutions. We might only do well to remember that for the Bradmans, Waughs, Tendulkars and Gavaskars to emerge there must be a very large number of people enthusiastically playing the game.

P. Balaram

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EDITORIAL

An editor's pleasure

The job of an editor often seems to involve a never-ending succession of routine chores; marking incoming manuscripts to referees, reading reviewers' reports on incomprehensible articles, composing soothing letters to irate authors, whose papers have been rejected by intemperate referees, and the inevitable struggle to beat printers' deadlines. At a journal like *Current Science*, editorial worries can extend to finances, circulation and, of course, impact factors. Misprints on the cover page can form the theme of a recurring editorial nightmare; fortunately in real life these are infrequent. An editor's tasks are generally routine, but the volume of clerical work can sometimes be tiresome. There are undoubtedly a few highpoints in an editor's existence. At *Current Science* one of the editor's privileges is to see all the books which come in for review. There is a certain indescribable pleasure in running one's hands over a glossy new book, randomly turning the pages to get a glimpse of its insides. On occasion, a book can lie with the editor for a few days, allowing hasty reading of bits and pieces of the text. Editorial privileges can run to even more extreme levels; I have at times received copies that do not appear to be intended for review. A few weeks ago there appeared on my desk a large package containing three excellently produced volumes entitled *The Shaping of Indian Science* (Vols I–III, Universities Press, Hyderabad, 2003). These volumes reproduce the Presidential Addresses at the annual Indian Science Congress, organized in three sections: 1914–1947, 1948–1981 and 1982–2003.

The Indian Science Congress was an idea that appears to have originated in 1911–1912, in a proposal that emanated from P. S. MacMahon of the Canning College, Lucknow and J. L. Simonsen of the Presidency College at Madras. The first Science Congress was held at Calcutta in 1914, under the presidentship of Asutosh Mukherjee, Vice-Chancellor of Calcutta University. His views on the utility of the Congress were clear: 'Personal association amongst scientific men may be pregnant with important consequences, not merely by fruitful exchange of ideas; cultivators of science, by periodical meetings and discussions, may bring their aims and views prominently into public notice, and may also, wherever necessary, press them upon the attention of the government—a contingency by no means remote, for, as experience has shown, even the most enlightened Governments occasionally require to be reminded of the full extent of the paramount claims of science upon the Public Funds.' (Vol. I, p. 2). In the years leading up to Indian independence, the Science Congress was presided over by both Indians and Englishmen. Prafulla Chandra Ray was President in 1920 at Nagpur. His address has much of relevance even today. In exhorting industrialists and princes of business to support the pursuit of science, Ray said: 'I stand on the platform of a city which is the home of a thriving cotton industry. Here we have merchant princes and successful mill owners and businessmen. The great philanthropist Andrew Carnegie, himself a self-made man, acted on the motto that "to die rich is to die disgraced" and gave away more than 100 crores mainly for workingmen's reading rooms and research institutes. I appeal to our wealth and eminence to follow in the footsteps of the great benefactors of men and I am sure that with their help the cause of science will flourish.' (Vol. I, p. 93).

The early presidential addresses and the biographical sketches provide a glimpse of the men and their times; M. Visvesvaraya, J. C. Bose, C. V. Raman, Shiv Ram Kashyap, M. N. Saha, U. N. Brahmachari, T. S. Venkatraman, J. C. Ghosh, B. Sahni, Ardeshir Dalal, D. N. Wadia, S. N. Bose, S. S. Bhatnagar and M. Afzal Husain among the Indians. Raman's 1929 address at Madras is devoted entirely to his discovery 'of a new phenomenon . . . which has a bearing on the fundamental problems of Physics and Chemistry'. (Vol. I, p. 227). L. L. Fermor's 1933 address at Patna begins with an allusion to Raman's impending move to the Indian Institute of Science and notes prophetically: 'At present Calcutta may be regarded as the centre of scientific research in India; but with the transference to Bangalore of one of her leading investigators, she will have to guard her laurels'. (Vol. I, p. 290). The birth of *Current Science* in July 1932 also finds a prominent place in Fermor's speech. Indeed, this journal's origins may be traced to discussions at the 1932 Science Congress in Bangalore. Fermor makes a case for subscribers: '. . . if Indian Science deserves the dignity of supporting an All-India journal in Science, it can easily secure this dignity by what is really a trivial increase in the number of subscribers; for what are 500 subscribers amongst over 300

million people.' (Vol. I, p. 291). I might indeed echo Fermor seventy years later in exhorting readers to subscribe, because no government grant can substitute for the support provided by a legion of dedicated subscribers, many of whom will also contribute to the journal. Volume I ends in 1947 with a 'stenographic summary' of Jawaharlal Nehru's address at Delhi on 3 January 1947. Nehru's words on the need for the Science Congress to work independently of government bear recounting: 'Governments may be good and may be bad, but Governments normally are very slow and the only thing that moves them is some immediate public outcry which affects their future indirectly. Therefore, I should discourage among the scientists a reliance always on what Government may or may not do.' (Vol. I, p.574).

In the years since independence, the Science Congress has slowly changed its character. It is an annual event which dominates the first week of January; a massive jamboree that moves around the country. The traditional inauguration by the Prime Minister is the centrepiece of every Congress; an occasion for government to periodically reiterate a commitment to support science. Security and formality dominate an event that appears increasingly designed to showcase the administrators of science. Scientists and academicians appear unimportant at the annual Congress, where even attendance dwindles after the inaugural session. In looking at the collected addresses of the presidents of the Congress, I realized that Vol. II (1948–1981) marked a transition, where a body of scientists slowly metamorphosed into an appendage of government. Vol. III lists 22 addresses. More than half of the Indian Science Congress Association's (ISCA) presidents over the last two decades have been Secretaries to the Government of India; undoubtedly men and women of eminence. The absence of academicians in large numbers tells the story; a gradual drift of perception, where the imprint of governmental authority holds sway over mere scientific scholarship. The volumes do indeed provide a fascinating glimpse of the growth of science in India over a period of 90 years. They should provide a starting point for a future analysis of the historical role of ISCA in 'The Shaping of Indian Science'.

There have been institutions which have been carefully nurtured by the commitment of very dedicated individuals and Bangalore is home to one of them. For the last 45 years the Geological Society of India has functioned unobtrusively, producing a journal, which has appeared with unfailing regularity. In 1974, B. P. Radhakrishna became its editor. In 1984, on the occasion of the society's silver jubilee, he began to write a monthly editorial. On the threshold of his 85th birthday, the Geological Society has produced an Anthology of Editorials authored by a man of extraordinary commitment, dedication and talent. (*Random Harvest*, Geological Society of India, Bangalore, 2003). This simply produced book must be read widely, for Radhakrishna writes with enviable clarity and simplicity on some of the most pressing problems of our time—water, environmental degradation, urbanization and conservation. He has, through his writings, been the most articulate champion of geology and allied disciplines, which today are the Cinderellas among the sciences in India. In his Preface, Radhakrishna recalls the objectives that Mahatma Gandhi prescribed for the editor of a newspaper, in *Hind Swaraj*: '(i) to understand popular feeling and give expression to it, (ii) to arouse among the readers some desirable sentiments, and (iii) to fearlessly expose popular defects'. Radhakrishna notes that his writings have been guided by these principles. True to his ideals, in 1989, when talk of scientific ethics in our circles was limited, Radhakrishna wrote on the V. J. Gupta scandal, which shook palaeontology, assuming international dimensions. Radhakrishna's essays, some short and others substantial, are unfailingly readable and of special value to uninformed readers like me. While the pages of *Current Science* carry a great deal of geology, I can hardly count myself as an enthusiast of the earth sciences. It is Radhakrishna's writings that have, for several years, highlighted the importance of a discipline that we need to purposefully exploit for progress.

Radhakrishna, and I must confess that I have never had the privilege of meeting him, comes across as a rare jewel, striking in his simplicity and prodigious in his talents. He notes: 'I am proud of the Indian way of life—a life of contentment, compassion for all living beings, respect for elders, tolerance, humility, service to others, belief in after life and rebirth, which has sustained Indian civilization for the last 3000 years and should not therefore be given up. These sentiments find a place in my editorials, reflecting my preference for the traditional Indian way of life.' He goes on to borrow from Tagore to list the benefits he has received from his writings:

'Thou hast made me known to friends whom I knew not;
Thou hast given me seats in houses not my own;
Thou hast brought the distant near and
made brother of a stranger.'

As Radhakrishna's colleague M. S. Rao notes in his thoughtful introduction, the 'Geological Society of India is honouring itself by this Anthology'.

The books that I have dwelt on in this essay have been produced in India. They deal with the growth of our science, provide insights into the men who were principal actors and highlight many problems which remain paramount today. They should be required reading for those who today would like to don the mantle of the shapers of science in India. In parts, they may prove inspirational to those merely interested in the pursuit of science. Receiving these books, unsolicited, at my desk has been a pleasant surprise; an editor's pleasure.

P. Balaram

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EDITORIAL

Supporting Science

'In the half century since Independence, India has been committed to the task of promoting the spread of science. The key role of technology as an important element of national development is also well recognized. The Scientific Policy Resolution of 1958 and the Technology Policy Statement of 1983 enunciated the principles on which the growth of science and technology in India has been based over the past several decades. These policies have emphasized self-reliance, as also sustainable and equitable development. They embody a vision and strategy that are applicable today and would continue to inspire us in our endeavours.'

Preamble – Science and Technology Policy – 2003
Ministry of Science and Technology

Statements of policy signify the long-term commitments of a government. When endorsed by Parliament they assume the dimensions of a statement of national intent and purpose. This year marks the unveiling of the new Science and Technology Policy – 2003. The year began with a Prime Ministerial pronouncement, at the Science Congress, on the need to 'debureaucratize' our scientific institutions. Indeed, one of the policy objectives of our new S&T Policy is: 'To vigorously foster scientific research in universities and other academic, scientific and engineering institutions; and attract the brightest young persons to careers in science and technology, by conveying a sense of excitement concerning the advancing frontiers and by creating suitable employment opportunities for them. Also to build and maintain centres of excellence, which will raise the level of work in selected areas to the highest international standards.'

The new S&T policy differs from its predecessors; the high-sounding phrases of the policy document are followed by a section outlining a 'Strategy and Implementation Plan'. For the practitioners of science in academia, some statements of intent may sound particularly encouraging: 'A concerted strategy is necessary to infuse a new sense of dynamism in our science and technology institutions. The science departments, agencies and other academic institutions including universities, i.e. the science and technology system as a whole, would be substantially strengthened, given full autonomy and flexibility and debureaucratized'. A clear statement regarding 'New Funding Mechanisms for Basic Research' will certainly draw the attention of academic scientists: 'The setting up of more efficient funding mechanisms will be examined, either by creating new structures or by strengthening or restructuring the existing ones, for promotion of basic research in science, medical and engineering institutions. In particular, administrative and financial procedures will be simplified to permit efficient operation of research programmes in diverse institutions across the country.'

In reading these sections of the S&T Policy 2003, it may be instructive to reflect on the history of organized government funding of science in India. The Department of Science and Technology (DST), which is the nodal agency for the promotion and support of science in India came into being only in 1971. The DST's major funding arm for supporting projects in all areas of science and technology, the Science and Engineering Research Council (SERC) came into existence in 1974. The SERC's mandate was to support basic research in new and emerging areas of science and engineering. Meeting three or four times a year, the SERC quickly became the driving force for funding a large number of individual investigator initiated projects all across the country. The 'thrust areas' program formulated in the early 1980s, following meetings of large groups of scientists at Baroda (now Vadodara) in the precincts of the then vibrant Indian Petrochemicals Corporation, was influential in directing funding towards chosen areas of basic science. The concept of Program Advisory Committees (PACs) introduced the organized peer review system on a broad scale, for the first time in India. Decisions on projects were now made by representative groups of researchers in specific disciplines. The PACs also assumed responsibility for evaluating the progress on projects which had been funded by the DST. The first 'group-monitoring workshops' were held in 1985; it is now hard to imagine that this mechanism of monitoring projects is less than twenty years old. The SERC has been extraordinarily influential, particularly in the first twenty years of its existence. This

body has provided funding for hundreds of investigators across disciplines and institutions; through specific programs, the SERC has been responsible for the creation of several major facilities, operated as national resources. In addition to funding research projects, the SERC mechanism has encouraged the participation of a sizable section of academic researchers in its various programmes. In the first week of July the SERC will hold its 100th meeting. While a centenary of meetings by a committee can hardly be a cause for celebration, the occasion undoubtedly provides an excuse for looking back and ahead. In over a quarter of a century, the SERC has provided an undeniable impetus for scientific research in India. Its mechanisms have been adopted by other funding agencies like the Department of Biotechnology (DBT) and the Council of Scientific and Industrial Research (CSIR), in its extramural programmes. However, as the scientific enterprise has grown, the time may be opportune to ask whether new mechanisms are needed to ensure the continued support of academic science and fundamental research.

The need to 'debureaucratize' (a word which does not roll pleasantly off the tongue) our science and technology institutions has been emphasized explicitly in the S&T Policy – 2003. Clearly, there is an implicit recognition, even in government circles, that our agencies for funding and promoting science are becoming enmeshed in bureaucratic problems. The time scales for funding individual investigator projects have slowly increased; the response times at agencies when questions or clarification arise are uncomfortably long; finance sections appear to hold sway, sometimes impeding the flow of funds to academic institutions, even after the approval of scientific committees. In some agencies, the malaise is deep rooted; others struggle to hold the line. The science agencies follow the rigidly laid down procedures of government; but their client institutions often have stubborn administrators, who follow an entirely different set of norms. In the clash of bureaucracies, the individual scientist is often a bemused and helpless observer. There are prolonged battles on 'utilization certificates', with agencies and institutions in permanent conflict over the format and the figures. The number of young and enthusiastic administrators at the science departments is diminishing; aging and retirements in an era of limited recruitments have taken an inevitable toll. The ambience in the science ministries provides little scope for rewarding administrators who perform; those who act as obstacles in the execution of projects can hardly be touched. The idea of administrative autonomy does not apply to the major funding arms of science, like the Departments of Science and Technology and Biotechnology. Instead, these agencies are enmeshed in a ministerial culture in which bureaucracy has acquired a stranglehold, despite the best efforts of many able and dedicated individuals, who have spent their careers in promoting and nurturing scientific activity.

Funding research projects or institutions is not the only activity in which the Ministries and agencies play a major role. For example, the fixation of scholarships to attract the best of students to science is an issue which concerns all academic institutions. While the initiative for the recent enhancement in Ph D scholarships emanated from the DST, the uncoordinated moves at the University Grants Commission (UGC) and the All India Council of Technical Education (AICTE), leave an impression that government bodies often seem to work at cross-purposes. Although the UGC and AICTE maintain a measure of autonomy, it is the Ministry of Human Resources Development (MHRD), which has increasingly called the shots. Despite the best of intentions all around, bureaucracy quickly gains a stranglehold.

The Prime Minister's statement at the Science Congress, the unambiguous enunciation of intent in the Science and Technology Policy – 2003 and the centenary of meetings of the Science and Engineering Research Council, provide a rationale for a critical examination of the structure of agencies, charged with the promotion and support of science. The time may be opportune to revive the idea of an autonomous National Science Foundation, which can operate out of a corpus generated through both government and private contributions. Those adept in financial matters will have little difficulty in envisaging scenarios, where a corpus of Rs 1000 crores or more can be generated. In this era of changing mores of financing, it may be worthwhile to think of new ways of recharging the scientific enterprise. Any new initiative will supplement and complement the existing mechanisms for supporting science.

P. Balaram

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EDITORIAL

Extinction

'"What I was going to say", said the Dodo in an offended tone, "was, that the best thing to get us dry would be a Caucus-race".

"What is a Caucus-race?" said Alice. . . .

"Why", said the Dodo, "the best way to explain it is to do it." '

Lewis Carroll
Alice's Adventures in Wonderland, 1865

'He wasn't alone after all. Standing on a golden perch behind the door was a decrepit-looking bird which resembled a half plucked turkey. Harry stared at it and the bird looked balefully back, making its gagging noise again. . . .

"Fawkes is a Phoenix, Harry. Phoenixes burst into flame when it is time for them to die and are reborn from the ashes." '

J. K. Rowling
Harry Potter and the Chamber of Secrets, 1998

June 21 is the longest day of the year. This year it was also the day that marked the release of J. K. Rowling's phenomenal best-seller, *Harry Potter and the Order of the Phoenix*. In the over 130 years that have passed since Lewis Carroll chronicled the adventures of Alice, one constant factor has remained a part of children's literature; mythical and extinct creatures come to life in a wonderful imaginary world. To many of us the Dodo is an ungainly, pompous looking bird, immortalized in John Tenniel's marvellous drawing of Alice receiving a thimble as a prize from the Dodo, at the end of the Caucus-race; an event at which the Dodo as the master of ceremonies, magisterially declares, '*Everybody* has won, and all must have prizes'.

Potter's phoenix is mythical, but Alice's Dodo did live comfortably for centuries on the island of Mauritius; its eventual demise as a species hastened by the arrival of the Portuguese on the island in 1513. Clara Pinto-Correia traces the history of the dodo's headlong plunge into extinction in a compellingly readable book, *Return of the Crazy Bird*: *The Sad*, *Strange Tale of the Dodo* (Copernicus Books, New York, 2003). The dodo has been described as a clumsy, placid, slow moving bird, which laid only one egg at a time and built no defense around it. As Pinto-Correia notes: 'The dodo had never encountered any enemies until the Europeans discovered the islands. For the same reason, the dodo had no idea what fear might be, thus acting "dumb" or "feeble of wit" as so many mariners would describe it.' Unprepared to defend itself against predatory humans, the last known sighting of the dodo occurred in 1681 (Pinto-Correia, p. 98). In less than 200 years after encountering humans, the dodo was extinct. Even in death, the dodo was to be subjected to indignity; the last known stuffed specimen at Oxford's Ashmolean Museum (an exhibit donated in 1683) was cast away on 8 January 1755. Only the head and foot were preserved and the rest burned as trash (Pinto-Correia, p. 142). It is presumably from drawings and the remnants that Tenniel resurrected the dodo in our imagination. In considering the swift demise of the dodo as a species, Pinto-Correia quotes Charles Lyell, the famous 19th century geologist: 'Some complain that inscriptions on tombstones convey no general information except that individuals were born and died – accidents which happen alike to all men. But the death of a species is so remarkable an event in natural history, that it deserves commemoration; and it is with no small interest that we learn from the archives of the University of Oxford the exact day and year when the remains of the last specimen of the dodo, which had been permitted to rot in the Ashmolean Museum were cast away.'

Centuries of biology have taught us one irrefutable truth; for the individual there is only one certainty in life and that is death. But, on longer timescales the fate that awaits individuals may also consume entire species. Any discussion of extinction must turn inevitably to the demise of the dinosaurs, another great favourite amongst children. There is something magnetic about the recreations of dinosaurs in print and on film; magnificent, terrible and awesome creatures that roamed the earth long before humans appeared. The word 'dinosaur' itself is of relatively recent origin; coined by the English naturalist Richard Owen in 1842. Images of dinosaurs are indelibly imprinted on the minds of those who have visited exhibitions of animated models or reconstructions of genuine fossils; and of course, by viewing Steven Spielberg's film *Jurassic Park*, based on Michael Crichton's book, which outlines a tantalizing scenario for resurrecting extinct species. Dinosaur extinction, unlike that of the dodo, appears to have been a cataclysmic event; the result of a mass extinction caused by the impact of a comet or an

asteroid with Earth, 65 million years ago. In a famous paper, the extraterrestrial cause for dinosaur extinction was first proposed by Luis Alvarez, Walter Alvarez and their colleagues (*Science*, 1980, **208**, 1095). The Cretaceous–Tertiary (K–T) mass extinction is the fifth and last great extinction episode in the Earth's history, 'wiping out an estimated 70% of all species worldwide' (Frankel, C., *The End of the Dinosaurs*, Cambridge University Press, 1999, p. 6). The first great mass extinction is estimated to have occurred 440 million years ago and the most recent accounted for the dinosaurs 65 million years ago. Palaeontologists also believe that at least 'twenty lesser mass extinctions have been identified in the fossil record, where the level of species killing exceeds 20%' (Frankel, p. 5).

Species extinction is a subject that should worry us all since ecologists have been increasingly projecting a much more rapid pace of species disappearance, as human activity dominates life on Earth. Robert May notes that the extinction rate has 'accelerated during the past 100 years to roughly 1000 times what it was before humans showed up'. He goes on to project a 'speeding up by a further factor of 10 over the next century or so'. His conclusion is then ominous: 'And that puts us squarely on the breaking edge of the sixth great wave of extinction in the history of life on Earth' (Wayt Gibbs, W., *Scientific American*, Nov. 2001, p. 30). The attempts of conservationists to spread the message of the importance of preserving biodiversity have thus far met with limited public support; in the poorer countries burgeoning populations and the imperatives of development leave little room for conservationist approaches. In the developed world, addictively affluent lifestyles can only be sustained at a high cost; most often by denuding the natural resources in other parts of the world. If the dodo is the first clearly documented example of extinction by human action, there are many more disappearances of species that remain unsung. A recent report highlights the 'catastrophic ape decline in western equatorial Africa' as a consequence of 'commercial hunting, facilitated by the rapid expansion of mechanized logging'. Both gorilla and chimpanzee populations are dramatically falling in Gabon and the Republic of Congo; Ebola haemorrhagic fever acting as an additional factor. A team of authors from three continents conclude rather sombrely: 'The stark truth is that if we do not act decisively our children may live in a world without wild apes' (Walsh, P. D. *et al.*, *Nature*, 2003, **422**, 611).

Extinctions happen too in the unseen world of microorganisms. Biologists estimate that there are about 1.5 million species of fungi, 1.0 million kinds of bacteria and half a million viruses; organisms which at times coexist peacefully with humans, plants and animals, but on occasion attack them. In the case of human pathogens, the smallpox virus is a prime example, a purposeful campaign of eradication has been successfully carried out; reservoirs of viruses presumably exist only in the vaults of the US and Russian biological weapons laboratories. The extinction of a human pathogen is, of course, an event for celebration. But, celebration is not always a unanimous reaction as demonstrated by the case of *Helicobacter pylori*. This organism which has been implicated as a causative agent in gastric ulcers and some forms of stomach cancer is rapidly on the decline, following the use of antibiotic therapies and improved sanitation. *H. pylori* has however been a component of our intestinal flora; its extinction may have unforeseen consequences. Murmurs against the purposeful eradication of *H. pylori* are already evident in the biomedical establishment. Curiously enough, this bacterium which survives in our gut, provides a way of tracking human migrations over the course of history (Whitfield, J., *Nature*, 2003, **423**, 583).

Human activity and natural cataclysms may not be the only causes of species extinction. Reductive evolution, when genomes are downsized and genes lost by mutation and rearrangement as organisms adapt to increasingly specialized niche, might eventually lead to the death of a species. The leprosy bacillus is a good example of an organism showing 'massive gene decay'. *Mycobacterium leprae* has a genome size of 3.27 million bases as compared to other mycobacteria which yield a genome size of 4.4 million bases. Indeed, the leprosy genome analysis leads to an estimate of a dramatically reduced number of protein products; a clear sign of an organism with impaired biochemical functions (Cole, S. T. *et al.*, *Nature*, 2001, **409**, 1007). *M. leprae* appears to be a species poised on the downward slope to a natural extinction, guided by poorly understood evolutionary pressures; a phenomenon that may provide some warning of the eventual fate that might await all species.

In wondering about extinction we must turn inevitably to the issue of predictions about the end of the human species. Astronomers and palaeontologists, comfortable with long timescales, are quick to accept a 'premise that the Earth will lose its plants and animals in a billion years or less, its oceans and all life within several billion years, and ultimately become assimilated into the red giant Sun in about 7 billion years' (Brownlee, D., *Nature*, 2003, **423**, 803). Undoubtedly, this is a disturbing future; but hardly one that is likely to cause us sleepless nights at present. Interestingly, Martin Rees, Britain's astronomer royal, raises the possibility that the end might be much nearer, suggesting that there is a good chance that extinction will happen by the end of the 21st century. For Rees, ironically, the end will be hastened by science and technology (*Our Final Hour*, Basic Books, 2003). Reviewing Rees' book, Brownlee asks: 'Can we manage the dark side of science and prove Rees wrong? Only time will tell.'

Nearly a century and a half has passed since Darwin produced his remarkable thesis on the *Origin of Species*. In this period much has been learned about the evolution of organisms. Biology, geology, palaeontology and astronomy provide the backdrop for the study of the birth and death of species. Whatever may be the cause of extinction we can be sure that dead species will not again rise like the phoenix from the ashes.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Science and Politics of AIDS

'Identifying HIV was the critical first step in defining the cause of AIDS, but, as Robert Koch so elegantly pointed out more than a century ago, showing that a particular infectious agent causes a specific disease can be an arduous process. This process is especially complicated when exposure to the agent is followed by an incubation period of months or even years before symptoms appear. Such is the case with AIDS.'

S. B. Prusiner
Science, 2002, **298**, 1726

'Despite its spectacular birthday the HIV-AIDS hypothesis has remained entirely unproductive to this date: There is as yet no anti-HIV-AIDS vaccine, no effective prevention and not a single AIDS patient has ever been cured – the hallmarks of a flawed hypothesis.'

P. Duesberg, C. Koehnlein and D. Rasnick
J. Biosci., 2003, **28**, 383

'Most tellingly – and tragically – many haemophiliacs who have been treated with clotting factors that were derived from large numbers of unscreened donated blood samples have developed antibodies to the virus and eventual AIDS. Duesberg continues to ignore or misinterpret these and many other facts about the virus.'

Christopher Wills
Plagues, Harper Collins, London, 1996, p. 225

Acquired immune deficiency syndrome (AIDS) entered the medical literature in 1981, with a description of immunodeficiency in homosexual men in the United States. Two years later the causative agent, a retrovirus, the human immunodeficiency virus (HIV) was identified by Luc Montagnier and his colleagues at the Pasteur Institute in Paris and Robert Gallo and his colleagues at the National Institutes of Health, USA. In an extraordinary essay commemorating World Aids Day, Stanley Prusiner, the 1997 Nobel laureate for his work in prions, notes that the discovery of the cause of AIDS 'ranks as a great scientific discovery'. But Prusiner goes on to note: 'However, I became disturbed by the rancor that seemed to engulf Montagnier and Gallo at each stage in the acquisition of new knowledge about this terrible disease' (*Science*, 2002, **298**, 1726). In a remarkable effort at peace keeping, Prusiner and the editors of *Science* put together a set of three essays in the 29 November 2002 issue of the journal. The first, authored by Montagnier, provides 'a history of HIV discovery' but notes: 'We still do not understand the origins of the AIDS epidemic; the slow destruction of the immune system by factors in addition to HIV ... the importance of cofactors in AIDS progression and virus transmission; and the nature of the HIV reservoir that resists triple drug therapy'. In the following piece, Gallo traces the 'early years of HIV/AIDS', in which many retroviral researchers jumped from cancer research to the emerging problem of AIDS. In the final essay, Gallo and Montagnier coauthor a view of the future, which begins with a frightening assertion: 'With close to 70 million people already infected with HIV and more than 20 million dead, AIDS is one of the great pandemics in medical history'. The discoverers of the AIDS virus conclude with many homilies directed at the developing world: 'There needs to be a strong political will on the part of the governments of developing nations and generous financial contributions from the developed world, conveyed through a United Nations organization, such as WHO or UNESCO, in coordination with UNAIDS. It is also important that developing countries themselves participate financially. We suggest that the amount of funding from developing countries for AIDS projects should be deducted from their national debts to developed countries' (*Science*, 2002, **298**, 1730). As Prusiner notes, in his introduction to this unique retrospective, the 'codiscoverer status' accorded to Montagnier and Gallo was 'a political solution ... to resolve the dispute over patent rights covering the blood test for HIV'. Prusiner and *Science*, thus, seem to have successfully negotiated the end of the first AIDS war.

How did an infectious disease that first appeared in the hospitals of San Francisco and New York, transform in about two decades into a major burden for the Third World? In marking the passage of exactly 20 years after the identification of HIV, *Nature Medicine* in its July

2003 issue provides a comprehensive round-up of the status of HIV/AIDS research. Anthony Fauci notes that 'the collective output of the HIV and AIDS research community has been prodigious; more than 125,000 papers related to HIV and AIDS are catalogued in the PubMed database of the National Library of Medicine' (*Nature Medicine*, 2003, **9**, 839). This extraordinary outpouring of biomedical research does not seem to have stemmed the progression of the disease worldwide; the incidence of AIDS is on the decline in the West, but appears on the upsurge in the poor countries. Since HIV infected individuals succumb to a host of opportunistic infections, the populations of the poor countries with a greater burden of infectious disease, might appear to be at great risk. How does a government, in the developing world, obtain accurate figures for the incidence of the disease? For example, in India the National Aids Control Organization (NACO) estimates about 3.97 cases of infection in 2001. However, the US intelligence agencies project figures of 25 million cases by 2010, a growth rate which is not borne out by any reasonable procedure for making realistic estimates. US intelligence reports can, of course, be dismissed as motivated. International agencies also seem to support highly inflated projections (A. S. R. Srinivasa Rao, *J. Biosci.*, 2003, **28**, 367). Epidemiological data based on seroprevalence, the appearance of HIV antibodies, may not correlate with the number of patients who are admitted to hospitals as confirmed AIDS cases. Fauci's article, quoting a UNAIDS report, warns that AIDS incidence is accelerating in 'China, India and parts of eastern Europe and central Asia'. The pattern of the spread of AIDS is clearly distinct from that of other infectious diseases, and may be contrasted with recent experience on the rise and fall of the SARS virus.

In a strange coincidence, even as the AIDS establishment celebrated the two decade old connection between the virus and the disease, *Nature Medicine*, an Indian journal provided the forum for Peter Duesberg to launch yet another attack on the HIV-AIDS connection (P. Duesberg *et al.*, *J. Biosci.*, 2003, **28**, 383). Duesberg, a virologist at the University of California, Berkeley, has long campaigned that HIV does not cause AIDS (*Scientific American*, August 2001, pp. 30–32). Largely discredited by the mainstream AIDS researchers, his views have been controversially supported by South African President Thabo Mbeki, who famously questioned 'the belief that AIDS is epidemic in South Africa and that HIV is its cause'. In his latest assault on the AIDS research establishment, Duesberg argues that recreational drugs and malnutrition may form the basis for the disease and that anti-viral chemotherapy may indeed do more harm than good. Such suggestions are hardly likely to meet with an enthusiastic response, particularly from major pharmaceutical companies which have licensed as many as 20 antiretroviral drugs with the US FDA in the period 1987–2003 (A. S. Fauci, *Nature Medicine*, 2003, **9**, 839).

Duesberg asks provocatively: 'Why is AIDS research not free to investigate non-HIV hypotheses?' His answer, which may strike a chord, in many who pursue lonely and often quixotic approaches in science, points to the influence of 'large government-sponsored research programs that dominate academic research... . Such programs favour individual investigators, who contribute to the establishment a maximum of data and a minimum of controversy'. Duesberg, argues that 'the corporate equivalent of the academia's peer review system would be to give General Motors and Ford the authority to review and veto all innovations by less established carmakers competing for the consumer'.

For viral diseases, the desirable goal would be the development of a vaccine. However, HIV has proved to be formidable foe. Indeed, the number of skeptics, who view many of the advanced vaccine research programs with little enthusiasm, has been growing. The recent failure of a vaccine based on a 'genetically engineered version of HIV's surface protein, gp 120', might have been anticipated by its critics. But, in a dangerous sequel the company, VaxGen of California, suggested that its product 'revealed a statistically significant efficacy rate in blacks, Asians and people of mixed race' (*Science*, 2003, **299**, 1495; **300**, 28; *Nature*, 2003, **423**). The corollary of such a conclusion is of import in the Third World; if accepted, a program to test these vaccines with World Bank and international agency support in Africa and Asia will quickly gain momentum. Not only will poor populations become guinea pigs, their governments will add to their debt burdens, paying interest on loans which are used to buy products of limited utility and unreasonable cost.

Despite the unremitting focus on the human immunodeficiency virus, its origins, evolution and transformation into a human pathogen are still a matter of scientific debate. Robin Weiss highlights a general consensus that the two most prevalent strains of the virus, HIV-1 and HIV-2, crossed over to humans from chimpanzees and sooty mangabeys about 60–70 years ago. Weiss goes on to look at HIV infection from an interesting biological perspective. He notes that 'AIDS is characterized by opportunistic infections', which in turn 'exacerbate HIV in a vicious cycle'. He argues that 'high-density immunodeficient populations are arguably unique in the annals of host–parasite evolution'. A large susceptible population may increase zoonoses, the spread of infectious agents from animals to man, by permitting adaptation of viruses, bacteria, fungi and protozoa. Weiss concludes in doomsday fashion: 'The AIDS pandemic compounds the threat from the deliberate or accidental release of new infectious agents' (R. A. Weiss, *Nature Medicine*, 2003, **7**, 887). If a connection can now be drawn between 'bioterrorism' and AIDS, the science and politics of this disease will continue unabated in the forseeable future.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Population

'I think I may fairly make two postulata.

First, That food is necessary to the existence of man.

Secondly, That the passion between the sexes is necessary and will remain nearly in its present state. . . .

Assuming then my postulata as granted, I say, that the power of population is indefinitely greater than the power in the earth to produce subsistence for man.

Population when unchecked increases in a geometrical ratio. Subsistence increases only in an arithmetical ratio. A slight acquaintance with numbers will show the immensity of the first power in comparison of the second.'

Thomas Malthus
An Essay on the Principle of Population (1798)

'*A struggle for existence inevitably follows from the high rate at which all organic beings tend to increase. Every being, which during its natural lifetime produces several eggs or seeds, must suffer destruction during some period of its life, and during some season or occasional year, otherwise, on the principle of geometric increase, its numbers would quickly become so inordinately great that no country could support the product. Hence, as more individuals are produced than can possibly survive, there must in every case be a struggle for existence, either one individual with another of the same species, or with the individuals of distinct species, or with the physical conditions of life. It is the doctrine of Malthus applied with manifold force to the whole animal and vegetable kingdoms; for in this case there can be no artificial increase of food, and no prudential restraint from marriage. Although some species may be now increasing more or less rapidly, in numbers, all cannot do so, for the world would not hold them.*'

Charles Darwin
Origin of Species

'*Coercive measures are often advocated for reducing fertility rates in the poorer countries. . . . The point is sometimes made that in a poor country, it is a mistake to worry too much about the unacceptability of coercion—a luxury that only the rich countries can afford. It is not obvious what this argument is based on. The people who suffer most from these coercive measures are often among the poorest and least privileged in the society. The regulations and the way they are operated are also particularly punitive with respect to women's exercise of reproductive freedom. . . . Since the advocacy of coercion, in different forms, has been growing in India, it is important to emphasize that it achieves little and destroys a lot. . . . Cooperation can contribute something that coercion cannot provide.*'

Amartya Sen
'Population Policy: Authoritarianism versus Cooperation'
Lecture, New Delhi
17 August 1995 (http://www.macfound.org)

'*In demographic terms, the population explosion is a numbers equation, birth rate decline lagging behind a fall in death rate. But in terms of historical action, the population explosion is the unequal diffusion of the two technologies of death control and birth control. One element, probably the most important, in this inequality stems from broad societal attitudes towards human reproduction. In the last analysis, it seems to be attitudes among decision makers and elite members of a society as they relate to the availability of choices, rather than attitudes among the world's contemporary millions, that still slow the repairing of the mistakes.*'

Malcolm Potts and Martha Campbell
'History of Contraception'
Gynecology and Obstetrics, 2002, vol. 6, ch. 8

India entered the new millennium with a population exceeding one billion. Malthusian fears of famine and food shortages seem to have been banished by the fruits of the Green Revolution. In the 1960s, when I first heard of Malthus in an undergraduate economics class, India's population was a little over half the present number, but famine, food imports and PL-480 were everyday words. The transformation of agriculture consigned Malthus to the seminar rooms of academia. In his 1995 lecture at Delhi, Amartya Sen traces the origins of the 'analysis of the population problem' noting that the Marquis de Condorcet had preceded Malthus in worrying about the consequences of increasing populations: 'When the increase of the number of men surpasses their means of subsistence, the necessary result must be either a diminution of happiness and population, or, at least, a kind of oscillation between good and evil. In societies arrived at this term, will not this oscillation be a constantly subsisting cause of periodic misery?' Unlike Malthus, Condorcet was an optimist, who saw that solutions to the problems created by burgeoning populations would emerge from 'a cooperative response through the reasoned agency of the people themselves'. Condorcet's view seems to have been vindicated, as voluntary family planning has completely

eliminated population growth as a threat to national economies in the developed world. Malthus' famous essay did however, contribute significantly in shaping Darwin's evolutionary synthesis; biology on a broad front may follow Malthusian dictates, humans, possibly, being an exception.

In reflecting on the voluntary control of population it is instructive to recall that birth control measures were publicly discussed in England, only several decades after Malthus wrote his essay. In 1877, Charles Bradlaugh and Annie Besant republished Charles Knowlton's strangely titled book, *The Fruits of Philosophy* (originally published anonymously in 1832). Knowlton provided the first explicit prescriptions for birth control. The government prosecuted the publishers; the Bradlaugh–Besant trial, which ended in their acquittal, legitimized public discussion of birth control. Annie Besant (1847–1933) was to go on, in a remarkable career, to become the President of the Indian National Congress in 1917, a champion for 'Home Rule' and self government in India. The publicity that attended the prosecution of Bradlaugh and Besant drove the sales of Knowlton's family planning booklet from 'fewer than 1000 a year . . . to more than 100,000 in the three months preceding the trial'. Annie Besant later wrote her own book, *The Law of Population*, which she dedicated to 'the poor in our great cities and agricultural districts, dwellers in stifling courts or crowded hovels, in the hope that it may point a path from poverty and may make easier the life of British mothers' (Potts, M. and Campbell, M., *Gynecology and Obstetrics*, 2002, vol. 6, ch. 8). In a sympathetic article on Besant's 'many lives', Kumari Jayawardena notes that even before her 'Indian phase', which began in 1893, Besant had cited India in her book 'as an example of overpopulation' (*Frontline*, 1997, **14**, No. 20). At that time our country's population would have been less than a quarter of what it is today. Birth control became an acceptable subject for discussion in the United States largely through the efforts of Margaret Sanger (1879–1965). In a striking throwback to the days of Besant, Sanger was prosecuted in America in 1914 for publishing a pamphlet called *Family Limitation*. She fled briefly to Europe but, when the government of the day withdrew its charges, returned to open 'America's first birth control clinic in Brownsville, New York', in October 1916. Several decades later, Sanger, along with Katharine McCormick, was instrumental in inducing Gregory Pincus to develop synthetic progesterone analogs as oral contraceptives, at the Worcester Foundation for Experimental Biology in the early 1950s. The application of progesterone as an anti-ovulant coincided with the remarkable flowering of the chemistry of steroids. The pace of development of the 'birth-control pill' and its rapid acceptance in the West is unlikely to be matched by any pharmaceutical product today.

Family planning and birth control were high on the government's agenda in the 1960s and 1970s, but the coercive strategies of the mid-1970s, notably under the cover of the 'Emergency', backfired, setting back population control in many poor states by several years. As Amartya Sen notes: '. . . The assembling of poorer women in sterilization camps through various kinds of pressures, is a practice of remarkable barbarity and injustice. . . . (V)oluntary birth-control programmes in India received, as family planning experts have noted, a severe setback from that brief programme of compulsory sterilization, since people had become deeply suspicious of the entire family-planning movement. Aside from having little immediate impact on fertility rates, the coercive measures of the emergency period were, in fact, followed by a long period of stagnation in the birth rate, which ended only in 1985'. Coercion, persuasion, education, inducements, penalties, legislation and economic uplift have all been viewed as instruments of population control. Medical science has provided the means of birth control; the more difficult task of spreading the virtues of the small family norm remains to be accomplished. According to present projections, India will overtake China as the most populous country on Earth in the early decades of this century; a distinction that is hardly a cause for celebration. The recent decision of the Supreme Court upholding a Haryana Government law prohibiting a person from holding a 'Panchayat' post if he or she had more than two children has once again sparked discussion. Laws regulating politicians and lawmakers are hardly likely to make any impact and only serve to divert attention from the central issue of evolving and implementing purposeful strategies for limiting overall growth rates and improving economic conditions for the poorest sections of the population.

Does science still have a frontline role in the area of birth control? Is there a case for the much touted 'contraceptive vaccine' of the 1970s? Immunocontraception, targetted primarily at women, was a highly funded research activity in India in the period between the 1970s and 1980s. The projects floundered; mired in both scientific difficulties and worries about public acceptability. Funding agencies began to seek a way out in the 1990s, with a committee report setting the stage for discarding all talk of 'birth-control vaccines', accepting 'the concerns expressed by some women's groups and by individual analysts, that immunocontraception will not prove to be a biomedically appropriate contraceptive option for women in general and that research and development investment in the area would be better placed elsewhere'. A review of the area concludes rather sombrely, 'immunocontraception itself may not have a place of its own in the fertility control programme at the moment' (Rao, A. J., *J. Biosci.*, 2001, **26**, 425).

Although Malthus highlighted food as the most important ingredient for survival, his postulates must, of course, be enlarged to encompass many biological and natural resources. Overpopulation and unrestrained consumption to maintain unsustainable lifestyles are likely to lead to a catastrophic situation in the future. Natural resource depletion is directly linked to human activity. Food, water, energy and ecological damage must be of major concern to policy makers, who face the daunting task of balancing the aspirations of growing populations with the constraints of limited resources. We live in a far-from ideal world and it may be impossible to exorcise the ghost of Malthus.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Nanotechnology

The principles of physics, as far as I can see, do not speak against the possibility of maneuvering things atom by atom. It is not an attempt to violate any laws; it is something, in principle, that can be done; but in practice, it has not been done because we are too big.

Ultimately, we can do chemical synthesis. A chemist comes to us and says, 'Look, I want a molecule that has the atoms arranged thus and so.' ... The chemist does a mysterious thing when he wants to make a molecule. He sees that it has got that ring, so he mixes this and that, and he shakes it, and he fiddles around. And, at the end of a difficult process, he usually does succeed in synthesizing what he wants. By the time I get my devices working, so that we can do it by physics, he will have figured out how to synthesize absolutely anything, so that this will really be useless.

But it is interesting that it would be, in principle, possible (I think) for a physicist to synthesize any chemical substance that the chemist writes down. Give the order and the physicist synthesizes it. How? Put the atoms down where the chemist says, and so you make the substance. The problems of chemistry and biology can be greatly helped if our ability to see what we are doing and to do things on an atomic level, is ultimately developed – a development which I think cannot be avoided.

— Richard P. Feynman
There's Plenty of Room at the Bottom
Engineering and Science, February 1960

Development of the ability to design protein molecules will open the path to the fabrication of devices to complex atomic specifications, thus sidestepping obstacles facing conventional microtechnology. This path will involve construction of molecular machinery able to position reactive groups to atomic precision. It could lead to great advances in computational devices and in the ability to manipulate biological materials.

— K. Eric Drexler
Molecular Engineering: An Approach to the Development of General Capabilities for Molecular Manipulation
Proc. Natl. Acad. Sci. USA, 1981, **78**, 5275

They reconfigured the nanoparticles to add solar power and memory. They rewrote the particle program to include a genetic algorithm. And they released the particles to reproduce and evolve, and see if the swarm could learn to survive on its own.
And they succeeded.
It was so dumb, it was breathtaking.

— Michael Crichton
Prey, Harper Collins, 2002

Nanotechnology's beginnings may be traced to Eric Drexler's 1981 paper in the *Proceedings of the National Academy of Sciences USA*. Drexler's focus then was on proteins; his paper contained a single table in which the common devices of everyday technology were related to a biological counterpart, invariably a protein molecule. Drexler compared cables and collagen, drive shafts and bacterial flagella, containers and vesicles, pipes and tubular structures and likened glue to intermolecular forces. He even made a comparison of 'sigma bonds', an everyday connection in molecules to rotary bearings. Drexler's paper, remarkable for its free-ranging speculation appears to have successfully bridged the gap between the terms that engineers readily comprehend and the sometimes impenetrable language of biochemistry. Drexler, of course, was inspired by an even earlier masterpiece of loud thinking; Richard Feynman's famous lecture on miniaturization, delivered at Caltech on 29 December 1959. Over four decades later we are still a long way from assembling molecules by putting 'the atoms down where the chemist says'. But the ability to manipulate atoms and molecules has advanced enormously since Feynman's days. Nanomachines, nanorobots and nanotechnology have become popular in fiction; Michael Crichton's bizarre vision of swarms of intelligent nanoparticles hunting down humans in the Nevada desert appears to have risen from a confluence of information technology, biotechnology and nanotechnology.

Nanotechnology seems to be on everyone's lips these days. A clear sign of a new and booming field of science is the number of conferences that are advertised, many in attractive and exotic locations. Government agencies also begin to target funding into the new boom areas. The US Congress is reported to have earmarked '$2.36 billion over three years for nanotech R&D' (Perkel, J. M., *The Scientist*, 28 July 2003, p. 20). In India, the Department of Science and Technology has already adopted nanotechnology for a special thrust and undoubtedly other agencies will follow suit. The Department of Bio-

technology has advertised for projects in the area of 'nano-biotechnology', a fusion of the two most glamourous prefixes in science today.

Most importantly, in projecting the prospects for a new 'nano era', it is 'technology' that is emphasized. There are two possible reasons for this. It is possible that 'nanoscience' is already a well-established discipline. Or, it might be that we could conceivably reap the benefits of a new technology without necessarily investing a great deal of time and money understanding the underpinning science. Drexler's 1981 paper offers some insights into his views at that time: 'Engineers (in contrast to scientists) need not seek to understand all proteins but only enough to produce useful systems in a reasonable number of attempts ... . Thus, the difficulties encountered in predicting the conformations of natural proteins do not seem insurmountable obstacles to protein engineering.'

Two decades later the obstacles, to 'protein engineering' in the production of technologically useful devices, are still formidable and largely unsurmounted. A quick search of the Internet with *Google* yielded 536,000 hits for nanotechnology and only 58,000 hits for nanoscience; clearly a sign that nanotechnology must be a popular term. Remarkable definitions abound on the World Wide Web. Consider this: 'Manufactured products are made from atoms. The properties of those products depend on how those atoms are arranged. If we rearrange the atoms in coal we can make diamond. If we rearrange the atoms in sand (and add a few other trace elements) we can make computer chips. If we rearrange the atoms in dirt, water and air we can make potatoes.'

The exploding interest in nanotechnology almost two decades after Drexler's paper does not seem to be based on any one major scientific advance. The atomic force microscope and the optical tweezer still appear to be research devices and not the routine machines of technology. How then did nanotechnology suddenly move to centre stage? In searching for answers I found it difficult to find definitions of the field but did come across some interesting statements: 'Unlike such fields as immunology and electronics, nanoscience deals not with a specific area of research, so much as a length scale: 0.1 to 100 nanometers' (Perkel, J. M., *The Scientist*, 28 July 2003, p. 20). This report goes on to quote the US National Nanotechnology Initiative: 'Nanotechnology is concerned with materials and systems whose structures and components exhibit novel and significantly improved physical, chemical and biological properties, phenomena, and processes due to their nanoscale size'. The Internet provided a more pithy definition of 'nanotechnology', 'molecular nanotechnology' or 'molecular manufacturing': 'Whatever we call it it should let us (i) Get essentially every atom in the right place (ii) Make almost any structure consistent with the laws of physics that we can specify in molecular detail (iii) Have manufacturing costs not greatly exceeding the cost of the required raw materials and energy.' To me, all of this sounds very much like chemistry and chemical technology.

Is nanotechnology really chemistry in disguise? Is it a new discipline that is springing up on the borders of biology, chemistry, physics and engineering, which will eventually have a unique identity of its own? Or is it an attempt to project a new range of potential applications based on conventional science? Moving atoms and molecules as Feynman envisaged in 1959 still seems to lie at the heart of the aspirations of nanotechnology. This has always been the realm of chemistry, with particle sizes alone constituting the parameter for distinguishing chemistry from nanoscience. 'Biology inspired physics' is another new term that is making the rounds; an area which should at some stage seamlessly integrate into nanotechnology. There are many terms which have become commonplace in the literature of chemistry, particularly at its borders with physics and biology, which appear to describe old phenomena in new language; investing conventional topics with a novelty that journal editors and referees find beguiling. Molecules nowadays do not 'crystallize', as they did in my youth; instead they 'self assemble', conjuring up a Drexlerian vision of a new technology in the making. The electron microscope has reached a level of sophistication that 'seeing' the nanoworld has become possible; Feynman would have been immensely gratified. As a result, nanoscientists (technologists?) describe with an infectious exuberance their observations of 'nanotubes'. The fullerene (carbon) nanotubes are the current favourite, little hollow carbon pipes which have for the past several years attracted great attention (Yakobsen, B. I. and Smalley, R. E., *American Scientist*, 1997, **85**, 324). The literature is now full of reports of 'nanotubes' observed in unique situations. My favourite is a paper, coauthored by the late Max Perutz, which carries an emphatically assertive title: 'Amyloid fibrils are water filled nanotubes' (Perutz, M. F. *et al.*, *Proc. Natl. Acad. Sci. USA*, 2002, **99**, 5591–5595). Biologists are undoubtedly drawing inspiration from the lexicon of nanotechnology. There is a sudden resurgence of interest in old areas. The physicists have discovered colloids and the chemists have rediscovered the hydrogen bond. Condensed matter physicists, materials scientists and many 'born again' chemists have converted to the new religion of nanotechnology. Very soon an army of biologists will move in, promising the fruits of fusion – nanobiotechnology. With a little luck and of course, plenty of work new technologies and even new science may undoubtedly emerge.

Even as the number of private nanotechnology ventures increases in the West in a manner reminiscent of the growth of the information and biotechnology industries, the backlash from environmental activists has already begun. Michael Crichton's novel *Prey*, which is based on an implausible scenario, may serve as a rallying point for activists against a new 'nano threat' (*Physics World*, July 2003, p. 13; *Nature*, 2003, **424**, 237). The often wild claims made on behalf of nanotechnology by its proponents will fuel the backlash. Interesting times lie ahead; neither the nanotechnologists nor the activists are capable of reigning in their imaginations. Raucous debate, enlivening but rarely informing, will undoubtedly continue for some time to come.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Pesticides in the environment

'The discovery of DDT was made in the course of industrious and certainly sometimes monotonous labour; the real scientist is he who possesses the capacity to understand, interpret and evaluate the meaning of what at first sight may seem to be an unimportant discovery.

Dr Paul Müller . . . Your discovery of the strong contact insecticidal action of dichloro-diphenyl-trichloroethane is of the greatest importance to the field of medicine. Thanks to you, preventive medicine is now able to fight many diseases carried by insects in a way totally different from that employed heretofore. Your discovery furthermore has, throughout the world, stimulated successfully research into newer insecticides.'

G. Fischer, Presentation Speech:
Nobel Prize in Physiology or Medicine, 1948

'Even the mode of action of DDT poisoning in an insect is far from fully explained. . . . The real cause of death is today still unknown. . . .

The field of pest control is immense, and many problems impatiently await a solution. A new territory has opened up for the synthetics chemist, a territory which is still unexplored and difficult, but which holds out the hope that in time further progress will be made.'

Paul Müller
Nobel Lecture, 11 December 1948

'The "control of nature" is a phrase conceived in arrogance, born of the Neanderthal age of biology and philosophy, when it was supposed that nature exists for the convenience of man. The concepts of applied entomology for the most part date from that Stone Age of science. It is our alarming misfortune that so primitive a science has armed itself with the most modern and terrible weapons, and that in turning them against the insects it has also turned them against the earth.'

Rachel Carson
Silent Spring, Hamish Hamilton, 1962
Penguin Books, 1965, p. 257

'From analysis of 36 samples of 12 different brands of soft drink samples it can be concluded that . . . Lindane, DDT and its metabolites, Malathion and Chlorpyrifos were most commonly found in 36 soft drink samples tested. . . . No pesticide residues were detected in the Coca-Cola and Pepsi samples from USA manufactured by the same multinationals.'

H. B. Mathur, S. Johnson, A. Kumar
'Analysis of Pesticide Residues in Soft Drinks', Centre for Science and Environment, New Delhi, 5 August 2003

In 1948 the Nobel Committee was exuberant in its recognition of Paul Müller's discovery of DDT, a magical chemical that appeared to eliminate lice, the vector for the transmission of typhus. DDT's enormous potential in the control of mosquitoes, and by extension, malaria, was evident. For all practical purposes, DDT appeared to be non-toxic to man, truly a magic bullet that targetted only the enemy. Fourteen years after Müller received the Nobel Prize, Rachel Carson wrote *Silent Spring*, a book which was to become the catalyst for the environmental movement. Carson marshalled her facts in highlighting the disastrous ecological consequences and hazards of indiscriminate spraying of insecticides. She noted sombrely: 'The crusade to create a chemically sterile, insect-free world seems to have engendered a fanatic zeal on the part of many specialists and so-called control agencies. On every hand there is evidence that those engaged in spraying operations exercise a ruthless power' (*Silent Spring*, p. 29). Carson mounted a major attack on synthetic pesticides, which she labelled as 'elixirs of death'. She pointed out that the pesticide industry 'is a child of the Second World War. In the course of developing agents of chemical warfare, some of the chemicals created in the laboratory were found to be lethal to insects. The discovery did not come by chance: insects were widely used to test chemicals as agents of death for man' (*Silent Spring*, p. 31). Four decades after Carson, pesticide manufacturers are among the most powerful multinationals in the world. These huge conglomerates dump tons of chemicals banned for use in several western countries into the Third World, abetted by regulatory bureaucracies who do not exercise control, when it matters most. The chlorocarbons and the phosphorus-containing pyrethroids are easy to produce and profit margins are enormous. Pests abound in the tropical countries. Pesticide use has grown in India and the developing

world, even if ineffective and counter-productive. Resistant strains of pests, mosquitoes among them, have appeared; a Darwinian response, in which the most resilient of pests have been chosen to multiply and spread their resistance. The dangers posed by pesticides to human health have now been well documented in the scientific literature, but indiscriminate spraying of chemicals continues, despite publicly expressed fears. The suspicions about the connection between the use of endosulfan and the growing evidence for human deformities in Kerala, have been growing. Distressingly, no clear, credible, scientific study has cleared endosulfan, which continues to be used, primarily on the strength of claims made by manufacturers of the chemical.

Environmental concerns have been raised by individuals and non-governmental organizations (NGOs) in India, over the last quarter of a century. In recent times, the magazine *Down to Earth*, nurtured for years by the late Anil Aggarwal, has provided a forum for the expression and analysis of concerns over the growing deterioration of our environment. The Centre for Science and Environment (CSE) in Delhi has recently set the cat among the pigeons, by releasing a report on the pesticide content of soft drinks manufactured and marketed by the two most powerful multinationals in the area, Coca Cola and Pepsi Cola. The CSE's report is a well-written document which details the methods and protocols used in a study which concludes that several pesticides are present at concentrations far above norms prescribed in the European Union. Disturbingly, the very same drinks bottled overseas have undetectable levels of these pesticide residues. The study used gas chromatography as the method of analysis, following standard protocols published by the United States Environmental Protection Agency (EPA). The conclusion, of course, was that the manufacturers used contaminated water, not bothering to put in place a clean-up procedure. The companies also appeared to follow quality control procedures which adopted different standards in the West and in India.

In a strange sequel to the episode, the government seemed to rise to the defence of the Cola companies, with two laboratories (including the Central Food Technological Research Institute, CFTRI), apparently providing analytical data, which cast doubt on the CSE report. I was unable to obtain a copy of the report of the government laboratories; an initial response to a request was brushed aside saying that the case was *sub judice*, a phrase that brooks no further appeal. Comparing methods, protocols and analytical data was therefore not possible, but the claims and counter-claims clearly emphasize the need for setting up professional analytical laboratories which can provide credible reports, using the most sensitive modern technologies. Gas chromatography is now complemented by liquid chromatography coupled to mass spectrometry, permitting detection and at times, *de novo* identification of trace chemicals. Application of such methods requires both expensive instrumentation and trained scientists. While the former is sometimes accessible, the latter are not always in place. In all areas requiring chemical analysis, watchdog regulatory agencies must have under their control, laboratories with the greatest technical competence.

The machinery of government has also grown too ponderous, bureaucratic and non-technical. Multiple ministries are involved in issues raised by the problems of the food and pharmaceutical industries and the environment. In most committees and ministries the dominant quality that one encounters is a disturbing disregard for technical issues. Bureaucratic wrangling takes precedence over matters of science and technology. It is therefore unsurprising that government statements on issues of chemical analysis and toxicity are met with well-founded scepticism. The Cola controversy may remain unresolved and marketing muscle, aided by political influence, may prevail. But, the episode does have a silver lining; it has focused public attention, albeit briefly, on the important problem of growing contamination of depleting water and soil resources by a battery of chemicals, which may have drastic long-term consequences for human health.

The West has long ago put in place elaborate mechanisms for analysing toxic residues and estimating risks due to long-term exposure to chemicals. Much can be learnt from the models that already exist elsewhere. It may be time to consider the need to dramatically upgrade the capabilities in regulatory laboratories and to ensure their proper functioning. Publicly funded institutions must be charged with a specific mandate to address environmental issues and act responsibly in matters that concern the health of the population. Unfortunately, we have a situation where government reacts only when NGO's raise questions of immediate importance. We might remember that those at greatest risk from chemical pollutants are children, who will have the longest exposure to Rachel Carson's 'elixirs of death'.

In considering the problems of controlling pests and pesticides it may be useful to recall Julian Huxley's words in his preface to Carson's book: 'Pest-control is of course necessary and desirable, but it is an ecological matter, and cannot be handed over entirely to the chemists. The present campaign for mass chemical control, besides being fostered by the profit motive, is another example of our exaggeratedly technological and quantitative approach. The ecological approach on the other hand involves aiming at a dynamic balance, an integrated pattern of adjustment between a number of competing factors or even apparently conflicting interests.' The CSE's widely publicized reports on the quality of bottled water and the Colas have highlighted the growing problem of pesticide overuse and misuse. Purposeful action by government bodies is necessary.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Teachers and Mentors

Teacher's Day is celebrated annually in India on 5 September, to mark the birth anniversary of Sarvepalli Radhakrishnan (1888–1975). S. Radhakrishnan had a remarkable academic career, which took him from a lecturership in the Department of Philosophy at Presidency College, Madras to Professorships at Mysore University and Calcutta University and eventually to Vice-Chancellorships at the Andhra University and Banaras Hindu University. For some time in the 1930s he was the Spalding Professor of Eastern Religions at Oxford University. He went on to become the President of India in 1962, capping a long and distinguished career in education and public service. Radhakrishnan wrote and spoke extensively on higher education; he was the scholar's scholar in the study of philosophy and religion. After his death his birthday has become the occasion to remember our teachers. Curiously, UNESCO also observes Teacher's Day, a month later on 5 October, every year.

Radhakrishnan's life coincided with the remarkable growth of modern science and in his prodigious writings he often alluded to the results of science: *Science reveals to us the inexhaustible richness of the world, its unexpectedness and wonder. Science does not profess to solve all problems. There are regions where its writ does not run. When all is said and done, the world remains a mystery. The ultimate questions are too deep and mysterious for science. We should recognize that we understand and control only a tiny fraction of the universe.* (*The Present Crisis of Faith*, Vision Books, Delhi, 1970, 1983, pp. 171–172). Radhakrishnan integrated Darwinian thought into his writings: *The story of continuous development through the whole of animate nature suggests the working of an automatic mechanism. No principle outside the natural world is needed to account for it. In a closed world governed by uniform laws, no spiritual principle can interfere . . . . We cannot be sure that species move on to higher stages of development in orderly sequence. Ever so many degenerate and some die out altogether. No sooner had some form of existence perfected itself than it proceeds to decay. The progress we have achieved is the result of the terrible method of trial and error. Struggle and suffering, disease and death are such pregnant facts that if there is any ruling power in the universe, it may be fate or chance or careless gods, but in no case a beneficient providence. Man is nothing more than the latest of a long series of living creatures and he did not arrive on this planet faultless and finished but is being slowly ground into shape by the shocks of circumstance.* (Radhakrishnan, *An Anthology* (ed. A. N. Marlow), George Allen and Unwin Ltd, London, 1952, pp. 87–88). The neutrality of science was readily incorporated into his writings, which focused largely on philosophy and religion. Radhakrishnan is eminently quotable and Marlow's anthology provides some stellar examples: 'We often refuse to admit facts, not because there is evidence against them, but because there is a theory against them' (p. 145). Radhakrishnan, and legions of teachers are now cursorily remembered on Teacher's Day, an occasion which is marked by obligatory functions in many schools, colleges and universities. I found myself a little over a fortnight ago at a seminar in the Biochemistry Department at Osmania University; an occasion where several retired faculty members had returned to their old department to be honoured by a new generation. There was an appealing sentimentality to the proceedings; a feature slowly disappearing from many of our institutions, where teachers are quickly forgotten by students whom they have shaped and sometimes, nurtured.

In institutions of higher education and research teachers are not only those who lecture in classrooms, but include those who initiate and guide students in the intricacies of research, which often leads to a doctoral degree. Here teachers are variously termed as professors (a generic term which accommodates all levels), guides, advisors and supervisors. Each of these terms appears to convey a distinct meaning. *Professors*, seem to have both high academic rank and presumably, correspondingly high scholarly attainment. *Guides* seem to be academics, who shepherd the novice student through difficult areas of research. *Advisor* (a common term in America) appears to signify a member of the faculty, who proffers advice when asked, suggestive of a largely independent role for the student in carving out a research problem in a specific area. *Supervisor*, is a descriptor now commonly used in many institutions, including my own. This is a term which seems to charge the academic faculty with a dominant role in shaping a student's thesis; conjuring up a vision of a shop floor (or even a Roman galley), where legions of workers are orchestrated to perform assigned tasks, by a superior authority. *Teacher* is a term that has strangely fallen into disuse in the academic research community. But there is another term and it is my favourite – *mentor*. The word's origins may be traced to Greek mythology and Homer. Odysseus entrusted his son Tele-

machus' education to his friend Mentor, before setting off for the Trojan War and his long voyage which was to give the English language yet another word – odyssey. Mentor was charged with shaping a future king and today mentor is another description for an all-round teacher, who gently prepares his students to face the challenges of the future.

Semantics apart, the relationships between research students in science and their professors, guides, advisors, supervisors or mentors really shape the attitudes of a beginner in science. The best of supervisors (and I choose this term because it appears most fitting in the modern context of research as a professional and at times, almost corporate activity) lead by example; their knowledge, enthusiasm and commitment to their disciplines often proving infectious, greatly facilitating the process of learning. The worst of supervisors are those for whom a research student is a commodity of academic life, with the supervision of Ph D theses merely being necessary for personal advancement. In many of our institutions, faculty promotions use the number of 'Ph D's produced' as an index of performance of the faculty member, reducing the Ph D to a number to be added to a score sheet. Unsurprisingly, in many institutions the least academically inclined members of the faculty have large groups of Ph D students; a complete denigration of the academic process. Between these two extremes, are the situations, where the process of Ph D research academically benefits both the teacher and the student, in a cooperative and continuous process of learning.

In all our advanced institutes for scientific research, Ph D students have become the most important element in conducting new and original research. Barring the laboratories of the strategic departments, students are really the front line of research activity in academia. As research acquires an increasingly competitive edge, scientists clamouring for success, recognition and fame transmit their fears and insecurities to their students; a situation that leads to far from ideal 'mentoring'. In the frenetic modern world, academic success is measured in terms of grants, impact factors of journals in which papers are published and awards; rarely is scholarship and the ability to transmit knowledge viewed as an important attribute of a supervisor. Indeed, the gulf between the traditional 'good teacher' and the modern upervisor' has been growing.

The demands of the research ambience in most of our better-known institutions place both research supervisors and students under pressure to obtain publishable results quickly; the professional advancement of faculty and students rests substantially on their ability to produce papers. The absence of postdoctoral associates in large numbers distinguishes the Indian research scene from the situation in the West. Ph D students now work in the front line of projects, which have much larger goals, than merely training students for a research career. The temptation to cut corners in the fast track is overwhelming, sometimes misguiding students and producing young scientists, who have a completely distorted perspective of the practice of science. Another distressing situation which prevails in many universities and some national laboratories is the practice of allowing an almost unlimited number of students to register for Ph D degrees, under a research supervisor. The 'pair of hands' syndrome dominates some disciplines, with the ability to perform physical labour as the sole criterion for entering a research laboratory. In these surroundings it is hard to think of the group head as a 'guide' or 'teacher', with little intellectual discourse between the student and the supervisor. The tendency to view research students as an essential part of the institutional infrastructure, provided to faculty, is growing. There is little or no consideration in many places of the need to promote 'teachers', who provide students with a balanced and healthy view of their chosen disciplines.

In the most successful examples of establishing a scientific tradition by training students, there have been famous instances of scientific dynasties, in which academic success has visited every succeeding generation. But in India, the flowering of schools of science has been limited and the traditions of the great teachers of the recent past have not survived the tide of modernity. A former associate of mine, after reading some of these columns, sent me a marvelous book, *Apprentice to Genius* by Robert Kanigel (The Johns Hopkins University Press, 1986), which traces 'the making of a scientific dynasty'. Kanigel, who is best known for his book on the life and times of Srinivasa Ramanujan, *The Man who Knew Infinity*, follows the growth of the discipline of neuropharmacology, focusing on student–mentor relationships. He spins a riveting tale beginning with the original teacher, Bernard Brodie and moving through three successive generations, Julius Axelrod, Solomon Snyder and Candace Pert. Axelrod, of course, won the Nobel Prize in Medicine in 1970 for his work on chemical neurotransmission, but earlier, together with his teacher Brodie, he did the work that led to the discovery of tylenol. Snyder and Pert were to discover the opiate receptor and to contribute enormously to the understanding of endogeneous opioids, work which fetched a Lasker Award for the former. Kanigel's study emphasizes the tensions and rewards of a 'traditional master–apprentice' relationship. Kanigel sums up these relationships succinctly: 'Through it, the student is trained in the ways of the mentor, and comes away with an approach, a style, a taste in the mouth or a feel in the gut for just what makes "good science". Through it, favours are granted, careers advanced, the sway of a particular scientific discipline extended. But through it, too, resentments sometimes spark, lifelong bitterness is kindled.'

Modern science is hardly a solitary pursuit. Collaborations and interpersonal chemistry determine the outcomes of many research projects. Teaching and mentoring have been critically important for the development of science in the past and their role will, undoubtedly, remain undiminished in the future.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Current Science: Then and now

Current Science was born in July 1932. The journal's conception took place at a meeting of academicians during the Indian Science Congress at Bangalore, in January 1932. A little earlier in August 1931, Martin Forster, the Director of the Indian Institute of Science, Bangalore had circulated a questionnaire soliciting views on the starting of an 'Indian Science News Journal', patterned after *Nature*. Forster's initiative was based on the enthusiasm of a small group of scientists at Central College and the Indian Institute of Science. It was a wonderful time to think of starting a high quality interdisciplinary science journal in India. C. V. Raman had received the 1930 Nobel Prize in physics. The stirrings of science were evident in many universities in India. Forster's proposal met with enthusiastic approval and by early 1932 several influential scientists had promised support; among them were Raman, S. S. Bhatnagar, M. N. Saha, Birbal Sahni and J. C. Ghosh. The first issue of the new monthly journal appeared in July 1932, after a remarkably short gestation period. The inaugural issue was a slim 22 pages and carried research communications covering a wide spectrum of science, from the 'Raman Effect in Liquid Carbon Dioxide' to the 'Longevity of Micro-filariae (*Wuchereria*) *Bancrofti*'. The Board of Editors was identified: F. H. Gravely, Superintendent, Government Museum, Madras, C. R. Narayana Rao, Professor of Zoology, Central College, Bangalore, V. Subrahmanyan, Professor of Biochemistry, Indian Institute of Science, Bangalore and B. Venkatesachar, Professor of Physics at Central College. The annual subscription was Rs 6 and individual copies were priced at a princely, 12 annas. The second issue appeared promptly in August 1932; its contents included an unsigned editorial article on the 'Future of Agriculture in India' and the obituary of Sir Dorabji Tata, dwelling on the tradition of philanthropy nurtured by J. N. Tata and his son. This issue also carried an appeal for a *Current Science Rupee Fund*, which exhorted all those 'interested in science in India' to contribute 'one rupee'. A group of scientists who would help collecting the one rupee donations was listed; their addresses ranged from Lahore (S. S. Bhatnagar) to Dacca (J. C. Ghosh) and Allahabad (M. N. Saha) to Madras (H. Parameswaran). The *Current Science Working Committee* noted that 'if all were to help the aggregate . . . would suffice to relieve the Committee from pecuniary anxiety'. *Current Science* in its early days was off to a flying start with the editors of *Nature* (Richard Gregory), *Die Naturwissenschaften* (Arnold Berliner) and *Science* (J. Mekeen Catell) becoming corresponding editors. Paradoxically, the pages of the early issues of the journal suggest that the world was a smaller place in those days.

The editorial columns of *Current Science*, in the first two decades of the journal's existence, addressed many key issues of national interest. In May 1933, the journal carried an unsigned editorial entitled 'An Indian Academy of Science'. Authored by the journal's first editor C. R. Narayana Rao (1882–1960), the essay noted that 'the early establishment of a National Academy of Science should secure closer and better organized co-operation of activities among all research institutes in India, and exercise through its official journal a wider influence for the consolidation and promotion of the best interests of science'. The editorial envisaged an Academy that would 'be a company of thinkers, workers and expounders comprising members of the New Estate upon whose achievements the world must in future depend for the preservation and advancement of civilization'. In words which appear curiously out of place today, the essay suggested that for members of the Academy 'their professional spirit must be service, rendered with absolutely no thought of personal advantage' (*Curr. Sci.*, 1933, **1**, 335). B. R. Seshachar in an obituary of Narayana Rao notes that 'the founding of the Indian Academy of Sciences at Bangalore under the Presidentship of C. V. Raman was a result of this appeal' (*Curr. Sci.*, 1960, **29**, 173). Over six decades later, Sivaraj Ramaseshan attributed the authorship of the 'Academy essay' to C. V. Raman. He also wryly noted 'that the essay was so effective and logical that any and everyone who read it wanted to start an academy of sciences' (*Curr. Sci.*, 1994, **67**, 636). The first two editors of *Current Science* – C. R. Narayana Rao (1932–1942) and the biochemist M. Sreenivasaya (1942–1950), of the Indian Institute of Science – steered the journal to a prominent role in the shaping of science

and science communication amongst academics in India. Sreenivasaya, one of the founding members of *Current Science Association* was prescient in considering the quality of our journals, which had begun to multiply in the late 1940s: '. . . when one takes into account the quality of our work, the manner of presentation, and the standard of get up of a good proportion of these journals we shall be confronted with a depressing situation which does not warrant any complacency on our part. Many of the journals have a poor circulation and their finances are far from satisfactory'. Raman became President of the Current Science Association in 1947 and was a major influence until his death in 1970. A succession of editors, G. N. Ramachandran (1950–57), A. Jayaraman (1957–58), A. S. Ganesan (1958–73), M. Sirsi (1973–74) and M. R. A. Rao (1974–88) have steered the journal's course, through the decades when the face of science changed with extraordinary rapidity. While *Current Science* remained the medium for publishing very short research communications and announcements, the journal had to face competition from several specialist journals published in India and the rapidly growing number of international journals, which attracted the best of Indian authors. By the 1970s the editorial columns no longer appeared with any regularity; the journal slowly transformed into a vehicle for publishing short research reports. This transition coincided with the rise of scientometric analysis, which provided rankings of journals and scientists; Indian journals began to become increasingly self-conscious about their 'impact'. In 1978, S. Ramaseshan speaking at a seminar on Primary Communication in Science and Technology in India said: 'To any one who surveys the quality of scientific publications produced in the country, the position would certainly appear to be bleak. This need not lead to total pessimism – as there are still a few journals in India which must be considered good by any standard of assessment. The question, therefore, is not whether we can produce a good scientific journal in India but what is it that has made the majority of them so bad'. In his lecture Ramaseshan made many new proposals for revitalizing struggling journals. He noted: 'But problems in India are different and difficult. We must evolve our own methods of tackling them'. Eleven years later in March 1989, Ramaseshan was a lead speaker at a 'Brain Storming Session on Indian S&T Journals' organized by IIT Madras and a non-governmental organization, 'Patriotic and People-Oriented Science and Technology Foundation'. He had just taken over as editor of *Current Science*, a journal with limited circulation and impact. It was my privilege to hear him; almost the only speaker with a passion for running scientific publications. He stood out among the participants, many of whom suggested impracticable solutions to the difficult problem of improving journals in India.

Ramaseshan, whose 80th birthday this issue celebrates, turned his talents to the task of transforming this journal at a time when journals, the world over, were entering a period of transition. The electronic revolution was still on the horizon, but colour, high quality printing, rapid refereeing and punctual production were the accepted international norm. This journal had been converted into a fortnightly in 1964, straining publication schedules. The experiment that he set in motion is still being conducted. Ramaseshan restored the original conception of the journal as a medium for both original research and general matters of interest to scientists. He introduced the 'Correspondence' columns, which now appear regularly, as a device to present diverse opinions on matters of concern to science and scientists. On occasion, he initiated a debate on controversial matters, which invariably elicited a wide response. Colour and a constant improvement in the quality of paper and printing were a feature of the years after Ramaseshan entered the editor's office. He has been a tireless solicitor of manuscripts, an enthusiastic proponent of thematic issues and a resolute champion of the right to criticize establishment positions.

In reviewing, albeit briefly, the journal's past and in recording our appreciation of Sivaraj Ramaseshan's stewardship, which has brought us to the present, it may be appropriate to dwell a little on what may lie ahead. The journal is larger, glossier and hopefully, more readable than it was fifteen years ago. Publication schedules are adhered to, circulation has increased and the journal's finances are no longer a nightmare for its managers. The journal's local impact appears to have risen, although its 'impact factor', as measured by the ISI, could do with considerable improvement. There are several issues of concern. The journal still does not attract enough of the large number of good research papers produced by Indian scientists. Some fields of science, notably physics, chemistry and molecular biology are under-represented on the journal's pages. Refereeing procedures take an inordinately long time on occasion and the gap between receipt and publication of a manuscript is sometimes unacceptably long. We need to plot a course towards steady and speedy improvement. The journal must attract authors and readers; it must necessarily offer rapid publication for the former and interesting fare for the latter. In a hark back to 1932, my colleagues and I must appeal to the Indian scientific community; subscribe, contribute, read and cite the journal. As an interdisciplinary journal which publishes original research, reviews, general articles and commentaries on issues of policy, *Current Science* is unique in the developing world. In opening its pages to debate and dissent the journal upholds the tradition of free enquiry. The journal has had a rich past and will undoubtedly have an even richer future. It should be our endeavour to raise the journal to a high rank in the world of science; there can be no better tribute to the founders of *Current Science* and Sivaraj Ramaseshan, to whom we owe an immeasurably large debt of gratitude.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Nobel Prizes: Hits, Misses and Fouls

'...it appears that the Nobel prize as an institution gets high marks in its efforts to identify good science and to honor good scientists. But it has been far less effective in taking into account the vast changes in the cognitive map of science that have taken place since the prize was first instituted... . Far less is known than we need to know in order to assess the actual effects of the prize on the advancement of scientific knowledge and on the quality of scientific life. We have yet to discover, whether in an ironic turnabout, the prize has diminished rather than augmented the legitimacy of the reward system in science.'

— Harriet Zuckerman
Scientific Elite, The Free Press, 1977, p. 248

'Whether all these laureled "discoveries, inventions, and improvements" have proved themselves contributions "most materially of benefit to mankind" – to quote Alfred Nobel – remains an entirely open question. Science's contributions to war, pollution, social blight, and other problems have prompted a decline in the so-called religion of science. Literature's benefit to the world now often seems confined to a few rather than the multitude: film became by far the dominant popular art in the twentieth century. As for peace, little needs to be said about civilization's success in reining back war and armies. The Nobel's own influence – whether beneficial or corrupting to science, literature and peace – is also entirely unsettled."

— Burton Feldman
The Nobel Prize
Arcade Publishing, New York, 2000, p. 23

October begins with an observation of the International Day for Older People, declared by the United Nations. In India, the first day of the month marks 'Elders Day', with the obligatory rallies, seminars and advertisements drawing attention to the problems of an increasing population of the elderly. Despite a little cursory research, I was unable to find the age at which one joins the international community of 'older people'; although, I suspect, 60 years seemed a reasonably good guess. The first half of October is also dominated by the Nobel prizes, announced every year in dramatic fashion in Stockholm, following an elaborate selection process, shrouded in secrecy. The 'elderly' did very well in this year's awards. The physics laureates, Alexei Abrikosov (75), Vitaly Ginzburg (87) and Anthony Leggett (65) and the winners for medicine Paul Lauterbur (74) and Peter Mansfield (70) had certainly reached a mature age. The laureates for chemistry seemed surprisingly young; Peter Agre (54) and Roderick MacKinnon (47), sharing the award for their high impact work on protein channels in cell membranes. The chemistry prize appears to cast a wider net than the physics award, encompassing many areas of modern biology. The medicine award which recognized rather belatedly, the enormous medical importance of magnetic resonance imaging, honours advances in the area of magnetic resonance, which might, with some justification, be considered a domain of physicists. But, disciplinary labels can often be misleading; Ernest Rutherford, who was famously dismissive of chemistry as a science, was awarded the 1908 Nobel prize for chemistry. Burton Feldman in his extensive analysis of the Nobel prizes concludes that the prizes in chemistry 'cover a field that is rich, sprawling, and not a little untidy.... Its details are overwhelming.... Coping with such a prodigality of topics has bred more and more specialities, and kept the Nobel chemistry jury busy from its start' (p. 202).

Feldman's analysis has a little section entitled, 'Notes on How to Win the Nobel Prize in Science', a tongue-in-cheek prescription for would-be laureates. His advice is simple: 'First come from or emigrate to the US, Britain or Germany: they did and still do dominate the prizes'. Feldman dwells upon pedigree, mentors, persistence and luck but finally concludes: 'Live to a very old age. They may finally catch up with you'. Many of the recent Nobel awards point to the importance of patience, persistence and longevity. Age, despite widely held perception, is no bar to scientific discovery in many fields; the elimination of mandatory retirement in the United States has permitted many productive scientists to continue their research, enhancing their chances of contributing decisively to the solution of important scientific problems. Both analysts of the Nobel prizes, Zuckerman (p. 164) and Feldman (p. 131) quote a famous piece of verse, ascribed to an icon of theoretical physics, Paul Dirac:

Age is, of course, a fever chill
That every physicist must fear
He's better dead than living still
When once he's past his thirtieth year

Feldman, for good measure also attributes to T. H. Huxley a statement 'recommending strangling scientists at sixty, as too fixed in their thinking' (Note 12, p. 423). But times have changed since Huxley's 19th century and the early decades of the 20th century, when Dirac

worked; the myth that science is exclusively the preserve of youth may no longer be tenable.

The juries that decide awards always face an enormously difficult task; the task of making distinctions between individual scientists and diverse contributions is never easy. Often, personal prejudices of committee members and their advisers play a major role; dispassionate, objective judgements are only theoretically possible. In the case of Nobel prizes the stakes are so high and the public scrutiny so unremitting, the awarding committees face an almost impossibly difficult task. Even in the cases of the most deserving awards, there is a difference in the reactions of the scientific community to cases where prizes are awarded for a single important discovery and those which appear to be given for 'lifetime achievement' in a respectable area of science. Sometimes the award winners for 'discoveries' and 'inventions' do not appear to be mainstream scientists, although their work is of enormous impact in the world of science. The examples of Kary Mullis, who introduced the polymerase chain reaction (PCR) into molecular biology (Chemistry, 1993) and Koichi Tanaka (Chemistry, 2002), who invented the matrix-assisted laser desorption ionization (MALDI) method, which has revolutionized the biological applications of mass spectrometry, immediately spring to mind. 'Lifetime awards' are always less controversial because the laureates occupy a prominent position, within the structure of the 'scientific elite'. There is an analogous situation in Hollywood. Actors and actresses who may never have won an Oscar for a specific role, may nevertheless be honoured with the prestigious lifetime achievement award.

The Nobel prizes have achieved an enormous public prestige; testimony to the remarkable calibre of the laureates over a period of a century. While controversy and skepticism often follow the Peace and Literature prizes, the awards in science have generally gone to scientists of undisputed accomplishment. The Nobel memorial prize instituted in Economics in 1968 is slated to become more controversial with each passing year: its credibility dimmed by repeated awards to economists, who owe allegiance to the conservative school at the University of Chicago, resulting in what Feldman terms as a 'self-perpetuating effect'. Despite their undoubted eminence, the science prizes have been dogged by their share of controversy. This year's award of the medicine prize for magnetic resonance imaging has been publicly challenged in a series of press advertisements by Raymond Damadian, who believes he deserves credit for inventing the method. Damadian, a medical scientist, turned businessman and entrepreneur in 1978, when he founded the Fonar Corporation to convert laboratory experiments into practical imaging machines. Damadian is a highly visible, controversial, but well recognized figure in the area of MRI. He shared the US National Medal of Technology with this year's Nobelist, Paul Lauterbur in 1988 and was inducted into the National Inventors Hall of Fame in 1989 and more recently, received the MIT-Levenson Lifetime achievement award for inventors in 2000. Damadian was indeed the first person to suggest that 'nuclear magnetic resonance (NMR) techniques combine many of the desirable features of an external probe for the detection of internal cancer' (*Science*, 1971, **171**, 1151). Medical imaging of course traces its roots to Paul Lauterbur's paper in *Nature* (1973, **242**, 190), which introduced the method of image formation and proposed the name 'zeugmatography'. Damadian, recognized the potential for medical imaging, coined the term MRI (hiding the fearsome prefix 'nuclear', which would have undoubtedly frightened patients) and made practical application a reality. As a digression I must note that while Lauterbur's seminal 1973 paper makes a passing reference to 'variations in water content and proton relaxation times among biological tissues' and holds out the possibility of 'selectively picturing the various soft structures and tissues', there is no reference to Damadian's 1971 paper. Omissions in the Nobel awards are fairly common; famous examples include Lise Meitner who should have shared the Nobel prize in 1944 with Otto Hahn for the discovery of nuclear fission. The prize for the discovery of the double helical structure of DNA in 1962, included Maurice Wilkins in addition to James Watson and Francis Crick. Another potential candidate, Rosalind Franklin died prematurely in 1958. In his analysis, Feldman (p. 262) notes that Wilkins' inclusion was due entirely to the pressures exerted by Lawrence Bragg. Evidently, both inclusions and omissions can result from the many external and internal pressures that build during the process of Nobel judgement.

The Nobel committees do a difficult job and they are certainly not infallible. In the past they have also been, on occasion, poorly informed. A celebrated case is the award of the medicine prize in 1952 to Selman Waksman for the discovery of streptomycin. The antibiotic was discovered by Albert Schatz working in Waksman's laboratory, with little direct contribution from the latter. Curiously, the Nobel committee seems to have been unaware of Schatz at that time, although he and Waksman had been publicly involved in a dispute over the streptomycin patent which was filed in 1945 and granted in 1948. There are also cases where the awards are hard to justify, even acknowledging that the later generations of analysts have the benefit of hindsight. Two awards in medicine appear as glaring examples of misjudgement. In 1927, Julius Wagner–Jauregg received the prize for treating patients 'suffering from insanity caused by syphylitic infection' by inducing fevers by inoculating them with malarial parasites. A little over two decades later Egaz Moniz, a Portuguese neurologist received the prize in 1949 for introducing a surgical procedure, then known as prefrontal leukotomy (now, lobotomy), for treatment of schizophrenia. While no treatments were available at that time for schizophrenia, it is still difficult to rationalize the Nobel committee's rush to judgement.

With the range of scientific activity rapidly expanding and with the growing importance of team effort in solving major problems, Nobel judgements will become increasingly difficult. Given the track record of the juries in Stockholm we can be sure of many hits, a few misses and some fouls.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Publishing Wars

The birth of a new science journal should normally have been an unremarkable event; after all, there are already thousands of journals representing the diverse areas of science. But the journal *PLoS Biology* which came into being in October, attracted unprecedented media attention; its launch preceded by a blitz of publicity, including a television commercial. The journal has a curious name, deriving its acronym from its publishers, the Public Library of Science (PLoS), a California-based organization which was founded in October 2000. The stated goal of PLoS was to make the 'world's scientific and public literature a public resource'. The efforts of the organization 'to encourage scientific publishers to make the archival scientific research literature available for distribution through free online public libraries of science', did not appear to meet with enthusiasm from the science publishing industry; their responses falling 'short of the reasonable policies' advocated by PLoS (www.plos.org). Armed with a $ 9 million grant from the Gordon and Betty Moore Foundation, the organization has now launched *PLoS Biology*, in a dramatic attempt to hasten the 'open access' revolution.

Physicists have, for over a dozen years, used the electronic pre-print bulletin board, established by Paul Ginsparg in 1991, to post their manuscripts in advance of formal publication. Initially founded to link the high-energy theoretical physics community, the e-print archive has grown to other areas of physics, recently expanding even into biology. Despite the growth of Internet connectivity worldwide, a 'pre-print bulletin board' concept has not found general acceptance in other areas of science. Many journals in chemistry and biology, including those published by scientific societies disqualify manuscripts which have been previously 'pasted' on electronic bulletin boards. The major criticism of this approach is that it permits display of manuscripts before peer review; uncritical acceptance of results and conclusions may hinder rather than hasten progress of an area. The physicists would argue that readers are the best judges; presumably able to sift fact from artifact. This may indeed be true in physics, where the readership may be confined to a group of closely knit specialists. But, the sprawling area of the biomedical literature has many non-specialist readers, searching, at times desperately, for the latest medical research advance. While peer review does not guarantee correctness of published research, it does provide a deterrence to making extravagant claims that are not based on scientific evidence. This is particularly important in many areas that are of direct public concern. Over the years the growth of the literature of biology has been explosive. Journals have multiplied, grown thicker and glossier and, of course, become more and more expensive. A situation is now being reached where most institutions can no longer afford to subscribe and maintain full print collections. Electronic publishing promised to be a revolutionary solution, but the growing costs of electronic access are beginning to prove inhibitory. In this scenario, the birth of *PLoS Biology* (despite a name singularly lacking in felicity) is an event that may mark the beginning of a new approach to science publishing.

It is only three years ago that PubMed Central was launched by the National Institutes of Health in the United States, spearheaded by its then director, Harold Varmus. PubMed has grown into a wonderful electronic resource invaluable to biologists and medical researchers worldwide. However, only a limited number of journals permit access to text of papers or even abstracts, limiting PubMed's utility for accessing scientific literature. Varmus has now thrown his considerable scientific reputation and organizational talents into the launch of *PLoS Biology*. The first issue is an attractively produced journal which carries a range of cutting-edge papers spanning wide areas of modern biology. Who will pay for *PLoS Biology*? Free electronic access will necessarily reduce print subscriptions. Producing the journal will, of course, cost a great deal of money. For a short while the journal might be subsidized by private foundations; the war with profit-making private publishers is a politically correct cause. But, eventually the journal must pay its way. The founders of *PLoS Biology* suggest that authors must pay a publication fee of $ 1500 per paper; at prevailing exchange rates in India this would be approximately Rs 70,000, a very large sum of money by any standard. In assessing the challenges of open access publishing

Declan Butler suggests that this figure underestimates the costs 'of producing a journal of the highest quality', (*Nature*, 2003, **425**, 554). The PLoS initiative is an attempt to transfer the costs of publishing journals from readers to authors, operating on the assumption that organizations which fund research will also pay for its publication. To a limited extent this already happens with journals which levy page charges, although in most cases the rates cover only a limited portion of the true cost. Many journals charge exorbitantly for colour reproductions; presumably an attempt to recoup some costs from authors. But, at present, it is libraries who pay high subscription costs on behalf of their readers. The electronic revolution has complicated journal access issues; many times subscriptions for institutional electronic access are hugely expensive. Divesting print subscriptions has meant that journal access is limited to those who have accounts on institutional computers, converting libraries, which once were public resources into limited facilities catering to specific institutional needs.

The 'open access' war appears to have two major combatants. On one side are the white knights in shining armour, who seek to provide the results of science without any cost to readers. Ranged against them is the evil empire of science publishers; the flagship of this armada is the Reed Elsevier group based in Amsterdam, which offers expensive packages to libraries, including a number of indifferent journals, inextricably linked to a few key journals. The charge is that science publishers price their products exorbitantly, making profits out of publicly funded research and depriving readers who cannot pay, an opportunity to access research results. Scientists, of course, double as both authors and readers. Simple economics dictates that someone must pay the costs; a conclusion that is succinctly summarized in that wonderful American saying – 'there's no such thing as a free lunch'. Interestingly, journals run by scientific societies have often passed on the costs both ways; high subscription rates and page charges have operated simultaneously. The war has heated up with two biologists at the University of California at San Francisco circulating an e-mail calling for a boycott (refusal to submit and referee papers and to do editorial work) of the journal *Cell*, currently one of the most prestigious outlets for biological research. For years, prominent scientists have courted the editors of the best journals in order to showcase their results in the publications with the highest impact. And over a period of time the best journals have acquired a prestige and importance which has permitted their publishers to steadily raise the costs of access. It is hard to see scientists readily follow the move away from commercial publishers. The best known journals will still attract authors, who need to make their reputations. They will be more concerned with the impact factor of the journal where their work is published, than with issues of whether their article is accessible to those who cannot pay.

For researchers in India and the developing world the PLoS initiative may be fundamentally flawed. It will be nearly impossible to pay high publication charges from limited research grants. In the West, the Howard Hughes Foundation, the Wellcome Trust and the Max Planck Society are gearing up to absorb publication costs. Researchers in the Third World may eventually be left to beg for 'waivers', converting them into supplicants for largesse from 'open access' publishers. The situation is murky; as authors we might prefer commercial publishers, while as readers we might opt for the champions of 'open access'. For readers the difficulties of access may be overcome, as attempts to use the Internet to connect 'reprint archives', without copyright violation, become more widely usable.

The ongoing turmoil in the world of science publishing should catalyse some thinking on the state of journals in India. It might be useful for the Academies and the Council of Scientific and Industrial Research to take a hard look at their stable of journals and at the economics of publishing them. Our journals seem to cater more to the needs of authors; the nature of the readership is generally not an important issue. Several journals which are published with significant government subsidy are of indifferent quality and even more importantly, appear with a limited frequency, diminishing both their visibility and utility. The time may be opportune to make one more effort to revamp science journals in India, consolidating, pruning and restructuring, in order to enhance their competitiveness in the international scene. Issues of access will be irrelevant unless there is a concerted attempt at quality improvement.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Tenacity of Purpose

The 28 August issue of *Nature* carried an advertising 'highlight' on India. Five pages of paid advertisements, by some of our more prominent institutions announcing faculty openings and directorial positions, constituted this 'highlight'. The section contrasted sharply with a 'Spotlight on Singapore' which appeared in the 16 October issue, which carried a two-page preface highlighting the 'Biopolis of Asia', featuring a write-up on Singapore's biomedical research initiative. The page in the India spotlight that drew my attention was an attractively coloured advertisement which boldly proclaimed, *Push Back the Horizon*. The intriguing publicity blurb put out by the Survey of India had this to say: 'On 10 April 1802, in one of the most daring scientific quests known to humankind, Col. William Lambton began the longest measurement of the earth's surface. For the next fifty years, this inch-perfect survey of over 2400 km involved the most perilous of journeys, extremely complex mathematical equations, and the loss of more lives than in most contemporary wars'. In the true tradition of all government advertisements which attempt to highlight achievement, however distant, the photographs of ministers and administrators stared benignly at readers. Two centuries after he began his odyssey, William Lambton's portrait was curiously missing. His famous compatriot George Everest, who completed the 'Great Indian Arc of the Meridian' did not merit a mention, in an advertisement, which ironically seemed to celebrate an important phase in a scientific enterprise that 'significantly advanced our knowledge of the shape of the planet and laid the foundation of the science of geodesy'.

Last year when the Department of Science and Technology (DST) launched a year-long celebration of the bicentenary of Lambton's quest, considerable media attention was focused on The Great Arc (Ramachandran, R., *Frontline*, 10 May 2002; Nirupa Sen, *Curr. Sci.*, 2002, **82**, 780). Lambton began his historic survey at St. Thomas Mount in Madras (now Chennai) in April 1802, pressing for years through often inhospitable terrain, burdened by heavy equipment, including his famed 'Great Theodolite'. He died in harness on 20 January 1823 at Hinganghat (Maharashtra). He had 'in 21 years on the survey completed 165,342 square miles of triangulation at a cost of £83,537' (Smith, J. R., *Everest*, Whittles Publishing, Scotland, UK, 1999, p. 39). Lambton's successor George Everest pushed the task of practically completing the Great Arc, in a remarkable feat of uncompromising commitment over the next two decades; retiring from the Survey of India in 1843. His successor Andrew Waugh was to extend the task of 'triangulation' into the Himalayas. In the period between 1852 and 1856 it became apparent that Peak XV was a particularly high mountain. Computations were the key to estimating precise heights from field measurements. Historians credit the 'chief computer' for the survey, Radhanath Sikdhar, for the discovery of the highest peak. Waugh was to name the highest mountain on earth, Mount Everest in 1856, a year before the Sepoy Mutiny of 1857 transformed British rule in India from indirect administration through the East India Company to direct rule by an imperial power. In the period that led up to direct rule, the survey of the subcontinent was the first step in 'the development of its infrastructure'; a phrase that appears in the recent advertisement in *Nature*.

Lambton and Everest were explorers and scientists, who displayed amazing commitment to the task of the survey of India. Their fanatical adherence to the principle of precise and uncompromising measurement makes the saga of the Great Arc one of the epic achievements of science. The survey did have its moments of discord; most notably the 'Jervis Affair' in which Everest, sidelined briefly by illness, cast doubts on the quality of the survey conducted by Thomas Best Jervis, appointed by the Directors of the East India Company as his successor designate. In the controversy that followed, Everest was ranged against a large section of the Royal Society. His biographer J. R. Smith notes that a letter signed by the Duke of Sussex, the then President of the Royal Society, and 38 Fellows (including Michael Faraday) dated 14 July 1838, appeared to be supportive of Jervis. Everest, himself a Fellow, was 'justifiably incensed'. Everest's interest in mathematics and his passion for geometry appears to have been kindled in India. The letters of his niece Mary Boole, reproduced in his biography must specifically interest Indian readers. In an article entitled 'Indian Thought and Western Science'

she says: 'I end as I began. Tell Hindus to read De Morgan's preface to Ram Chundra. Tell them it is the voice of Mount Everest calling to India to awake and arise, and recover the treasures of its past'. The reference here is to a book entitled *A Treatise on Problems of Maxima and Minima* by Ram Chundra published in 1859 by W. H. Allan & Co. London from an edition published earlier in Calcutta in 1850 (J. R. Smith, pp. 162–163). The survey was a major step forward in cartography; it was also a decisive step in the colonization of India.

In considering the life and times of William Lambton and George Everest, I was reminded of the opening lines of Philip Mason's *The Men Who Ruled India* (Rupa Publishing, New Delhi, 1992, 2001), a compellingly written overview of Britain's involvement with India; 'There comes a time in a man's life when he may well stand back and consider what he has built, planted, written or begotten and whether it was worth doing. If in such a mood the English – and by that I mean all those who speak English: Chaucer and Drake, Milton and Marlborough, Clive and Hastings, belong to us all – if the English look back on their varied history, the long connection with India will be an achievement that cannot be ignored'. Mason, as so many before and after him, wonders how so many could be ruled so decisively by so few. In his words, his countrymen 'mastered and ruled so many millions by the sword, by diplomacy, above all by a stubborn tenacity of purpose' (p. xi). Mason notes that the 'rulers of India were not cast in a mould; they were men, quick with fleshy desire, lust for power, and all the miraculous diversity of man; humourous, solemn and unpredictable; adventurous, soaked in routine, timid and bold. Yet they have something in common. Nearly all of them – after the transformation – believe that the performance of duty is something good in itself; hardly one questions the value of his work' (p. xiii). The unquestioning commitment and stubborn persistence displayed by many Englishmen in the first half of the 19th century, Lambton and Everest amongst them, paved the way for the total dominance that was to follow.

In celebrating Lambton and the Great Arc, the DST and its subsidiary organization the Survey of India have curiously chosen an event from our colonial past, to showcase India's science and technology. Apart from adding to *Nature's* already considerable advertising revenues, such attempts at publicity reflect a lack of professionalism amongst those who seek to project India's image as a country, with a substantial base in science and technology. The Survey of India's sense of history seems oddly misplaced on the pages of *Nature*; the photographs of politicians and bureaucrats providing a tasteless backdrop. In view of the increasingly frequent attempts to blame many of our present day ills on colonizers of the last few hundred years, the celebration of Lambton's survey on the pages of *Nature* appears ill conceived.

The attempts to promote science and technology that emanate from our Ministries seem to be strangely out of place. The year 2003 has witnessed many public and expensive celebrations of the 50th anniversary of the DNA double helix, including a major event in Delhi. Such historical anniversaries are best marked by scientists in the seminar rooms of their institutions or the pages of their journals. They are hardly cause for public national celebration, particularly when the connection to India, its science and its scientists is limited or non-existent. Ministries and administrative departments which manage science would do well to focus on the present and take a hard, pragmatic and clear-headed look at the conditions that prevail in the institutions under their charge. A true tribute to Lambton and Everest, the first of the 'surveyors' of India would be to assess the present state of many of our 'survey' departments; the Survey of India, the Geological, Botanical and Zoological Survey departments. We might project their successes of the past half a century and introspect on their failures. We need to focus on reviewing the functioning of many of our institutions and to chart a strategy for showcasing them as attractive places for a new generation of enthusiastic individuals to work. If there is one lesson to be learnt from Lambton, Everest and the Directors of the East India Company that supported them, it is this: tasks once identified, however difficult, must be pursued with unswerving commitment and, to borrow Philip Mason's phrase, a tenacity of purpose.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Synthesizing Life

'I stated that by the action of cyanogen on liquid ammonia, besides several other products, there are formed oxalic acid and a crystallizable white substance, which is certainly not ammonium cyanate, but which one always obtains when one attempts to make ammonium cyanate by combining cyanic acid with ammonia e.g. by so-called double decomposition. The fact that in the union of these substances they appear to change their nature, and give rise to a new body, drew my attention anew to this subject, and research gave the unexpected result that by combination of cyanic acid with ammonia, urea is formed, a fact that is noteworthy since it furnishes an example of the artificial production of an organic, indeed a so-called animal substance, from inorganic materials.'

Friedrich Wöhler
Ann. Phys. Chem., 1828, **88**, 253

'The chemical synthesis of the viral genome combined with de novo cell-free synthesis has yielded a synthetic virus with biochemical and pathogenic characteristics of poliovirus. In 1828, when Wöhler synthesized urea, the theory of vitalism was shattered. If the ability to replicate is an attribute of life, then poliovirus is a chemical, $C_{332,652}H_{492,388}N_{98,245}O_{131,196}P_{7501}S_{2340}$ . . . , with a life cycle.'

J. Cello, A. V. Paul and E. Wimmer
Science, 2002, **297**, 1016

'The capabilities of DNA synthesis have lagged far behind our ability to determine sequences during the past 30 years. If this gap can be closed, then limitless possibilities for the application of synthetic methods to the study and practical application of genomics will emerge. . . . Synthetic genomics will become commonplace and provide the potential for a vast array of new and complex chemistries, altering our approaches to production of energy, pharmaceuticals and textiles.'

H. O. Smith *et al.*
Proc. Natl. Acad. Sci. USA, 2003, Dec. 2

Wöhler's urea synthesis marked a decisive turning point in the history of chemistry. The laboratory production of a substance that had thus far been associated exclusively with living organisms breached the barrier between the animate and the inanimate. Wöhler and, a few decades later, Pasteur delivered the death blows to the theory of vitalism. No mysterious 'life force' appeared to be necessary for maintaining living cells; chemistry and biochemistry seemed sufficient. Wöhler was exuberant when he wrote of his experiments to Jöns Jacob Berzelius: 'I can no longer as it were, hold back my chemical urine; and I have to let out that I can make urea without needing a kidney, whether of man or dog; the ammonium salt of cyanic acid is urea'. Berzelius had, a year earlier, in 1827 noted in his *Textbook of Chemistry*: 'In living Nature the elements seem to obey entirely different laws than they do in the dead. . . . The essence of the living body consequently is not founded in its inorganic elements, but in some other thing, which disposes the inorganic elements. . . to produce a certain result, specific and characteristic of each species' (Hunter, G. K., *Vital Forces*, Academic Press, 2000, p. 56). We know today that urea is an exceedingly simple substance; its chemical formula CH_4N_2O would not appear intimidating even to a high school student. But even in Wöhler's time urea was not regarded by some as a 'real organic molecule', with Johannes Muller sounding magisterial in his dismissal: 'Urea, however, can scarcely be considered as organic matter, being rather an excrement than a component of the animal body'.

In the century after Wöhler, chemistry and life remained resolutely separate, with the rapid and divergent growth of the disciplines of organic chemistry and biochemistry. At times it almost seemed that any discussion of living organisms and life must have been banned in departments of organic chemistry, in response to edicts issued by the high priests of the discipline. The chemical synthesis of proteins, nucleic acids and oligosaccharides was not an activity looked upon with favour by traditional departments of organic chemistry; the monotonous chemistry of the biopolymers did not seem to afford a sufficient intellectual challenge for the synthetic chemist. The formidable technical obstacles to the synthesis of biopolymers were dramatically overcome in the early 1960s, with the introduction of solid phase synthesis, first applied to peptides and proteins by R. B. Merrifield. DNA synthesis was achieved in a monumental effort by Har Gobind Khorana, with the first gene synthesis reported in the late 1970s. The introduction of solid phase procedures and

automation brought the synthesis of oligonucleotides, small DNA fragments, to molecular biology laboratories by the 1980s. But the methods have been restricted to assembling individual genes, by enzymatic joining (ligation) of synthetic nucleotide fragments. Genomes, which contain several genes strung together in sequence provide a more formidable synthetic challenge. If 'life' must be synthesized, then genomes which provide the blueprint for an organism's chemistry are the target of choice.

One hundred and seventy four years after Wöhler, in a chemical *tour de force*, Eckard Wimmer and his colleagues at the State University of New York, Stony Brook reported the synthesis of poliovirus complementary DNA and the successful generation of an infectious viral particle in the 'absence of any natural template'. The genome length of ~ 7500 nucleotides may be compared with the first gene synthesized in the late 1970s, which had a length of 207 nucleotides. While the methodologies for gene assembly have been in place for some years, the synthesis of the poliovirus demonstrates that a virus capable of replication, and therefore a life form, can be put together by laboratory procedures without the intervention of any native viral DNA (Cello, J. *et al.*, *Science*, 2002, **297**, 1016). In a more recent report Craig Venter and his colleagues report the construction of a synthetic genome of the bacteriophage phiX174, which is a virus capable of infecting the common bacterium *E. coli*. The assembly of a stretch of 5386 nucleotides was achieved in a remarkable time of 14 days; improved methodologies enhancing the fidelity of synthesis. Once again infective viral particles could be generated. These two examples suggest that methodologies now exist for the assembly of larger genomes, permitting the laboratory creation of organisms with 'minimal genomes'. Chemistry and biochemistry have now seamlessly integrated to permit the synthesis of 'life'.

Viruses live in a shadowy no-man's land on the borders of chemistry and biology. The simpler viruses, phiX174 and poliovirus among them, consist of relatively simple protein coats packaging small genomes, which possess the instructional capability for recruiting host machinery for viral replication. Many viruses, including the poliovirus have been crystallized and their atomic arrangements laid bare by X-ray diffraction, a technique that had its first major success in the early years of the 20th century when the Braggs, father and son, described the structure of sodium chloride. Viruses are not free-living creatures but need the resources of a host organism to multiply; nevertheless they are undoubtedly the simplest collection of chemicals that possess some of the many attributes of life. The work of Wimmer, Venter and their colleagues is a tribute to the robustness of the methodologies of chemistry and biochemistry, which have now brought us to the threshold of the synthesis of small genomes. Chemical synthesis, with assistance from enzymatic ligation and the polymerase chain reaction appears to be a viable strategy for assembling genomes in the laboratory, raising possibilities that have so far been confined to the realm of science fiction. Critics have been quick to attack the Wimmer paper on the polio virus; arguing that similar methods may be used to put together dangerous viruses as weapons in the area of biological warfare (bioterrorism, seems a more widely used word, nowadays). Some biologists have gone so far as to suggest restrictions on the availability of viral genome sequences; a knee-jerk reaction which may hamper research in virology rather than protect dangerous exploitation of sequence information. Interestingly, the poliovirus paper was a blandly written technical report that offered no real justification for the choice of a human pathogen and made no reference to any ethical questions that may accompany this undoubted advance of scientific methodology. In the phiX174 paper that followed, Venter and his colleagues note: 'Prior to attempting synthesis of a microbial chromosome we commissioned an independent bioethical review of our proposed scientific plan. After more than one year of deliberation the reviewers concluded that we were taking a reasonable scientific approach to an important biological question. The broader implications of the creation of life in the laboratory can now be considered as a realistic possibility' (Smith, H. O. *et al.*, *Proc. Natl. Acad. Sci. USA*, 2003, Dec. 2). In the ethical review report entitled 'Ethical considerations in synthesizing a minimal genome', the authors note that 'the attempt to model and create a minimal genome represents the culmination of a reductionist research agenda about the meaning and origin of life that has spanned the 20th century'. They conclude decisively: 'The prospect of constructing minimal and new genomes does not violate any fundamental moral precepts or boundaries, but does raise questions that are essential to consider before the technology advances further' (Cho, M. K. *et al.*, *Science*, 1999, **286**, 2087).

Two complementary approaches to creating minimal life forms in the laboratory have emerged over the last few years. Downsizing an existing genome, a 'top-down' approach by large scale gene knock outs is already on the cards. The 'bottom-up' strategy with genome synthesis as the starting point is illustrated by the work on poliovirus and phiX174. But there is a long way to go before 'life' in its fullest form can be created artificially. The backdrop of evolution and selection makes living organisms an incredibly formidable target for chemical assembly. When asked to comment on origins of life research, Lynn Margulis was forthright: 'Biochemical systems are effectively historical accumulations. So I don't think there is ever going to be a packaged recipe for life; add water and mix and get life' (Horgan, J., *The End of Science*, Broadway Books, New York, 1996, p. 140). Whole genome synthesis promises a new approach. The 21st century may witness tumultuous advances on this front, paralleling the changes that began in the 19th century with Wöhler's serendipitous synthesis of urea.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Conferences: Unrestrained Proliferation

The period between November and February is a generally pleasant and agreeable season in India. Even the cold, and sometimes foggy, days of winter in North India are mild by comparison with the harsher climes of Europe and North America. In India, it is tourist season and also the time for conferences. Bangalore's winter is always wonderfully pleasant and conferences follow one another. The Indian Institute of Science has in quick succession seen several hundred molecular biologists attend the congress of the Federation of Asian and Oceanian Biochemistry and Molecular Biology (FAOBMB), followed by the meeting of the American Mathematical Society, which established that mathematicians can rival biological scientists in their numbers and diversity. Scientific conferences have been rapidly increasing in number, spreading to exotic locations worldwide, making science a wonderful profession for those who love to travel. In thinking about conferences, two gatherings of scientists, which contributed immeasurably to the shaping of science in the 20th century, immediately spring to mind. The Solvay Conferences, held in Brussels beginning in 1911, transformed physics and science. The conference photographs which feature the immortals of modern physics are now a part of history. The 1911 meeting chaired by Hendrik Lorentz formalized the ongoing transformation of physics, set in motion by Einstein's famous papers of 1905. Over the next two decades, the Solvay conferences were the stage for showcasing (a modern word) the succession of revolutionary changes in physics. By 1927, Heisenberg and Born could use the conference to triumphally declare: 'We regard quantum mechanics as a complete theory for which the fundamental physical and mathematical hypotheses are no longer susceptible of modification'. The other remarkable meeting ground for scientists was the summer course at Cold Spring Harbor, initiated by Max Delbrück in 1945. Delbrück, was a physicist whose conversion to biology was catalysed by Niels Bohr's lectures on 'Light and Life' and 'Biology and Atomic Physics' delivered, at Copenhagen and Bologna, in the 1930s. As one of the discipline's most influential chroniclers notes: 'Through Delbrück, Bohr's epistemology became the intellectual infrastructure of molecular biology, the reason, perhaps for its hegemony over twentieth century life sciences. It provided for molecular biologists the philosophical guidance for navigating between the Scylla of crude biochemical reductionism, inspired by nineteenth century physics, and the Charybdis of obscurantist vitalism, inspired by nineteenth century romanticism' (G. S. Stent in the Introduction and Overview to *Mind from Matter*, M. Delbrück, Blackwell Scientific Publication, Palo Alto, 1986). Delbrück's remarkable presence ensured that the Cold Spring Harbor meetings were as influential in spreading the revolution in molecular biology as the Solvay conferences were in physics. There are many other influential and exclusive scientific meetings, which have played a role in shaping the course of scientific research; probably the most widely known among these are the Gordon Conferences, held traditionally in New England; although an inevitable sign of the times is the expansion to winter meetings in California.

Conferences and meetings, seminars and symposia, congresses and conventions have all played a critically important role in the growth and development of science. In India the number of scientific meetings and societies has been growing. International conferences are held in India with increasing frequency; the winter months seem to be becoming excessively crowded with overlapping meetings. Many conferences have become much too large, with attendance running to several hundred people, with figures as high as several thousand, sometimes. The last meeting of the Society of Neuroscience in New Orleans is stated to have been attended by 28,000 participants. I chanced, recently, to overhear an amusing conversation between two visitors from America:

Neuroscientist (*proudly*): Do you know that neuroscience is exceedingly important? The recent neuroscience meeting in the US was attended by over 20,000 people.

Molecular biologist (*archly*): Yes. I believe there are more neuroscientists than plumbers in the US. It is possible that many of the plumbers have turned to neuroscience.

It is debatable whether very large meetings serve any purpose; but they do provide a forum for students and early

career researchers to meet, hear, and on occasion, have extended discussions with established scientists in their own fields. Organizing large meetings in India has become quite common with a new and growing breed of 'organizers', who have the ability to generate the resources necessary for these meetings. Typically, the large international meetings in India focus an excessive part of the energies of their organizers on the inauguration, the obligatory 'cultural program', the 'banquet' and in making arrangements for 'prima donnas', who appear as plenary speakers. The practice of having elaborate inaugurals complete with politicians and long speeches is spreading in India, leaving many of the participants nonplussed even before the scientific sessions commence. The larger the conference, the more bewildering is the mix of parallel sessions and overcrowded poster presentations. For habitual conference attenders there is a sense of *déjà vu*; many speakers and posters seem familiar, the science stale and repetitive. Greater solace is then to be found outside the conference, converting many of our large meetings into organized tourist festivals.

Conferences in India are also becoming steadily more expensive, restricting student participation from all except the well funded national institutions. For meetings organized in conjunction with international scientific unions, concessions in registration fees, for local participants, are frowned upon. Organizers then resort to elaborate artifices to subsidize those who find it difficult to pay the exorbitant and at times, unnecessarily high registration charges. In most meetings, the scientific content has little bearing on the costs of the meeting, with the scientific program being put together almost as an afterthought. There is a large influx of non-resident Indians for symposia held in India in winter; a welcome opportunity to combine a visit home with a scientific meeting. There is another set of conferences, exclusive and generally held at exotic locations; Goa, Sikkim, the Kerala beaches, the Andamans and varied resorts and hillstations across the country are favoured destinations. Here participation is limited to small clubs and academies; the purpose ostensibly to imitate the famous conferences of yore, which advanced science, without the distractions of the presence of large and inexpert audiences. The irony of these expensive meetings is that many of the most regular participants are senior scientists, who have long abandoned the mundane routines of day to day research and others who show little interest in academic discourse within their own institutions.

Conferences and meetings would not be an issue for discussion if the quantum of public funds expended were modest and the participation net cast wide. Unfortunately, at present we seem to have too many conferences, which cost substantial sums of money. The proliferation of conferences parallels the multiplication of scientific societies and academies. This growth of scientific bodies is, of course, a worldwide phenomenon. A cursory glance at the yearbook of the Indian National Science Academy (INSA) reveals that this organization interfaces, on behalf of the country, with as many as 25 international bodies. Many of these, from their names, appear to span overlapping fields. The International Union of Biochemistry and Molecular Biology (IUBMB) and the International Union of Biological Sciences (IUBS) presumably share common interests. To an uninitiated observer, distinctions between the International Union of Geodesy and Geophysics (IUGC), International Union of Geography (IGU) and International Union of Geological Sciences (IUGS) are not readily apparent; but their existence must undoubtedly facilitate more conferences. Following the cue from international bodies, the number of scientific societies in India has grown alarmingly. Biology is probably the most dynamic discipline with societies that cater specifically to biological chemists, cell biologists, microbiologists, reproductive biologists, biophysicists and many more. A curious feature of our proliferating societies is that many times we have scientists of eminence, who preside at different times over different bodies. Presumably the societies must then have a considerable overlap of disciplinary interests. This fragmentation of scientific bodies for reasons which defy comprehension, is best exemplified by our science Academies. They are growing in number, with several specialist academies sprouting up. Most often, the main purpose of academies seems to be to 'elect fellows' and to hold annual (and sometimes, biannual) meetings. As academies and societies proliferate, so too do conferences.

I must hasten to add that conferences are a wonderful medium for scientific discourse. However, we might pause and wonder if their proliferation is not a cause for worry. There is the danger of indigestion, both intellectual and otherwise from a surfeit of conference dinners.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Sivaraj Ramaseshan

When I go from hence let this be my parting word,
that what I have seen is unsurpassable.
I have tasted of the hidden honey of this lotus
that expands on the ocean of light, and thus
am I blessed – let this be my parting word.
In this playhouse of infinite forms
I have had my play and here
have I caught sight of him that is formless
My whole body and my limbs have thrilled
with his touch who is beyond touch;
and if the end comes here, let it come—
let this be my parting word.

Rabindranath Tagore, *Gitanjali*
Translation by the author from the
original Bengali, UBSD Publishers, 2003

Sivaraj Ramaseshan, editor of this journal and one of India's most accomplished scientists, died in Bangalore on 29 December 2003, at the age of 80. The end came after a long and valiant struggle against failing health. In Ramaseshan's passing this journal has lost its most devoted champion and Indian science one of its most distinguished practitioners. In a career spanning over half a century Ramaseshan carried out front-line research in physics and materials science. Optics, crystallography, and high pressure physics were among the disciplines where Ramaseshan made decisive and influential contributions. But he was far more than a researcher lost in the technicalities of his chosen discipline. Ramaseshan was a builder of groups, institutions, scientific organizations and academies. He was the prime architect of a movement to enhance the prestige, visibility and content of scientific journals published from India. He was among the foremost popularizers of science in this country. To every sphere of his diverse activities Ramaseshan made a distinctive and lasting contribution.

Ramaseshan was incurably romantic about science. Growing up in the heady days of India's freedom movement and entering research in the early 1940s under the tutelage of his legendary uncle, C. V. Raman, was to make an indelible impact on the youthful Ramaseshan. Many years later, in 1978, while delivering the first C. V. Raman Memorial Lecture at the Indian Institute of Science he recalled: '*To Raman, scientific activity was the fulfillment of an inner need. His approach to science was one of passion, curiosity and simplicity. It was an attempt to understand. To him science was based on independent thought, combined with hard work. Science was a personal endeavour, an aesthetic pursuit and above all a joyous experience.*' In full measure, these words describe Ramaseshan's approach to science. Ramaseshan was also greatly influenced by two of the great figures of 20th century science, the English crystallographer Dorothy Hodgkin and the astrophysicist Subrahmanyan Chandrasekhar. In 1990, writing in *Current Science* he quoted Chandrasekhar: '*The pursuit of science has often been compared to the scaling of mountains, high and not so high. But who amongst us can hope even in imagination to scale the Everest and reach its summit when the sky is blue and the air is still, and in the stillness of the air survey the entire Himalayan range in the dazzling white of the snow stretching to infinity? None of us can hope for a comparable vision of nature and of the universe around us. But there is nothing mean or lowly in standing in the valley below and awaiting the sun to rise over Kinchinjunga.*' Ramaseshan was thrilled by the touch of science and to the very end it was talk of science and scientists that made him sparkle, even when the ravages of illness had begun to take their toll.

Ramaseshan's academic career began at the Indian Institute of Science, Bangalore, where he worked for his doctoral degree and also as a member of the faculty of the physics department in the 1950s and early 1960s. He was to return nearly two decades later to serve as the institution's Joint Director (1979–81) and Director (1981–84). In the intervening years, he established the physics department at the Indian Institute of Technology, Madras (now Chennai) and the Materials Science Division at the National Aeronautical Laboratory (NAL, presently the National Aerospace Laboratories). The world-class materials program built by him at NAL has been a critical element in the development of indigenous capabilities in aerospace research. His personal research contributions, most notably his early work on anomalous dispersion

methods in crystallography and his later research on condensed matter physics and materials, were characterized by deep physical intuition and a unique ability to see the heart of a problem. But, most remarkably, Ramaseshan was to make the most significant contributions to many institutions, at a time when his formal academic career drew to a close. He collaborated closely with M. S. Valiathan and his colleagues at the Sree Chitra Tirunal Institute for Medical Sciences and Technology, Thiruvananthapuram in the development of the Chitra heart valve, a triumph of multidisciplinary research in India. He was also closely involved in the successful transfer of technology for blood bag production, developed at this institute. In 1984, Ramaseshan retired from the Directorship of the Indian Institute of Science, to begin a remarkable post-retirement phase.

In 1973, Ramaseshan was the motivating force in launching the physics journal, *Pramana*, published by Indian Academy of Sciences. He noted in an editorial: 'The publication in foreign journals of the major part of the work done in India today is having a deleterious effect on Indian science. Relegating the refereeing of our best scientific work leads to loss of judgement and self-confidence. This process has sapped the inner resources of Indian scientists and, among other things has led them to follow blindly fashions set elsewhere in choosing fields of work'. In its early years *Pramana* attracted some of the best physics papers emanating from India. In 1978, he wrote at length on the problem of scientific journals in India. In a paragraph that will find many echos today, he asked: 'Can our journals help to produce an intellectual ferment so that the highest quality of science is produced in this country?' He added: 'This can be done only if the culture of science is made to percolate deeply into the community. Only then can this metamorphosis take place. Can we help to revive our dying universities? Can we help our bright young students to pursue the excitement of creative science?'

In 1989, Ramaseshan took over the editorship of *Current Science*. This journal, founded in 1932, had an illustrious past as a widely read interdisciplinary science journal. But it had fallen on hard times. Diminished circulation and decreasing visibility in India were problems that Ramaseshan had to contend with. He brought to the task of rejuvenating *Current Science* a remarkable sense of purpose and boundless optimism. As 1988 drew to a close, I entered my somewhat chaotic laboratory not noticing that a student of mine was engaged in conversation with a visitor. She came to my desk a few minutes later to say that there was a senior gentleman waiting for me. This was Ramaseshan. He had conducted a conversation with my student, not revealing that he was a former Director. His mission was to recruit researchers and students to the task of rejuvenating *Current Science*. He charted a course that took this journal from a period of uncertain production and finances to its present state. He was an indefatigable solicitor of manuscripts and financial support. He wrote at length in the 'In this issue' pages, which were intended to highlight the journal's contents. When the contents were forgettable or obscure, Ramaseshan digressed, drawing on his formidable memory for people and events. He organized special sections and wrote persuasively and entertainingly on men and matters. He had a ringside view of the development of science in India over a period of more than half a century and at times he produced incisive portraits of this country's most influential scientists – Raman, Chandrasekhar, Harish Chandra, Bhabha, Sarabhai, Mahalanobis among them.

In reflecting on the life and times of Ramaseshan, I am drawn inevitably to two of his great contemporaries, G. N. Ramachandran (1922–2001) and Satish Dhawan (1920–2002). For a brief period, in the transition years between the 1970s and 1980s, all three were at the Indian Institute of Science. They were three entirely different personalities, but they all bore the indelible stamp of greatness, collectively contributing to the growth of our science and our institutions. With Ramachandran, Ramaseshan shared deep interests in optics and crystallography. With Dhawan, he shared an abiding commitment to building and fostering the institutions on which this country's progress depends.

When C. V. Raman died in 1970 he left three institutions: the Raman Research Institute, the Indian Academy of Sciences and the Current Science Association—all of which he had presided over for long periods of time. In the years that followed, Ramaseshan, more than anyone else, ensured that these organizations acquired a new and distinctive character, which would ensure a bright and robust future. Ramaseshan served on the councils and governing bodies of these organizations for long periods of time, with great devotion to their welfare. In early 1985, Ramaseshan presided over the Academy's golden jubilee meeting where he spoke of quasi-crystals and Penrose tiling. It was a memorable event at Bangalore's Chowdiah Hall, with Subrahmanyan Chandrasekhar, fresh from his Nobel Prize, reflecting in his inaugural lecture on 'The Pursuit of Science: its Motivations' followed in the evening by a concert by the incomparable M. S. Subbulakshmi.

Ramaseshan was an outstanding lecturer and conversationalist. His self-deprecatory humour and ability to laugh at himself were engaging. A photograph of the audience during his farewell speech when he retired as the Director of the Indian Institute of Science shows a large section of the audience laughing; clearly even formal occasions were lightened by his wit. Sivaraj Ramaseshan lived a full life contributing greatly to his surroundings. Those who knew him personally were immeasurably enriched. We share with his family a great sense of loss. We shall miss him and cherish our memories of him.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Style, hyphens and the split infinitive

Authors of scientific papers always await referees' reports with trepidation. Nothing can be as disheartening to authors as a summary rejection of their manuscripts by editors, who often base their decisions on devastatingly worded comments from 'peer reviewers'. Many referees seem to delight in dismissive language; anonymity provides an impenetrable shield. Sometime ago, I received referees' reports on a paper that I had submitted to a journal published from the United States. The editor's covering letter seemed promising, requesting the inevitable revision. But one of the reviewers, while grudgingly accepting that the mundane science described in the paper was publishable, went on to launch a vigorous attack on my language. He (and here I speculate on the sex of the referee) charged me with 'violating' two of his 'pet peeves'. First, he came down heavily on my use of the 'split infinitive'. Second, he was pained by my use of 'nouns as adjectives to modify nouns that are used as adjectives without hyphens'. He did add, somewhat condescendingly that it was all a matter of taste, implying clearly that while we might even agree on matters of science, our differences on points of grammar were irreconcilable. Upon re-reading my own manuscript I realized that I could neither recognize split infinitives, which were undoubtedly strewn all over the paper, nor could I decide on the location of the missing hyphens. Stung by the criticism I refrained from submitting a revised version of the paper, waiting for an opportunity to clear my increasing doubts about my own grammar. At one of the symposia where booksellers display their wares, I came across a copy of *Scientific English* by Robert Day (Universities Press, 2000). Reading a book on writing scientific papers is not easy. But, Day's preface was both encouraging and entertaining. He quoted both Plato and Confucius. The former had said: 'Beauty of style and harmony and grace and good rhythm depend on simplicity'. The latter was characteristically brief: 'In language clarity is everything'. A little further on Day came to my rescue rather forcefully: 'I have good news. You *may* split infinitives. In fact, you may, on occasion, violate every one of the "rules" dreamed up by generations of grammatical fussbudgets. ... The obvious purpose of grammatical rules is to facilitate clear communication. When rules of grammar do not serve this purpose, they should be disregarded' (p. xi). On the hyphen, Day was equally encouraging: 'The hyphen has a number of uses, most of them confusing' (p. 92). For good measure he concludes by quoting John Benbow: 'If you take the hyphen seriously, you will surely go mad'. Even *Fowler's Modern English Usage* (ELBS and Oxford University Press) is harsh on the hyphen, quoting Winston Churchill's famous dictum: 'One must regard the hyphen as a blemish to be avoided as far as possible'. In protesting against the hyphen, Churchill argued 'that you may run them together or leave them apart, except when nature revolts'.

Indian scientists (and undoubtedly scientists from many non-English speaking parts of the world) are often admonished by British and American referees: 'The authors should have their manuscripts read by a *native* English speaker'. This kind of comment is received even when the language is passable, often motivated by a casual reading of the authors' address, rather than by an analysis of the manuscript's grammar. Despite my current aversion for grammar, stimulated by a disagreeable referee, I must confess that as a reader and editor I often wish that many authors paid some attention to writing style. There is nothing more disheartening for an editor than to receive a letter for publication, which is completely incomprehensible. Summary rejection of such contributions seems unfair; improving them by editing seems impossible. This journal's office appears to be receiving an increasing number of manuscripts, which are very poorly written, carelessly proof-read and often display a complete disregard for the suggested format. After years of glancing through manuscripts in diverse fields of science, I have reached the inescapable conclusion that compulsory instruction on writing scientific papers might be an invaluable exercise for research students. The habit of consulting dictionaries and style guides is to be encouraged and analysis of the text of published papers, for the manner of their construction, might be helpful. Interestingly, even as I was thinking about grammar and style a book entitled *Communicating in Style* by Yateendra Joshi was received in our office for review. This reasonably sized (about 250 pages) and moderately priced (Rs 300) book, published by The Energy and

Resources Institute (New Delhi, 2003), appears to have been generated for in-house use. But, this book is not for those interested in clarifying the nuances of grammar; it seems more directed towards those interested in presentation. Curiously, the cover carries a blurb attributed to John le Carré: 'A gem. Courteous, unfrightening and essential. A perfect companion to Fowler's *A Dictionary of Modern English Usage* for today's communicators'. I would prefer Fowler and common sense. A book more directly addressed to scientists is *The ACS Style Guide* (Dodd, J. S., ed., *American Chemical Society*, 1997). Although this book focuses primarily on chemistry, it would undoubtedly be useful to writers in all branches of science. Most satisfying, I found both the split infinitive and the hyphen discussed. The ACS committee decreed: 'It is acceptable to use split infinitives to avoid awkwardness or ambiguity'. Hyphens are elaborately treated, as the literature of chemistry is replete with broken words; scanning though long lists of 'unit modifiers' (two words that together describe a noun, e.g. electron-diffraction, excited-state) I could not help but feel that the ACS had ignored Churchill's dictum. The *ACS Style Guide* is the kind of book that ought to be lying around in our laboratories, in the hope that students may idly turn its pages and in the process come across useful nuggets for improving the style and presentation of papers. There are sections in this book which go beyond writing style, providing useful tips on copyright matters and oral presentations of science. In these days of facile internet access and rapid 'downloads', many seminars are delivered in which figures, data and even animated movies of real and imagined interactions of atoms, molecules, cells and organisms are projected. Most often, there are no references to the site from which the illustrations have been taken. Computers, the 'world wide web' and 'Powerpoint' have made plagiarism of sorts a routine event in many presentations, both oral and written.

While the paper written for a professional scientific journal can get away with a significant degree of obscure language, articles addressed to a more general audience must be clearer. This journal, for instance, has a section entitled 'general articles', which should hopefully be accessible, at least in some part, to a non-specialist reader. Unfortunately, most submissions in this category are manuscripts which are unintelligible; their sole claim to being 'general' is the absence of original research normally found in a scientific paper and the lack of scholarship that one associates with a 'review'. In an essay on *How to write a popular scientific article*, J. B. S. Haldane provides compelling advice: 'The first thing to remember is that your task is not easy, and will be impossible if you despise technique. For literature has its technique, like science, and unless you set yourself a fairly high standard you will get nowhere.... You must ... know a very great deal more about your subject than you put on paper. Out of these you must choose the items which will make a coherent story'. For articles that do not have to be published immediately, Haldane has an interesting prescription: 'When you have done your article give it to a friend, if possible a fairly ignorant one. Or put it away for six months and see if you still understand it yourself. You will probably find that some of the sentences which seemed simple when you wrote them now appear very involved. Here are some hints on combing them out. Can you get in a full stop instead of a comma or a semicolon? If so, get it in. It gives your reader a chance to draw his breath. Can you use an active verb instead of a passive verb or verbal noun? If so, use it' (*On Being the Right Size*, J. B. S. Haldane (ed. Maynard Smith, J.), Oxford University Press, London, 1985).

The growing technical complexity of the scientific literature makes even 'reports of research focused on an outstanding finding whose importance means that it will be of interest to scientists in other fields', impossible to comprehend at first glance. I took the above quote from *Nature*'s guide to authors (cf 2004, **427**, 84). The provocation was my attempt to read a letter entitled 'Bcl10 activates the NF-κB pathway through unbiquitination of NEMO' (Zhou *et al.*, *Nature*, 2004, **427**, 167). Undoubtedly important, the paper illustrates the difficulty that non-specialist readers face in reading papers in many disciplines of science. Abbreviations and acronyms abound. Some have been introduced into the literature to satisfy a momentary humorous impulse of an author. NEMO took me back to Jules Verne and *Twenty Thousand Leagues Under the Sea*. The literature of molecular biology is full of genes and mutants with curious names—*hedgehog*, *sevenless* (and, of course, *son of sevenless*) and even an apparently familiar, *laloo*. Biology's literature is rapidly becoming as intractable as chemistry, with nomenclature providing an impassable obstacle for a general reader.

Writing style is, of course, a 'matter of taste' as my referee correctly pointed out. Grammar can serve as a guide, not a shackling constraint. Ultimately, a published paper must be useful. For this, it must be understandable. A little attention to style and technical crafting while writing a letter, a paper or a report would help readers (and copy editors). Reminding authors of the rules of grammar, on occasion, may be valuable. I, for one, will look with respect at both the hyphen and the split infinitive, in future.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Biophilia and Chemiphilia

Modern biology has produced a genuinely new way of looking at the world that is incidentally congenial to the inner direction of biophilia. In other words, instinct is in this rare instance aligned with reason. The conclusion I draw is optimistic: to the degree that we come to understand other organisms, we will place a greater value on them, and on ourselves.

Biophilia, E. O. Wilson
Harvard University Press
Cambridge, Massachusetts 1984, p. 2.

It may not going too far to suggest that the appeal of the abstract logic of organic chemistry, closely akin to that of Euclidean geometry, combined with the intriguing aromas of volatile compounds and the visual beauty of various crystals, inspires a sense of chemiphilia in many chemists, analogous in some ways to E. O. Wilson's biophilia.

J. Meinwald
Proc. Natl. Acad. Sci. USA, 2003, **100**, 14514.

E. O. Wilson, biologist and naturalist *par excellence*, defines biophilia 'as the innate tendency to focus on life and lifelike processes'. In a remarkable essay entitled 'Bernhardsdrop', in his book *Biophilia*, Wilson reflects on a naturalist's quest, at the end of a long day observing and collecting ant species in the South American rainforest. In Wilson's words: 'Mundane events acquired the raiment of symbolism, and this is what I concluded from them: That the naturalist's journey has only begun and for all intents and purposes will go on forever. That it is possible to spend a lifetime in a magellanic voyage around the trunk of a single tree. That as the exploration is pressed, it will engage more of the things close to the human heart and spirit. And if this much is true, it seems possible that the naturalist's vision is only a specialized product of a biophilic instinct shared by all, that it can be elaborated to benefit more and more people. Humanity is exalted not because we are so far above other living creatures, but because knowing them well elevates the very concept of life.' (*Biophilia*, p. 22).

My attention was drawn to the word 'biophilia', by an article entitled 'Understanding the chemistry of chemical communication: Are we there yet?', which appeared a few weeks ago (J. Meinwald, *Proc. Natl. Acad. Sci. USA*, 2003, **100**, 14514). Meinwald, while introducing a collection of papers from a symposium entitled 'Chemical communication in a post-genomic world', emphasizes the central role of organic chemistry in probing the molecular mechanisms of biological signalling and introduces the term 'chemiphilia'. Organisms communicate with one another and often control their environment by secreting specific chemicals, which home in on target receptors, with amazing economy and efficiency. 'Natural products chemistry', a branch of organic chemistry, is a discipline as old as 200 years. Meinwald quotes from a famous letter written by Jons Jakob Berzelius to Friedrich Wohler, in 1835: 'Organic chemistry just now is enough to drive one mad. It gives me an impression of a primeval tropical forest, full of the most remarkable things, a monstrous and boundless thicket, with no way to escape, into which one may well dread to enter.' In the century and a half that has passed since Berzelius' letter, natural products chemists have isolated and catalogued the structures of thousands of organic compounds from nature, focussing on 'secondary metabolites', which at first glance seem to have no real use in the organisms which produce them. Natural products researchers have been seduced by the complexity of nature's chemicals, turning into 'chemiphiles', who can, to borrow Wilson's phrase, 'spend a lifetime in a magellanic voyage', characterizing all the metabolites in the extract of a single plant.

The borders between primary and 'secondary' metabolism were drawn early in the development of biochemistry, which found a home within the broad disciplinary framework of biology. Organic chemistry developed by erecting barriers, excluding those who transgressed artificial boundaries. The divide between biochemists, who transformed themselves into molecular biologists, and natural products chemists was highlighted by Arthur Kornberg in 1987: 'By 1950, organic chemistry had been enriched by more than a century of impressive achievements. . . . They belonged to a proud and venerable science. . . . The problem was that organic chemists placed arbitrary boundaries on their science. Although in their pursuit of natural products, they might still eagerly seek

the challenge of an Amazonian butterfly pigment, they would not accept nucleic acids, proteins, and enzymes as proper natural products. I recall in an organic chemistry textbook of 1960, a reference to the importance of proteins "as a source of amino acids for living cells". 'This is like saying the importance of the automobile is as a source of its parts.' Kornberg went on to chastise molecular biologists: 'In its rapid and turbulent growth, molecular biology has washed away much of the bridge to chemistry. . . . Molecular biology falters when it ignores the chemistry of the products of the DNA blueprint – the enzymes and proteins and their products – the integrated machinery and framework of the cell. Molecular biology appears to have broken into the bank of cellular chemistry, but for lack of chemical tools and training, it is still fumbling to unlock the major vaults.' (A. Kornberg, *Biochemistry*, 1987, **26**, 6888).

In the years following Kornberg's lament, a greater integration of chemistry and biology seems to have occurred with many departments of chemistry adding the term 'chemical biology' to their names. Analytical methods which have their roots in chemistry and physics are now the common tools of biology. The genomics and proteomics revolutions will be spearheaded by techniques which cut their teeth on chemical problems, X-ray crystallography, nuclear magnetic resonance (NMR) and mass spectrometry among them. Lasers are at the heart of the most powerful microscopic imaging methods which have transformed cell biology. But even as chemistry has moved to embrace biological problems, classical natural products chemists have become an endangered species. For the last few decades organic chemistry has been dominated by the practitioners of chemical synthesis and developers of methodology. The study of 'natural products' has been unfashionable. This is ironic because in the aftermath of genomics, it is now clear that 'secondary metabolites' are anything but secondary. Microorganisms and plants orchestrate the synthesis of these complex organic substances, using sequential enzymatic reactions, with modular multienzyme complexes produced by 'turning on' the expression of genes, lined up as a cluster, controlled by a single regulating element. The profusion and structural diversity of microbial metabolites is best illustrated by *Streptomyces*, which ushered in the 'golden age of antibiotics'. In his Nobel lecture of 1952, Selman Waksman (who many believe was wrongly credited as the discoverer of streptomycin, which for a while was the front-line drug against tuberculosis) quotes from the *Ecclesiastius* (XXXVII, 4): 'The Lord hath created medicines out of the earth; and he that is wise will not abhor them.' The reference here is to the abundance of soil microorganisms, which hold immense promise for the discovery of the next generation of antibiotics. The growing interest in 'secondary metabolites', the 'natural products' of classical chemistry is fuelled by the increasing importance of pharmaceutical leads based on plant extracts, that have been used in traditional, indigenous medicine. Microbial metabolites and marine natural products hold an enormous promise for the discovery of new molecules. This resurgence of interest in the molecules of nature comes at a time when the tools of purification and structural analysis have reached an extraordinary level of sensitivity and sophistication. Sadly, the chemists with interest and insight into natural products chemistry have long been sidelined. In India, few major institutions can boast of credible 'natural products' programs, although they all pay lip service to the importance of developing pharmaceutical leads.

The new and developing area of chemical ecology will also require considerable expertise in the chemistry of natural products. Thomas Eisner, one of the founders of the field, notes: 'New disciplines arise by a convergence of interest. Chemical ecology is the product of a partnership between biologists and natural products chemists united by a shared vision and empowered by complementary skills. The vision stems from the realization that all organisms emit chemical signals and that all, in their respective ways respond to the chemical emissions of others.' Eisner states emphatically: 'Chemical ecology is now embarking on the most ambitious and inventive phase of its existence. To stand by and allow natural products chemistry to vanish or even to be weakened, is to deny chemical ecology its future.' (*Proc. Natl. Acad. Sci. USA*, 2003, **100**, 14517).

In his book *Biophilia*, Wilson considers a 'superorganism', an ant colony and a symbiotic fungal mat. He calls the ant–fungus combination, 'one of evolution's master clockworks, tireless, repetitive, and precise, more complicated than any human invention and unimaginably old'. (p. 37). Nature is full of examples where chemistry, shared and complementary, controls the dynamics of ecology. The meeting of minds of 'biophiles' and 'chemiphiles' is necessary to restore natural products research to its rightful place at the advancing borders of chemistry and biology.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Autonomy, accountability and responsibility

The Ministry of Human Resources Development (MHRD) oversees the sprawling enterprise of education in India. Two autonomous (and here I use the word loosely) organizations that fall within the umbrella of the MHRD, the University Grants Commission (UGC) and the All India Council of Technical Education (AICTE) serve to regulate our Universities and 'professional' institutions which provide higher education in engineering and technology. With education being a 'state subject' the vast majority of our universities are controlled by state governments, most of which have a limited interest in higher education. MHRD's direct control is felt by relatively few institutions, a handful of Central Universities, the Indian Institutes of Technology (IITs), the Indian Institutes of Management (IIMs) and the Indian Institute of Science (IISc), among them. In general, the MHRD provides a substantial annual grant to these institutions, but has permitted them to function with a considerable degree of autonomy. The IITs and IIMs were creations of the state at a time when globalization and liberalization were unknown terms; even Mikhail Gorbachev's famous phrases of *glasnost* and *perestroika* were yet to enter our lexicon. Set up as national institutions of academic excellence, the IITs and IIMs have proved enormously successful. In a relatively short span of time they have established a worldwide presence; their alumni among the most influential Indians, both at home and abroad. With economic liberalization, these institutions have found that raising funds to create an institutional corpus is easy and their coffers have swelled. Directives from the government, that subsidies in higher education must be minimized have led to a substantial increase in the fees that students pay at the IIMs and IITs. Higher education has slowly become more expensive, with elite institutions like the IIMs raising the annual fees to a figure of Rs 1.5 lakhs. High fees seemed to be in line with the thinking in governmental circles that subsidies must be reduced for courses, where students can invariably repay any loans that may be necessary to finance their course of study. But in a sudden turn-around the MHRD has directed the IIMs to reduce their fees to a figure of Rs 30,000 per year. Normally the reduction in the price of a product or a service would be a cause for celebration. Instead we have a brewing controversy. The pages of magazines and newspapers abound with articles, where even students who might welcome a fee reduction expound on the terrible consequences of MHRD's move. To an uninformed outsider the reactions are inexplicable. Closer examination of the issue reveals the contours of the arguments on the two sides. The IIMs (or at least their most vocal supporters) view this as an attempt by the MHRD to gain control over the institutions. A return to subsidies would ostensibly permit government a handle to erode institutional autonomy. For the MHRD a lowered fee structure would mean a greater accessibility of the IIMs to students who are intimidated by high costs of education. This view is promptly countered by the IIM protagonists, who argue that loans are readily available for all students who are admitted to these institutions. Indeed the success of the IIMs is judged by the salaries that each successive graduating class commands on the market, making as IIM alumnus extremely credit worthy. Is the MHRD's position based on misguided populism or is there a deeper purpose, posing a threat to institutional autonomy? Are the IIM supporters overreacting to a move which may be little more than playing to populist sentiment, at a time when the government seeks to project a favourable image?

The most curious feature of the IIM fees controversy is the public role played by captains of industry. They have mounted a public campaign, which is unprecedented in its intensity against a government directive. Business interests in the IIMs run deep. The graduates of these institutions, with very very few exceptions, enter the private sector. The government has invested considerably in these institutions since their founding. The beneficiaries of this support, continued, consistent and largely undemanding, have been the alumni and the organizations that have employed them. It would almost appear that having reached a stage of possible financial self sufficiency, these institutions and their supporters look forward to a future, where 'privatisation' has occurred seamlessly, unnoticed by all. The transformation of publicly funded institutions into essential private organizations is a diffi-

cult one. In the area of higher education there are many complex issues to be considered. Some years ago there was a proposal to 'buy control' of the IIT system with an infusion of about a billion (and here I write from memory) dollars. A little reflection might lead to the conclusion that these institutions are worth much more; even more importantly, public control over these national assets may be critically important. The captains of industry who worry that increased dependence on MHRD funds will limit autonomy at the IIMs, with consequent effects on their products, might detail the nature of their fears. Would there be a compulsion to dilute academic standards? Will the academically unqualified enter the institution? Would the MHRD dictate the course of future faculty recruitments? Would some faceless bureaucrat decide the nature of the courses to be offered?

We might ask, more generally: 'What is the autonomy that is desirable at institutions of higher education which are supported by public funds? The answer, of course, would be the complete lack of interference in academic decision making. This would encompass student admissions, course content, examination systems, evaluations and faculty recruitment and assessments. Traditionally, the sometimes conflicting demands of internal autonomy and public responsibility have been integrated by management councils which are broadly representative of different sections of the academic community, government, industry and public representatives. Erosion of autonomy occurs when leaders of institutions and their councils of management abdicate their responsibilities in the face of political or bureaucratic pressure. In many matters like fixation of salaries or disbursement of perquisites, many institutions conveniently opt for autonomous decision making or acceptance of MHRD directives, depending on which option is more favourable. A couple of years ago the MHRD, in a burst of generosity, provided funds for travel to international symposia to the faculty of IITs and IISc. There were no murmurs of protest at an expenditure which does little for research and subsidizes conference organizers. Autonomy can be jealously guarded only when the administrators of institutions and their councils of management have a clear sense of public purpose and the strength of conviction to adopt unpopular positions. Unfortunately, the constitution of institutional councils is not a subject which attracts much attention, often leading to situations where institutions lack powerful, visible and energetic champions. Here the IIMs have scored, with many public figures weighing in on their side against the MHRD.

Finally, there is the question of accountability. Publicly funded institutions must adhere to standards, procedures and practices which justify public investment and public trust. Autonomy cannot be used as an impenetrable shield. Academic freedom cannot be interpreted as a license for non-performance. When the dust has settled on the IIM–MHRD battle, some reflection on the relationships between public institutes of higher education and the government might be worthwhile. The policies of institutions and governments cannot be matters of impulse. The growing power of business interests will also be a factor which may redefine these relationships in the future.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science, scientists and scientometrics

Some thirty years ago when I had just begun a research career at the Indian Institute of Science (IISc), the library followed the practice of displaying the week's new journals on Friday afternoons. The most eagerly awaited arrival would be the copy of *Current Contents* (*CC*), already fragmented by discipline. *CC* Life sciences which had the largest readership was jealously guarded by staff at the counter; readers signing for its issue, before spiriting it away to a quiet corner to get a quick glimpse of the title pages of the world's science journals. Notebooks and reprint request cards were always in evidence, as the journals would arrive only several months later; attractive stamps on a request card might induce authors, with a philatelic bent of mind, to send copies of their papers by airmail. This was the era before xerox machines, e-mail, fax and the Internet. Networks and networking were words which were still to enter everyday use. Even as I waited, weekly, for my turn at *CC*, I must confess that the title pages of journals interested me only marginally; I was (and presumably am) a non-competitive scientist content with the enormous holding of old journals in IISc's venerable library. I really waited in line to read Eugene Garfield's weekly essay in *CC*; a column in which he was to lay the groundwork for disseminating the ideas of scientometrics and information science to an audience of practising scientists. Garfield's style was engagingly direct, uncomplicated and forthright. He explained the concept of impact factors, considered possibilities for their use and abuse, introduced the idea of *Journal Citation Reports* (*JCR*) and developed the ideas of citation analysis for identifying highly influential papers and their authors. The *Science Citation Index* (*SCI*) had a print edition, so outrageously priced, that there was only a sample copy in the IISc library, which remained forlorn and untouched on the shelves. Entries were made only on the basis of first author names, making the *SCI* a difficult and ineffective tool for ranking scientists. Garfield's Institute for Scientific Information (ISI) in Philadelphia was beginning to amass (capture ?) data about science and scientists. Citation counts identifying the '100 most highly cited' scientists would appear in Garfield's columns, sifted by discipline and specialization. Sitting in a corner of the library, I would devour the names of these superstars, some already legends in their fields. These lists seemed to reveal some history of science and uncover some of the major actors.

No one I met in the 1970s and 1980s, among those who mattered, seemed to have read Garfield's essays. 'Citation' was a word rarely heard in Indian scientific circles. Garfield himself, lectured at IISc, sometime in the early 1980s (but my memory may be playing tricks), to a largely indifferent audience, which had not yet begun to be mesmerised by citations and impact factors. There were occasions when the scientific impact of work done in India was revealed by citation analysis. In 1986 Garfield traced the impact of Sambhu Nath De's seminal work on cholera enterotoxin, establishing that De's paper published in 1959 in *Nature* (1959, **183**, 1533), was drawing over 30 citations per year in the decade between 1973 and 1983. This observation, which revealed a 'Nobel class' scientist in post-Independence India, attracted my attention, stimulating a special issue of this journal four years later (*Curr. Sci.*, 25 July 1990, **59**). De died in 1985, 'unhonoured and unsung in India's scientific circles'. Clearly even in the 1980s and early 1990s, scholarly citation analysis did not create any ripples in India and had little impact on our thinking. The arrival of the electronic versions of the *SCI* has dramatically changed perceptions in India and around the world. For a while the *SCI* was available only to a limited few; those who had CD versions or those whose institutions were devoted to analysing the output of science. The ISI as an organization grew into a commercially powerful entity, vigorously marketing its products, the *SCI* and Journal Citation Reports. The 'Web of Science' is now available, in the relative comfort of a scientist's office, in many of India's major institutions. Journals are also flooded with self advertising, where impact factors are prominently quoted. Even *Nature*, which one would presume is pre-eminent as an interdisciplinary journal, is constrained to immodestly proclaim: '30.4, No *Nature*, No impact'. The ambience in scientific circles has transformed so dramatically that it is hard to believe that there was a time when 'impact' was a word rarely used in a precise, quantitative sense in assessing science, journals, papers and their authors. In our surroundings, it has become com-

mon-place to sit in presentations where speakers project lists of publications, complete with the impact factors (presumably, current) of the journals listed. The message is clear, unambiguous and aggressive; the audience must judge the speaker and the science presented only on the basis of impact factors.

The tools and techniques of scientometrics have been used to make assessments of science in India; exercises which have proved controversial when the conclusions are pessimistic, as recently demonstrated by the reactions to a letter published in this journal (S. Arunachalam, *Curr. Sci.*, 2002, **83**, 107). On the pages of history books it was customary to execute those who brought bad news. Very few recall that over twenty years ago Garfield had turned his attention to analysing Third World research. Using articles published in 1973 and the 1973–1978 *SCI* as a source, Garfield placed India at position 8 on a list determined by the total number of papers. Even then the parameter 'impact' suggested that quantity was not correlated to quality, a conclusion that many would have drawn, intuitively (Garfield, E., *Curr. Contents*, 15 August 1983, **33**, 253–263). I suspect the situation has changed in the last two decades; the issue of course is, have we improved or has there been a 'relative decline'? In all rankings we must remember that the competition is not standing still and what we compare might be the relative rates of progress.

My thoughts turned to scientometrics when I was asked to speak at a recent congregation of 'information scientists', many of whom are traditional librarians in disguise. The meeting organized by those who market ISI's portfolio of products was held in a lavishly, opulent hotel. Armed, as I was, with a skimpy set of handwritten transparencies, I felt curiously out of place in a gathering largely bereft of practising scientists. Here was an audience whose primary purpose was to watch scientists, tracking their performance using formulae and indices, which they understood only imperfectly. But as I warmed to my theme, which forms the title of this essay, I was emboldened to attack the growing breed of 'Indian scientometrists', who have little understanding of science, with all its complexities, and an even more limited appreciation of the pitfalls of statistics; especially the statistics and inferences drawn from small samples. Carried away by righteous indignation I quoted Hilaire Belloc: 'Statistics are the triumph of the quantitative method, and the quantitative method is the victory of sterility and death'. Apparently stung by my, undoubtedly, overstated criticism of scientometrics as practised in India, a member of the audience asked rather pointedly: 'Are not information scientists only holding up a mirror in which scientists see themselves?'

Quantitative assessments of science based upon journal impact factors and citation counts have begun to influence the behaviour of scientists worldwide and in India in particular, especially in the high profile national institutions. Authors decide the journals to which they submit their papers on the basis of impact factors; IF 2.0 is considered superior to IF 1.5. While journals of the highest impact must be the media of choice, acceptance of manuscripts is decidedly uncertain, forcing authors to hunt for outlets which will boost the 'average impact factors' of their publication lists. The common wisdom that the science of a good paper remains independent of the journal in which it appears, does not find many supporters, nowadays. The greater visibility attached to high impact journals can also attract greater attention (mostly negative) to flawed papers. The spate of retractions in high profile journals, most recently in the area of cell biology (Strohl, G., *Cell*, 2004, **116**, 481), might suggest that the most extravagant claims are often made in the most visible journals. Citation counts which are used to rank authors have begun to influence citation behaviour. Authors, referees and editors pay great attention to reference lists for different reasons, of course. Authors use 'directed citation' to influence choice of referees improving their chances of acceptance; referees demand inclusion of citations, presumably to their own published papers and editors encourage excessive (and sometimes unnecessary) citation of their own journals, a device that can enhance impact factors. In the surreal world of quantum objects physicists worry about the effect that observations may have on the objects of measurements. In the more mundane and real world of scientometrics, measurements have begun to influence the subjects being studied.

In India the focus on citations and impact factors has begun to distort our perceptions about science and scientists. Every administrator, every librarian and every scientist who takes no joy in his science has now become a convert to scientometrics in its basest form. The sight of directors of national institutions and heads of agencies abdicating scientific judgement in the face of the juggernaut of the impact factor is distressing. Even otherwise rational scientists seem unaware that quantitative comparison across disciplines is ridiculous. For example, the 'mean impact factors' for journals in the *SCI* in different fields follow the order: Life Science ~ 3.1, Physics ~ 1.5, Chemistry and Chemical Engineering ~ 1.4, Mathematics and Computer Science ~ 0.5 (M. Amin and M. Mabe, *Perspectives in Publishing*, Elsevier Science). Clearly one should not compare individuals and institutions across fields. A degree of circumspection is also warranted in sprawling disciplines with many subfields. Scientometrics in India is a field in the grip of practitioners, who are largely devoid of the insights that are necessary for scholarly and thoughtful analysis. Many scientists, and more importantly, leaders of science have allowed themselves to be seduced by the promise of scientometrics in decision making. We have already begun to pay a price as reflected in the decided change in the ambience of our institutions. I cannot but look back with nostalgia on gentler times.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science and Technology Proficience: China and India

'A vicious cycle is at work whereby the developing nations (especially the S&T lagging countries) fall farther and farther behind the industrialized nations that have the resources – in financial as well as human-development terms – to apply scientific advances and new technologies ever more intensively and creatively. The current disparity is likely to grow even wider as the industrialized nations continue to master the tools of science and invention, vastly outspend the developing nations in research and development (R&D) and even capture some of the developing nations' most precious human resources for their own use.'

Inventing a Better Future
InterAcademy Council, 2004

'China has over many years sustained its efforts to turn itself into a world class scientific power... . Despite such continuous development and despite major expenditure on science by the Chinese government, China is not yet fulfilling its scientific potential... . This results not only in a low scientific profile on the world stage but also in lost opportunities to make the most of what the rest of the world can offer, both scientifically and technologically. This also leads to missed opportunities for China to improve its economy and the quality of life for its citizens.'

P. Campbell
Nature, 2004, **428**, 203

An elegantly produced report entitled 'Inventing a better future' has been released a few weeks ago by the Inter-Academy Council (IAC). The subtitle, 'A strategy for building worldwide capacities in science and technology' suggests that science and the technologies derived from science may be used to build a better future for the billions of deprived people across the world. The IAC is a body that 'brings together the collective advisory expertise and experience of a truly worldwide group of academies'. The body has its headquarters in Amsterdam, with the Royal Netherlands Academy of Arts and Sciences playing the host. Its governing board consists of the Presidents of 15 'national academies of science and equivalent organizations' and the Third World Academy of Sciences. The countries represented are Brazil, China, France, Germany, India, Israel, Japan, Malaysia, Mexico, Russia, South Africa, Sweden, Britain and the United States. India, with its bewildering range of Academies, is represented by the Indian National Science Academy (INSA), Delhi. The IAC considers itself a non-governmental organization (NGO) and can 'mobilize the world's best scientists for providing expert knowledge and advice to international bodies such as the United Nations and the World Bank, on 'critical issues – such as economic transformation and globalization; reduction of poverty, hunger and disease; and the sustainable use of natural resources'. The intent of the IAC report is to emphasize that the 'knowledge-based economies of the 21st century' require 'capacity building in science and technology everywhere'. The report states clearly that 'the need is greatest for developing nations' and calls for a 'global movement to address this need, which has been insufficiently addressed when not neglected altogether'.

The IAC report, put together by a blue ribbon panel, whetted by diverse experts, is a long wish-list of actions that must be performed by governments and agencies worldwide, particularly in the developing world. The report is largely devoid of statistics and avoids analysis of problems that may be country or continent-specific. Instead, in carefully measured language, the report advances the need for the poorest countries to invest in S&T and higher education and calls for international cooperation to bridge the growing gulf, in the capacity to absorb new and enabling technologies, between the advanced and developing countries. In attempting to read a document of 144 pages (and I did skip it in parts), I was struck by the boundless optimism of the IAC in hoping that governments across Asia, Latin America and Africa would take the proffered advice seriously. Might this not be another coffee table document which would one day be reissued with minor revisions, since most of its recommendations would remain unimplemented? Governments generally ignore even the reports that they commission and generate internally. Why then would the IAC report command more than a passing commendation, for a job well done? Even as I pondered the fate of the IAC report, I was drawn to some of its sections. This report divides the world in an intriguing fashion into four groups: S&T-advanced countries, S&T-proficient countries, S&T-developing countries and S&T-lagging countries. The advanced countries (although a classification is

not provided) are undoubtedly the developed 'First World' nations. The lagging countries must be the poorest nations, with little or no S&T infrastructure. Many countries in Africa would probably fall into this category; others which are always in the news like Iraq and Afghanistan would undoubtedly qualify, despite the former's much-touted ability to produce weapons of mass destruction. The two middle groups provide an interesting distinction. S&T-proficient countries appear to have a distinct edge over the S&T-developing countries, but the IAC report does not explicitly classify the 90 countries whose national academies are represented on the Inter-Academy Panel on International Issues. In diplomatically understated fashion the IAC seems to define the Second, Third and Fourth Worlds which, in the area of science, are represented by the Third World Academy of Sciences (TWAS).

The world's two most populous countries, China and India, are presumably S&T proficient; a term precisely defined by the IAC as applicable to countries 'with scientific and technological strength in several research areas and a growing S&T capacity in all aspects, including personnel, infrastructure, investment, institutions, and regulatory framework'. Coincidentally, even as the IAC report became available, *Nature* (11 March 2004) produced a supplement; a collection of seven articles by scientists of Chinese descent working in the West, who offer advice to their compatriots in China. Indeed, a characteristic of 'S&T proficience' is the ability to export legions of trained researchers to the West, creating a large expatriate community which then has the ability to compare policies and practices in the West, with those in the countries of their origin. China and India, of course, lead the rest of the world in producing human resource for the S&T enterprises in the 'advanced countries'. The articles on China in *Nature* may appear curiously familiar to many Indian readers. Chinese-American venture capitalists and entrepreneurs conclude that 'exceptional synergies between the Chinese and US economies are evolving'. As a result of increasing involvement of US companies, the authors suggest that China will 'lead the world' in nanotechnology and biotechnology (Hsia, J. C. and Fong, K., *Nature*, 2004, **428**, 218). Some of us may have heard similar expressions of faith from US-based venture capitalists of Indian origin. In an assessment of basic research in China in the area of biological sciences the judgements are less favourable: 'Despite a recent improvement the number of high quality research papers published by scientists from mainland China has remained low'. The author, Ray Wu, suggests that 'this is because there are few productive scientists in China and the level of financial support for basic research is inadequate. The system for evaluating research proposals and distributing funds is far from ideal, and does not promote innovative research. In addition, the education system in most universities does not encourage students to think critically and creatively'. In a commentary on publication practices Wu notes that 'the vast majority of papers are published in Chinese journals which are usually not read by scientists of other countries and thus are seldom cited'. Many of Wu's comments will appear familiar to Indian readers; China and India share many problems as they attempt to move up the ladder of S&T advancement. The popular saying in China's scientific circles, 'Small grants, big review; medium grants, small review; big grants, no review', succinctly summarizes the situation in India too. In analysing cultural traditions which inhibit the growth of science in China, Mu-Ming Poo notes that 'Chinese institutes have two important tasks: reform of the administrative structure, and establishment of a merit based system for staff evaluation and resource allocation... . Scientific administrators at all levels have enormous responsibility – and power. These administrators often control resources and give instructions rather than provide services'. Poo quotes Joseph Needham who concluded that 'bureaucratic feudalism' inhibited the rise of modern science in China (Poo, M.-M., *Nature*, 2004, **428**, 205). Indian readers may recall that the Prime Minister called for the 'debureaucratization' of science, at the Indian Science Congress in January 2003, while releasing the government's Science and Technology Policy. Over a year later, it is business as usual and there are indications that the mechanisms for administering science will remain complex and opaque. Reading *Nature*'s review of science in China, I must conclude that both countries face very similar problems as they struggle towards the IAC label of 'S&T advanced country'.

In returning to the IAC report, it is worth noting that a 'box' describes 'Indian research centres', featuring the Indian Institute of Science and the Indian Institute of Technology system. These institutions are highlighted in a section on 'S&T capacity building', an activity which is termed a 'shared regional and global responsibility'. Ironically, it is these very institutions which have been targeted for 'sanctions' by the governments of some S&T advanced countries, notably Japan and Germany which appear to have intensified restrictions on sales of scientific equipment over the last couple of years. In an interesting sidelight, the American Type Culture Collection (ATCC) does not ship, DNA segments ('clones') generated during the genome analysis programs, to some of these institutions. Presumably, the academies of both the 'S&T-advanced' and 'S&T-proficient' countries are unaware of these difficulties or unwilling and unable to address them. The many wonderful recommendations of the IAC panel would undoubtedly be implemented, by governments and scientific communities in different nations, in an ideal world. No one can quarrel with the need 'to ensure that science and technology are harnessed to address the needs of all, rather than add to the luxury of a few'. Sadly, we live in a far from ideal world.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Autism, vaccines and editors

Autism is a term used to describe a group of developmental disorders that lead to severe behavioural abnormalities. Autistic children may be severely limited in their social interactions, display unusual behaviour patterns and often appear withdrawn and uncommunicative, despite possessing 'average or superior cognitive skills' in specific areas. Autism can be devastating for affected families; there can be no more wrenching experience than to watch a severely disabled child struggle with life in a world with which they can hardly communicate. The description of autistic syndromes can be traced to the work of Leo Kanner and Hans Asperger in the 1940s. Since then the 'autistic spectrum' has widened and there is a much greater awareness of the range of clinical symptoms that may signal autism. As awareness has increased, so have the number of reported cases of autism, worldwide, particularly in the developed countries. Some estimates place the incidence of autism at a figure of 1 in 500 people; in India this would suggest a figure of about 2 million affected individuals (www.autism-india.org). There is a limited awareness about autism amongst medical professionals in India, with childhood symptoms often dismissed by impatient and insensitive doctors. 'Mental retardation' is such an all-encompassing term to rationalize developmental disorders, that autistic children may have to wait for several years before their special status is recognized. In this period, they may be subjected to an educational system that can be harsh, impersonal and rigid. The established fact that some autistic children display exceptional abilities and have extraordinary I.Q's makes the management of autism an extremely challenging task. At present, the causative factors that precipitate autism remain unknown; treatment with drugs has proved elusive. Management of autism by educating parents and other care-givers appears to be the only pragmatic approach towards alleviating the enormous burden that autism imposes on its sufferers.

Even as reported cases of autism have increased, attempts to establish a connection between environmental factors and autism have intensified. In 1998, Andrew Wakefield of the Royal Free Hospital, London and a large number of colleagues published a paper in *Lancet* entitled, 'Ileal-lymphoid-nodular hyperplasia, non-specific colitis, and pervasive developmental disorder in children' (1998, **351**, 637–640). Despite its forbidding title, the paper had a message which was clearly understood by the general public in Britain, aided by the inevitably sensational reporting in the press; the authors suggested a direct connection between the administration of the measles, mumps and rubella (MMR) vaccine and the onset of the behavioural disorders characteristic of autism. This paper was to touch off a furious debate on the connections between the MMR vaccine, inflammatory bowel disease and autism in children. The Wakefield paper was an obviously limited study, that was followed by a spate of reports, many of them in *Lancet*, which appeared to cast doubt on the Wakefield conclusions. The *British Medical Journal* entitled an editorial: 'MMR vaccination and autism 1998. *Deja vu* – pertussis and brain damage 1974?' The reference was to a 1970s scare which resulted in a fall in pertussis vaccination levels, leading to many deaths in Britain and elsewhere. The authors noted: 'Since each year over 600,000 British children received MMR in their second year, an age when autism can typically manifest itself, chance alone dictates that some cases will appear shortly after vaccination. Cases will be selectively referred to a group known for its interest in MMR, inflammatory bowel disease, and autism, so the hypothesis rests on clinical anecdote rather than on an epidemiologically sound base'. The authors concluded on a cautious note: 'While (no) vaccine can be guaranteed to be without any risk, this has to be weighed against the huge advantages of protection against disease. Seeds of concern have been sown among parents and no doubt will continue to be spread. Those advising families must make sure parents can base their decisions on hard science and evidence'. (A. Nicoll, D. Elliman and E. Ross, *BMJ*, 1998, **316**, 715–716). Despite the vigorous criticism of the Wakefield paper, public doubts about the MMR vaccine resulted in a significant drop in the rate of administration of the vaccine to children in Britain, falling from a coverage of about '90% in 1998 to its current level of less than 80%' (J. Giles, *Nature*, 2004, **427**, 765). The MMR controversy reached its highest level of visibility when the British

Prime Minister, Tony Blair, was questioned in Parliament on whether his youngest child had been vaccinated.

The Wakefield study has now returned to centre stage. In February, a review of the available scientific evidence categorically stated that 'MMR does not cause autism or any particular subtypes of autistic spectrum disorder' (F. Destefano and W. W. Thompson, *Expert Review of Vaccines*, 2004, **3**, 19–22). The British press also found that Andrew Wakefield did not declare a potential conflict of interest in which he conducted a paid study for the Legal Aid Board on 'whether parents whose children had allegedly been harmed could sue manufacturers of the MMR vaccine' (*Nature*, 2004, **427**, 765). In a further development, 10 of the paper's 13 original authors, Wakefield was not one of them, published a curiously worded retraction in March 2004: 'We wish to make it clear that . . . no causal link was established between MMR vaccine and autism as the data were insufficient. However, the possibility of such a link was raised and consequent events have had major implications for public health. In view of this, we consider now is the appropriate time that we should together formally retract the interpretation placed upon these findings in the paper, according to precedent'. In an extraordinary move to preempt the inevitable media blitz following the unearthing of the Wakefield 'conflict of interest' story, the *Lancet* editorially declared the paper 'flawed' and stated that the paper would not have been published if the authors had declared the apparent 'conflict of interest'. Interestingly, such declarations appear routinely in the biomedical literature. Why did it take *Lancet* six years to uncover scientific flaws and did the 'conflict of interest' have any bearing on the interpretation? Do declared 'conflicts of interest' legitimize scientific findings? Editors of scientific journals and editors of high profile biomedical journals in particular confront difficult situations in such cases.

An organization called the Committee on Publication Ethics (COPE), based in London, was established in 1997 to help editors discuss ethical issues. Their most recent document, The Cope Report 2003 is an engaging production with an explicit section on 'editorial accountability' (www.publication-ethics.org.uk). In his introduction to the section Richard Smith, Editor of the *British Medical Journal* says: ' . . . We don't really know what misconduct is in an editorial context. We have vague ideas, but we don't really have a clear idea'. He adds, for good measure: 'And editors are peculiarly unaccountable – perhaps some of the most unaccountable people in the world because of their traditions of editorial freedom'. The report details many specific cases where editors and journals have been found wanting, providing a spectrum of examples which should strike a chord in many editors. The all time classic in editorial misconduct is cited by Smith, in which Cyril Burt founded the *British Journal of Statistical Psychology*. Burt ran riot, publishing his own flawed, unrepeatable work extensively. Interestingly, he altered 'the work of others without permission, sometimes adding favourable references to his own work'. With biomedical publishing becoming a frenetically competitive activity, both authors and editors confront situations where corners are cut. Considerations of ethics and accountability may fade into the background, resulting in frequent public controversies when questionable practices are brought to light.

The public discussion of the link between MMR and autism however raises important issues of vaccine safety. The eradication of smallpox, the drive against polio and the protection against the common infections of childhood, diphtheria, pertussis, tetanus and measles have contributed enormously to improved standards of public health, worldwide. Universal immunization programs seek to eliminate human reservoirs of pathogens, with the ultimate goal of decisively eliminating specific diseases. Thus far, smallpox is the most dramatic global success story. But as the disease burden diminishes, concerns about vaccine safety begin to surface. Side effects in rare cases are always possible; sometimes coincidence can confuse interpretation. In the case of a very low occurrence of side effects, policy makers must wrestle with the difficult choice of deciding public acceptability of a vaccine. As seen in the MMR controversy, compliance will be determined by public perception; in most countries coercion may not be a viable strategy. But, as biomedical research advances, the portfolio of vaccines that compete to enter universal immunization programs will increase. Given the size of the potential market, commercial pressures will build. Champions will be found in the scientific community and in governments to advance the claims of specific vaccines. In many cases, acceptably low toxicity may be easier to demonstrate than efficacy. The uncertainties in the design of many epidemiological studies contribute to the difficulties in this area. In India, a hepatitis B vaccine is already being pushed by government directive. Pressures will mount for its inclusion in the universal immunization program. Those of us who took the 'typhoid and cholera vaccines' in the 1960s and 1970s will wonder: 'Were these shots effective and whatever happened to them'.

The MMR controversy has now turned its focus to Wakefield's conflict of interest and the *Lancet* editor's response to the growing criticism of the original paper. The public debate, raucous at times, indicates that a fragile thread connects scientific research outcomes and vaccine acceptability. It is a sobering thought that 'conflicts of interest' may drive both proponents and opponents of vaccines. But, in the heat and dust of this controversy we must not forget that the causative factors of autism remain unknown and that effective treatment of this disorder must be an important goal of biomedical science.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Icons of Industry and Philanthropy: The Tata Centenaries

This, then, is held to be the duty of the man of wealth; to set an example of modest, unostentatious living, shunning display or extravagance; to provide moderately for the wants of those dependent upon him; and, after doing so, to consider all surplus revenues which come to him simply as trust funds, which he is called upon to administer.... the man of wealth thus becoming the mere trustee and agent for his poorer brethren.

—Andrew Carnegie
quoted by Frank Harris in
Jamsetji Nusserwanji Tata
Blackie, London, 1958, p. xi

His sturdy sense of character prevented him from fawning on any man however great, for he himself was great in his own way, greater than most people realized. He sought no honour and he claimed no privilege; but the advancement of India and her myriad peoples, was with him an abiding passion.

—Obituary of J. N. Tata
Times of India, May 1904
quoted by R. M. Lala in
Beyond the Last Blue Mountain
Viking, India, 1992, p. 7

I am convinced that of all the big problems that face India today, nothing is more important than the development of scientific research, both pure and applied, and scientific method. This is indeed the basis and foundation of all other work.

—Jawaharlal Nehru
21 November 1945

The development of science and technology in India over the last fifty seven years since Independence, has been based on Jawaharlal Nehru's unswerving faith in the importance of science and scientific institutions for national progress. Nehru's commitment to science has been shared by many successive governments. India's large network of laboratories and institutes has grown steadily over the past half a century. But two of the jewels in the crown of Indian science, the Indian Institute of Science (IISc), Bangalore and the Tata Institute of Fundamental Research (TIFR), Mumbai do not trace their origins to governmental initiatives; they were conceived by individuals and midwifed into existence by acts of private philanthropy. Jamsetji Nusserwanji Tata (J. N. Tata) conceived of a 'University or Institute of Research' in a plan made public in September 1898. His biographer, Frank Harris notes that J. N. Tata drew his inspiration when Lord Reay, the Governor of Bombay speaking (in 1889) as the Chancellor of the University, pointed out that 'India was at the parting of the ways; higher education could no longer develop if the universities remained purely examining bodies'. Lord Reay exhorted his audience: 'It is only by the combined efforts of the wisest men in England, of the wisest men in India, that we can hope to establish in this old home of learning, real universities which will give a fresh impulse to learning, to research, to criticism, which will inspire reverence and impart strength and self reliance to future generations of our and your countrymen' (Harris, p. 116). Twenty years after J. N. Tata began to think about an institution for scientific research, the IISc was formally born on 27 May 1909, when Lord Minto as the Viceroy of India signed the Vesting Order. Its founder, J. N. Tata had died a few years earlier, on 19 May 1904. Seventy days after J. N. Tata's death, one of his most distinguished successors was born, Jehangir Ratanji Dadabhai Tata (J. R. D. Tata). About four decades later, Homi Bhabha would approach J. R. D. Tata with the idea of establishing an Institute for Fundamental Research. In a remarkably short time the Sir Dorabji Tata Trust had accepted the proposal for establishing an institute focusing on 'mathematics and physics (including nuclear physics)'. J. R. D. Tata's biographer, R. M. Lala, notes that the approval came in April 1945, 'four months before the first atomic bomb was dropped on Hiroshima and the world woke up to the power of atomic energy' (Lala, p. 302). This year, 2004, we mark the centenaries of two of India's greatest sons; the centenary of Jamsetji Tata's passing and the centenary of J. R. D. Tata's birth.

By all accounts J. N. Tata was a remarkable man. He founded the industrial empire of the Tatas. Beginning in the area of 'cotton spinning and general trading' Jamsetji

laid the foundation of an industry that was to provide India with steel and hydroelectric power, catalysing the industrialization of this country. He built the Taj Mahal Hotel in India in the dying years of the 19th century; a symbol of Bombay's rise as India's business capital. Alongside was to be erected many years later, that symbol of imperial grandeur, the Gateway of India. Today, the monument built to commemorate the visit of King George V and Queen Mary to India in 1911, for the Grand Durbar, and Jamsetji's hotel are tourist attractions at the waterfront; the former a symbol of a colonial past, the latter a testimony to an emerging vision of India's future. To view Jamsetji's contributions in perspective we must remember that he announced his scheme for a 'University or Institute of Research' in September 1898 at a time ver set on the British Empire'. Gandhi had not returned to India from South Africa and many years were to pass before he was proclaimed Mahatma; Nehru was a child; self determination and independence were the dreams of very few public men. Philanthropy as a means of promoting higher education and research was a concept, novel even in the United States. Andrew Carnegie's famous endowment of $1 million 'five percent gold bonds' to set up a 'Technical School' in Pittsburgh, now the Carnegie-Mellon University, was made in 1900. Jamsetji Tata's offer of Rs 30 lakhs (£ 200,000) in 1898 was princely by any standard. The Indian Institute of Science would take another eleven years of gestation before it was born in Bangalore, aided by the generous donation of 300 acres of land by the Maharajah of Mysore. The British government of the day under Lord Curzon was slow, almost deliberate, in its treatment of Jamsetji's offer; he died in 1904 unaware that his vision for science in India would indeed be fulfilled. The Council of IISc erected a monument to its founder in 1916, facing the handsome building that now is a symbol of the Institute's history. I have walked past Jamsetji's statue on innumerable occasions over the past thirty years, scarcely noticing the details of the monument. In researching for this essay I came across a description in Frank Harris' biography of J. N. Tata first published in 1925: 'Near the main entrance stands a noble monument, the work of Mr Gilbert Bayes. One relief contains the figure of Jove, with his thunderbolts to typify electricity, and Vulcan with his anvil ready for steel; in the other Minerva holds a distaff covered with flax, and Calliope represents research. A bronze rail has as its centre a lamp of learning, flanked by dolphins to symbolize the founder's travels. Abundance at one side, and Knowledge at the other, support the centre shaft. At its summit is a bronze statue of the benefactor, and at its feet is an inscription, which will serve to remind future generations of the generosity of Jamsetji Tata, and the persistence with which he worked for the welfare of India' (Harris, pp. 144 145).

The Tata empire was shepherded through much of the 20th century by another remarkable individual, J. R. D. Tata. From 1938 to 1991, he presided with remarkable success, over a growing conglomeration of enterprises. This period spanned the movement to freedom, nearly four decades of socialism and government controls over private industry, to the dawn of the new era of a liberalized economy. He ushered, in the most romantic fashion, the aviation industry into India. The Tata Aviation Service, inaugurated in September 1932, would become the seed for the growth of Air India in the years to come. In his long innings at the helm of the Tata empire, J. R. D. Tata and his chosen colleagues carefully built upon the tradition of philanthropy set by Jamsetji and his son Dorabji. He played a key role in guiding Homi Bhabha towards the Sir Dorabji Tata Trust in 1944. The establishment of TIFR allowed Bhabha to recruit a nucleus of physicists and engineers, who would eventually build the Atomic Energy Establishment at Trombay, in the 1950s. Bhabha himself worked at IISc before moving to set up TIFR. The future leaders of many national programmes would emerge from these institutions. J. R. D. Tata was highly honoured by successive governments in India, receiving the Bharat Ratna in 1992, a year before his death, after a rich and rewarding life. J. R. D. Tata moved with the most powerful and influential politicians and leaders of his day. But in his own assessment 'the most outstanding of the distinguished men I have known was, undoubtedly, Homi Bhabha'. He noted that Bhabha was 'one of those who made me believe that some men in human history are born with the stamp of predestination on them which leads them to accomplishments beyond ordinary human capabilities' (*Keynote*, J. R. D. Tata, Tata Press, Bombay, 1986, p. xv). In many ways J. R. D. Tata's unflinching confidence in Bhabha's proposals set the stage for building a pillar of science in independent India.

Science in India owes a great debt to its benefactors, who persuaded colonial governments to permit and support institutions for research. In the history of science in India two events must stand out; the founding of the Indian Association for the Cultivation of Science in Calcutta in 1876 by Mahendralal Sircar and Fr. Lafont and the conception of the IISc in 1898 by Jamsetji Tata. In marking the Tata centenaries, we might do well to note that both IISc and TIFR have evolved from the very beginning in a manner in which private philanthropy and public support worked in concert for the benefit of institutional growth.

Anniversaries are a time of remembrance. On these occasions the exhortation 'Let us now praise famous men and our fathers that begat us' (Ecclesiasticus 44) might be appropriate. In thinking of Jamsetji Tata and private philanthropy, Andrew Carnegie's simple assertion, 'To die rich is to die disgraced', may be fitting.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Creation, Evolution and Intelligent Design

Little Lamb who made thee?
Dost thou know who made thee?
Gave thee life and bid thee feed,
By the stream and o'er the mead;
. . . . .
When the stars threw down their spears,
And water'd heaven with their tears,
Did he smile his work to see?
Did he who made the lamb make thee?

—William Blake

There is a grandeur in this view of life, with its several powers having been originally breathed into a few forms or into one: and that while this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been and are being evolved.

—Charles Darwin
The Origin of Species

Natural selection is the blind watchmaker, blind because it does not see ahead, does not plan consequences, has no purpose in view. Yet the living results of natural selection overwhelmingly impress us with the appearance of design by a master watchmaker, impress us with the illusion of design and planning.

—Richard Dawkins
The Blind Watchmaker
Penguin Books, 1990

Descended from the apes! My dear, let us hope that it is not true, but if it is, let us pray that it will not become generally known.

—The wife of the Bishop of Worcester upon hearing of 'Origins', quoted by Patricia G. Horan, Foreword, *The Origin of Species*, Avenel Books, New York (Edition 1979).

No writer has had a greater impact on our view of life and its evolution on this planet than Charles Darwin. His famous synthesis of the observations made during the voyage of the *Beagle* forms the pillar on which our modern view of biology is supported. Selection and adaptation are words which are now commonly used even by molecular biologists, bringing Darwinism to areas at the very borders of biochemistry and biology. Darwin published his immortal book in 1859 and died in 1882, but his ideas continue to be fiercely discussed even today. In the world of science, Darwin's views are rarely challenged; his ideas as firmly rooted as Newton's are in classical mechanics and in our view of the physical world. But in the larger world, Darwin arouses a fierce response from 'creationists', who hold the view that life forms on earth were created by a divine act. Science and religion appear to come into direct conflict, with the biological view having no need for the intervention of a 'master watchmaker', a benign God manifested in different forms in diverse religions. To open a discussion on science and religion is to tread on dangerous and slippery ground; an area best left to thinkers, philosophers and ideologues. It is in an area that I would normally skirt, (and indeed, I will try to do so here) afraid of being involved in discussions in which I have no expertise. But my interest was sparked by a question posed by a friend, a journalist, who asked: 'Why is it that the creation vs evolution debate rages unabated in the United States, but never seems to be an issue anywhere else in the world, including India?' I was spurred on by an editorial that appeared in the 20 April 2004 issue of *The Hindu*, a newspaper that has always provided space for science. The editorial entitled 'Humankind and Evolution' ended by quoting Clarence Darrow, the flamboyant defence lawyer in the Scopes trial of 1925: 'Ignorance and fanaticism is ever busy and needs feeding'. Within a couple of days of its publication, the *Hindu* editorial drew responses from readers. Of the three published letters, one endorsed the need for 'scientific inputs' at a time when 'obscurantist opinions' seem to prevail. Two others seemed to take a different view. One writer suggested that 'scientific theories are subject to change as new evidence emerges'; an unassailable viewpoint which implied that Darwin and natural selection must still await the jury's verdict. The other correspondent was more emphatic; he took the

view that opponents of Darwin have long favoured, noting that evolution 'involves a degree of creativity and consistency, which points to the hand of a supreme being'. The discussion in the columns of the *Hindu* clearly suggests that natural selection as the driver of biological evolution is a concept that sits uncomfortably in the public perception.

What do children in India learn at school? I found an ISC Biology book aimed at 12th Std students which began a chapter on the 'Origin of Life' with a paragraph on the 'Theory of Special Creation' but quickly moved to a biochemical discussion of molecules, with a prefatory sentence, that 'the idea that life could have originated in the primordial earth in some warm little pond' was advanced by Charles Darwin. The NCERT textbook for Class X in its chapter on 'Heredity and Evolution' resolutely avoids any mention of 'special creation' and votes for Darwinian natural selection and gradual evolution. As opposed to the relative equanimity with which evolution is viewed in the world, Darwinian ideas seem to invite greater challenge in the United States. President George Bush in his election campaign, in 2000, asserted: 'On the issue of evolution the verdict is still out on *how* God created the Earth'. Strangely, this statement seemed to imply that the question of 'who' had been resolved and that the discussion centred on 'how'. Creationists in the US have often challenged the teaching of evolution in schools and demanded equal time for a biblical view of the origin of life. Some years ago the Kansas School Board was embroiled in a controversy; more recently, the Ohio Board of Education has considered the introduction of 'Intelligent Design' to be taught alongside the Darwinian view of evolution.

'Intelligent Design' is a view couched in language that appears borrowed from science; far removed from biblical descriptions of the genesis. Biochemistry seems to enter this challenge to Darwin; its proponents referring to the 'irreducibly complex' systems of the molecular machinery of living cells. One of the champions of intelligent design goes so far as to say: 'Biochemistry . . . describes the workings of many living molecular machines within our cells, but offers very little information about how these systems supposedly evolved by natural selection. . . . Perhaps molecular machines appear to look designed because they are really designed.' (M. J. Behe, *Natural History*, April, 2002). The critics are quick to respond, citing many examples where the components of irreducibly complex systems have 'different but still useful functions'; clear evidence that complexity may evolve by gradual accretion and modification of function (K. R. Miller, *Natural History*, April 2002). Even as the boundaries between the fields of molecular and cell biology, developmental and organismal biology are bridged, new questions on the way evolution has shaped biology will be raised. The gap between chemical and biological evolution is a yawning chasm. The molecular fossil record is practically non-existent; biochemistry's 'irreducible complexity' manifesting itself in all living organisms. A study that has just been published described a double-stranded DNA viral capsid in a thermophilic archael virus. The similarity of the viruses which infect the three domains of life, Eukarya, Bacteria and Archaea, suggests a 'common ancestor that precedes the division into three domains of life >3 billion years ago' (G. Rice *et al.*, *Proc. Natl. Acad. Sci. USA*, May 2004). The search for fossils of 'ancestral life forms' or for 'primitive molecules', that convincingly demonstrate the blind watchmaker at work, is one of the biggest gaps in 'origins of life research'. Evidence for natural selection, adaptation and speciation abound in Darwin's biological world; the world of biochemistry is not blessed with such a wonderful fossil record. Nearly 20 years ago, A. G. Cairns-Smith wrote a marvellous little book *Seven Clues to the Origin of Life* (Cambridge University Press, 1985; Canto, 1998). The author, an unabashed fan of Sherlock Holmes, suggests that 'the origin of life is a Holmesian problem—that if we can understand how life could have started *at all*, then we should be able to work out, roughly at least, how it *did* start'. Cairns-Smith provides no definitive answers; some of his chapters have dangerously misleading, but seductive titles, cf. 'Build your own *E. coli*'. Nevertheless, his introduction to biology's least understood area remains required reading for anyone, scientist and layman alike, who would like to worry about life's beginnings on earth.

The creation vs evolution debate in the US reached a dramatic climax in the famous 'Scopes Monkey Trial' held in Dayton, Tennessee between 10 and 25 July, 1925. John Scopes, a young school teacher was set up by 'rationalists' to challenge a state law banning the teaching of evolution. The trial could have been scripted in Hollywood; pitting a famous lawyer Clarence Darrow arguing for evolution and a three-time presidential candidate William Jennings Bryan, upholding the creationist view. Darrow demolished Bryan in the trials climax, by putting him on the witness stand.

The battle between creationists and evolutionists continues to be fought in the US, with opponents of Darwinism labelling evolution as a 'secular religion'. In an essay published last year a cautionary note was sounded: 'We who cherish science should be careful to distinguish when we are doing science and when we are extrapolating from it, particularly when we are teaching our students. If it is science that is to be taught, then teach science and nothing more. Leave other discussions for a more appropriate time.' (M. Ruse, *Science*, 2003, **299**, 1523).

In thinking about the origins of life and evolution we would do well to remember a guiding principle in deductive logic enunciated by Sherlock Holmes (*The Adventure of the Beryl Coronet*): 'It is an old maxim of mine that when you have excluded the impossible, whatever remains, however improbable must be the truth.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Shanghai Rankings

Mirror, mirror on the wall
Who is the fairest of us all?

—The Queen in *Snow White*

Beauty contests feature prominently in the media and it is fashionable to deride them. Rating beauty is, necessarily, a subjective exercise; most of us would endorse the old saying that 'beauty lies in the eyes of the beholder'. Rankings and ratings enter every sphere of human activity, not only in the election of beauty queens. In many areas like sports, rankings of teams and sportsmen and women are relatively easy to draw up; the parameters on which the ratings are based are generally well accepted. After all, performance is easy to judge and winners and losers are clear to all interested observers. Few would argue against the proposition that Steve Waugh's Australian cricket team was the best in the business, over the last several years, or that Ferrari and Michael Schumacher are the undisputed champions of the Formula-1 racing circuit. There is an element of obviousness in ranking sporting excellence. The Cambridge mathematician G. H. Hardy was fond of ranking his fellow mathematicians. His scale was borrowed from his favourite sport, cricket. The highest grade that a mathematician could receive would be to be placed in the 'Bradman Class'; referring, of course, to Don Bradman universally acknowledged as the greatest-ever batsman in the history of cricket. Hardy's judgements were inevitably subjective, coloured undoubtedly by personal opinions. In science too, 'beauty contests' abound; every selection for an award and every election to an academy becomes an exercise in which individual and collective preferences and prejudices lead to a final judgement. Ranking scientists has become easier as quantitative indicators like journal impact factors and citation counts have become commonplace yardsticks; most judges blissfully unaware of their significance. Almost all quantitative indicators are better applied to institutions rather than individuals; averaging over large numbers of scientists and disciplines may provide a more reasonable index of comparison.

An institutional 'beauty contest', thus appears an interesting exercise. The most recent ranking of universities worldwide has appeared, curiously enough, from a group at the Shanghai Jiao Tong University (http://ed.sjtu.edu.cn/ranking.htm). Nian Cai Liu and his colleagues have used five parameters to rank universities: (i) Nobel laureates on the faculty of physics, chemistry, medicine and economics, (ii) Highly cited researchers (1981–1999) in 21 broad subject categories, (iii) Articles published in *Nature* and *Science* (2000–2002), (iv) Articles in the expanded *Science Citation Index* and *Social Science Citation Index*, (v) Academic performance per faculty, where the indices (i)–(iv) are weighted by the number of faculty. The authors provide a detailed description of their procedure and go so far as to anticipate questions about their protocol (and their intentions) and provide clear answers. For example, they ask: 'Is our academic ranking objective'? Their answer: 'Any ranking is controversial and no ranking is absolutely objective. People should be cautious about any ranking including our Academic Ranking of World Universities. Nevertheless our Academic Ranking is based on internationally comparable data that everyone could check'.

The Shanghai ranking of universities is based on the most stringent research criteria and the range of disciplines covered, presumably, permits a fair comparison of large research universities. The results are unsurprising. The top ten universities are Harvard, Stanford, Caltech, Berkeley, Cambridge, MIT, Princeton, Yale, Oxford and Columbia, in that order. Between 10 and 20 lie seven US universities, along with, Imperial College and University College London and Tokyo University. The University of Toronto at 23 and ETH Zurich at 25 are the other non-US universities in the top 25. The Shanghai group notes that data has been collected on 2000 universities and present a list of the top 500. They also provide convenient lists of the top 100 American Universities, European Universities and Asian/Pacific Universities. The last list covers a broad geographical area beginning with Turkey and Israel in the West, passing through all of Asia and includes Australia and New Zealand.

The Shanghai lists reaffirm the pre-eminence of the American research universities and the English centres of research. Five Japanese institutions, Tokyo, Kyoto, Osaka, Tohoku and Nagoya appear in the top 100. Germany and France do rather poorly with the University of Munich at 48 and the University of Paris-VI at 65, being the highest

ranked. Curiously, the highest ranked institutions listed under China are National Taiwan University (152–200, the range indicating degeneracy) and the Hongkong University of Science and Technology (251–300).

When I pored through these rankings, and admittedly there is something compelling about lists and ranks, my purpose was to find Indian institutions. It took me a while to locate the highest ranked among our institutions, the Indian Institute of Science (IISc), Bangalore, which appears at 251–300. The only other institutes that appear in the top 500 are IIT Delhi and IIT Kharagpur, both weighing in at 451–500. On the Asia/Pacific list IISc comes in at 27–36, while the two IITs are listed at 78–92. The rankings are indeed based on stringent criteria which heavily weigh research performance as judged by high quality publications. It is clear that if more honours, in addition to the Nobel Prize, or more journals, in addition to *Nature* and *Science*, were considered, the list would remain substantially unaltered. If I had been asked to list the top ten research universities in the world, my list would have largely agreed with the Shanghai list. But, as we move further down, ordering on the basis of personal opinion (prejudice ?) becomes more uncertain. There are some notable features of the Shanghai rankings. Six Australian institutions (Australian National University, and the universities at Melbourne, Queensland, Sydney and Monash and New South Wales) appear ahead of IISc. Clearly, Australia has more than sport on its mind. Seoul National University (South Korea) and the University of Auckland (New Zealand) are also marginally ahead.

What then, is the message of the Shanghai lists? It is clear that research output at the best of Indian institutions does not compare well on an international scale, in this analysis. Since the Chinese researchers provide a detailed break-up of the individual scores on their five parameters, I quickly examined the one which appears most widely used – the number of articles in the *SCI* and *Social Science Citation Index*. Even with this criterion, which minimizes the effects of 'impact, citations and recognition', it does not appear that the positions will change appreciably. A somewhat distasteful conclusion would be that in measuring research activity over large numbers of faculty in universities, quality and quantity are correlated. The growing tendency to dismiss publication-based analyses of the country's scientific output as an inappropriate measure of the health of our scientific institutions must be tempered. The Shanghai study excludes specialized institutions and laboratories, of which we have many. Despite the UGC's rather liberal award of the term 'deemed university' to many of these institutions, most would fail to qualify for the category of a 'research university', characterized by activity across a range of disciplines. Sadly, the real universities in India are limping, research consigned to an unimportant role. Even as funding has increased for many of UGC's 'five star' universities, there are evident problems, with faculty disinterested in research clearly outnumbering those with an academic bent of mind. Many Indian universities no longer directly involve themselves in undergraduate education; the process of divestment to constituent colleges is almost complete. University departments must therefore be postgraduate centres of research and scholarship, as indeed they were fifty years ago. Even in the IITs, research is a low priority; the major emphasis is on undergraduate technical education and the major instrument of success is the Joint Entrance Examination (JEE).

I found the Shanghai rankings provocative and disturbing. A widespread analysis of these lists might prove instructive. It is clear (and indeed, it has been so for sometime now) that the academic ambience of our large institutions is decaying. Stemming the rot may be a formidable task. The government which funds these institutions, and the bodies that govern them need to take a long, hard and critical look at the higher education scene in India. A disproportionate amount of effort is spent on self-congratulatory analyses of Indian Institutes of Management and IITs. 'Brand equity' is a term that is based on the performance of students produced, it does not reflect the research activity at these institutions. Progress in an increasingly competitive world will depend on a measure of original research activity. The new government, which is still to take office, at the time of this writing, has many complex tasks to consider. The state of higher education and research is one. The Shanghai 'beauty contest' has served a purpose. It has provided a dispassionate ranking of universities worldwide. We must now decide whether we should execute the messenger.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Governance: Science and Technology

The installation of a new government is an appropriate time to think of governance. The post-election scene is always dominated by an unseemly scramble for power and portfolios; the business of governance seems relegated to the background as both politicians and bureaucrats (technocrats among them) jockey for positions of advantage. Elections which signal a change of political leadership, as this one has, lead to a much greater period of uncertainty regarding policies and priorities. But once the dust has settled, the routine business of governance resumes. While some sections of the media focus excessively on the stock markets and financial policy, there are many aspects of governance which attract very little public attention. For readers of this journal, science and technology (S&T) and higher education may be areas of special interest. For many years science and technology was an area in which prime ministers took a special interest; but over the last fifteen years the perceived importance of this portfolio has declined dramatically. Education used to be watched over by a Ministry of Education; but a change of title to Ministry of Human Resource Development (MHRD) was effected a long time ago; a move that seemed to signal a degree of ambivalence about the relationship between education and human development. The domains of both S&T and higher education are beset with problems that need attention.

The previous government enunciated an S&T policy in 2003; a document (S&T-2003), which stated objectives and outlined a 'Strategy and Implementation Plan'. However, in the year that has passed since its public unveiling at the 2003 Indian Science Congress, there has been little sign of any move to implement any of the stated objectives. S&T-2003 declares: 'A concerted strategy is necessary to infuse a new sense of dynamism in our science and technology institutions. The science departments, agencies and other academic institutions, including universities, i.e. the science and technology system as a whole, would be substantially strengthened, given full autonomy and debureaucratized'. Implicit in this statement is the acceptance of the fact that our institutions need rejuvenation. S&T-2003 goes on to add: 'Mechanisms will be established to review on a continuous basis the academic and administrative structures and procedures in the science and technology system at all levels, so that reforms could be effected to meet the challenges of the changing needs'. Unfortunately, since its launch, S&T-2003 appears to have been consigned, like all policy documents, to oblivion.

The health of our research institutions has been the subject of some debate; many quantitative indicators suggesting that research productivity has stagnated. The opponents of publication statistics have been quick to react, setting in motion studies of alternative indicators for the health of our S&T establishment. It will be interesting to see the results of these analyses and the indices on which they rest. The science advisory mechanisms which assist in the governance of the S&T sector have been largely ineffective. The Science Advisory Committee to the Cabinet (SAC-C) has had little to show for its efforts; establishing a well-known fact of governance, that instruments that lack executive authority are doomed to irrelevance. The attempt to expand the interface between government and the scientific community by involving the science academies has not been a success; with the academies often electing presidents, who also serve in the executive arm of government, thus precluding independent inputs. The profusion of academies also ensures that government sees the scientific community as a formless, heterogeneous group, whose concerns are diverse and poorly articulated. In this scenario, the spokesmen for science are the secretaries to the government S&T departments, who also have the responsibility to implement policy. Unsurprisingly, the enthusiasm for change and reform is minimal in the various departments that constitute the Ministry of Science and Technology.

The practice of science and technology in India is mainly divided amongst three major classes of institutions. The strategic departments, Defence Research and Development Organization (DRDO), the Department of Atomic Energy (DAE) and the Indian Space Research Organization (ISRO) are largely insulated from public review and budgetary fluctuations. The national laboratories, with the Council of Scientific and Industrial Research (CSIR), the Indian Council of Agricultural Research (ICAR) and the Indian Council of Medical Research (ICMR) being the most visible, constitute a second category. Many of the institutions that come under this

group struggle with problems of limited research output, an aging scientific workforce and management practices that are an anachronism in the present context. Finally, there is a third category; a large body of academic institutions including universities (deemed and real), funded centrally and by the states. Within this class, public perception and scientific output varies widely. The levels of funding and the management structures also show considerable variations; the academic ambience and environment for research can be dramatically different. As old institutions, many of our most famous universities among them, have declined as centres of research, new institutions have been created. These have generally been small, focussed in specific areas and generously funded, creating small islands of academic strength, in an otherwise indifferent environment.

A major and immediate cause of concern is the state of many national laboratories and universities. Once productive and flourishing departments have decayed; a degeneration witnessed and sometimes encouraged by successive governments at the centre and in the states, which are responsible for appointing and supporting those who govern these institutions. In the universities the inability to recruit a new generation of enthusiastic and competent faculty, with strong research interests, has been debilitating. S&T-2003 states, rather optimistically: 'Flexible mechanisms for induction of new faculty in key areas of science would be developed. Constancy of support and attention will be ensured over at least a ten-year period'. It may be time to put in place a scheme for underwriting salary and research support for 'new blood' faculty in many of our university departments, which have been decimated by retirements and neglect. The issue of new 'central' initiatives in state-run universities must be viewed afresh, with the UGC and AICTE promoting a culture of research vigorously.

S&T-2003 begins with the then Prime Minister, Atal Bihari Vajpayee, quoting Jawaharlal Nehru: 'Scientists are a minority in league with the future'. But he adds: '... let us remember that a bright future can be realized only when science is in league with the majority of our society'. There are many areas where modern science may have a direct impact on large sections of our predominantly deprived society; agriculture and health are preeminent among them. The full potential of emerging technologies which stem from the dramatic advance of modern biology needs to be tapped. To do so the connections between laboratories which emphasize basic research and the agencies within the Health and Agriculture ministries, ICMR and ICAR, need to be strengthened. The various ministries and departments do not always coordinate activities effectively; collaboration and group efforts will be necessary to bring new vaccines to trials and to address the many contentious issues raised by the introduction of transgenic crops.

Successive governments have reaffirmed their commitment to promoting science and technology in India and to enhance government spending on science. The growth in the budget for scientific research and the consequent increase in the number of researchers have strained funding mechanisms. Despite several well-intentioned efforts, the administrative and financial procedures at the funding agencies and recipient institutions remain mired in bureaucratic tangles. S&T-2003 promised to set up 'more efficient funding mechanisms... either by creating new structures or restructuring the existing ones, for promotion of basic research in science, medical and engineering institutions. In particular, administrative and financial procedures will be simplified to permit efficient operation of research programmes in diverse institutions across the country'. S&T-2003 does consider the need for 'innovative fiscal measures to ensure successful implementation of the policy objectives'. Enhanced private investment in the S&T sector must be promoted, if innovative applications of research are to become more common.

Governance is a difficult and demanding task. Policies and visions are easier to enunciate than to implement. The challenge before those who accept responsibility is to do the best that is possible, with the greatest commitment to the tasks at hand.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Training a new generation

June is the month when college and university admissions are in full swing. The results of most entrance examinations have been declared and career choices loom before the prospective entrant to our institutions of higher education. June brings both the monsoon and examination results, which often cast a pall of uncertainty over the lives of hundreds of thousands of students. June also brings hundreds of prospective students to the gates of our research institutes in search of an opening that will permit them to enter scientific research. At the Indian Institute of Science (IISc), Bangalore the largest numbers of students congregate, in disciplines that lie as far apart as biochemistry and mechanical engineering. In a week of interviews, students who have qualified through a variety of screening examinations, are assessed for their suitability, with only about 10% of the candidates eventually obtaining a passport for admission. This is a process played out, albeit on a smaller scale, at many other prestigious institutions across India. A striking feature of the students who come to IISc (and, indeed, many of our national institutions) is that they are truly representative of India's diversity; their degrees having been obtained at universities from Kerala to Kashmir and Gujarat to Arunachal Pradesh. Irrespective of origins, academic accomplishment and skills to communicate, the students are desperate to get a foothold in research. The wide variations in the Master's degree programs at Indian universities, places those who emerge from some institutions at a disadvantage; their exposure to their chosen fields limited by inadequate facilities and teaching. But, for the interviewer (and I have just played that role), the week of questioning prospective students can provide some insights, not all of them comfortable, into India's sprawling higher education system, with a focus on science.

University science departments, and I believe there must be very few exceptions, have become academically fossilized, with internal debate almost invariably focused on the politics of administration, to the exclusion of discussions on modernization of courses, laboratories and student training. Dedicated teachers are rarely recognized by university administrators, who masquerade as academicians of distinction. In many departments, even the mindless system of 'rotation' of administrative responsibility has been unable to provide brief interludes, where academic issues are purposefully addressed. In the West, administrators in universities and academic institutions are judged by the collective successes of their academic colleagues; in India they seem to be beyond assessment. In some fields, the absence of academic discourse is most evident, because of the great pace of change in these areas. Biology is a dramatic case. Traditionally, universities have had botany, zoology and microbiology departments. Biochemistry, although a century-old discipline, has rarely merited a separate department in many universities. The high tide of molecular biology has changed everything. The unity of biology was never more evident; plants, animals (humans among them) and microbes share a genetic and biochemical heritage, rendering disciplinary borders, both porous and inconsequential. Yet, our universities have erected impregnable walls between departments; a situation for which academics alone must bear responsibility. The mushrooming of 'biotechnology' departments has further diluted the training of a new generation of students. In some places, one of the biology departments is usually magically transformed, becoming an automatic recipient of government grants aimed at promoting biotechnology. In the newer universities, omnibus 'life sciences' departments were created, which have failed to develop strengths in traditional areas, while struggling to carve a niche in modern biology. Teaching has been a major casualty; interviews with the products of this system quickly reveal that little of the excitement of contemporary science has been conveyed to students. The situation is not very different in areas like physics and chemistry, where changes have been less turbulent over the last thirty years.

The research institutions, which admit students with highly variable backgrounds, make little attempt to introduce rigorous pre-Ph D course programs, a feature that American research universities have so successfully implemented for decades. Teaching is not an activity that commands much respect, within our research departments and deemed universities. Even while bemoaning the limited training and exposure of new entrants to re-

search, scientists and faculty at our most prestigious institutes view potential students as 'technicians', whose exertions serve primarily to advance the careers of their supervisors. The long-range goal of training the next generation of scientists is not discussed with seriousness in the academic bodies of these institutions; indeed many Ph D granting establishments even lack the formal supervisory academic mechanisms that ensure the standards of Ph D degrees. The divorce between postgraduate teaching and research is almost complete in India; efforts to redress this situation by creating 'National Institutes of Science' may flounder because of difficulties in recruiting motivated faculty in adequate numbers at diverse locations, some of which may prove inhospitable for institution building.

Indian research institutions also compete with Western universities to attract the best of our B Sc and M Sc students to research programs. Institutions in places like Singapore and Australia have also begun to look attractive to prospective researchers. The need to induct the most promising students into research has attracted the attention of universities worldwide. The attempts to reform undergraduate science education have been vigorous in the US. A news item in *Science* (2004, **304**, 810) highlights a report of a panel of 'students, faculty and administrators' at Harvard University that 'urges the university to give its undergraduates a genuine view of the excitement of research science'. These initiatives aim to 'provide a more interdisciplinary approach to life and physical sciences'. Harvard is only one among many North American universities to continually worry about reforming undergraduate science education. In India, there have been many laudable attempts to promote summer research internships that permit B Sc/B Tech and M Sc students to experience the atmosphere of research laboratories, during vacation periods. The two national programs, which together support between 200 and 300 students for two-month periods every summer, are run by the Indian Academy of Sciences and the Jawaharlal Nehru Centre for Advanced Scientific Research, both based in Bangalore, with financial inputs coming from the Department of Science and Technology. These programs are a wonderful opportunity for a prospective student to gauge the requirements for research. Most degree-granting institutions at B Tech/M Sc levels have also introduced 'project work'. Unfortunately, most such institutions do not have facilities or faculty directly interested in research. Students need to fend for themselves, searching for placements, where 'projects' can be executed. The situation is scandalous in the area of 'bioinformatics' and 'biotechnology', with institutions, which charge high fees, providing no facilities for mandatory 'project work'. Although many of these courses and institutes have received formal recognition and accreditation, academic norms appear to be observed only on occasion.

An experiment which began some years ago at IISc was designed to attract students into research by offering admission to Ph D program immediately after a B Sc degree. This 'integrated Ph D' model has found some favour in a couple of other institutions. The program requires a mandatory course load, laboratory rotation and a research project before settling into a doctoral thesis project. The program caters to a very small number of students, some of whom leave midway to enrol in universities overseas. But these programs have consistently attracted good students, suggesting that inductions into research can be successfully accomplished after our undergraduate degrees. There are some obvious benefits to students who may graduate a bit earlier, having slipped into research without the intermediacy of a two-year M Sc program. For faculties, who are loath to teach, these programs appear to be an extra burden; few realize that teaching and research are two sides of the same coin. In the overwhelming majority of institutions, research degrees appear to have become a private contract between students and research supervisors ('guides'); departmental requirements and responsibilities are minimal. If standards of post-graduate education and research are to improve, it is necessary that universities and research institutions critically evaluate their academic programs. Institutions which treasure autonomy must also awaken to their responsibilities.

Science affects almost every aspect of modern life. Training the next generation of scientists is a pressing task. The constant lament about the flight of students from science has become a convenient excuse. Clearly, not enough is being done for those who do enter science courses.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Smell, science and the press

But 'tis my heart that loves what they despise
Who in despite of view is pleased to dote
Nor are mine ears with thy tongue's tune delighted,
Nor tender feelings to base touches prone,
Nor taste, nor smell, desire to be invited
To any sensual feast with thee alone.
But my five wits nor my five senses can
Dissuade one foolish heart from serving thee,

William Shakespeare (Sonnet 141)

The five senses of sight, hearing, smell, touch and taste are taken for granted; rarely do we pause to wonder at our marvellous abilities of sensory perception. It is only when one of these gifts of nature is impaired, that we worry about the disabling effects of conditions like blindness and deafness, the most common disorders of our sensory system. Disabilities of the senses of touch, taste and smell are far more uncommon. At one of the many sites on the internet which focus on the sensory system, I found an on-line poll, querying visitors on the order of importance of our five senses. Predictably the votes cast led to the order: sight (66%), touch (11%), sound (8%), taste (8%) and smell (4%). Clearly, an overwhelming majority of voters did not consider a loss of the ability to smell as a major disorder; at times the absence of the sense of smell might even be considered an advantage. Anosmias or disorders of the olfactory system do occur and can be a significant handicap for affected individuals. My attention was drawn to the area of 'smell research' by an unusual editorial in a recent issue of *Nature Neuroscience* entitled 'Testing a radical theory' (2004, **7**, 315). The editorial justified the publication of a paper in the same issue which was a 'refutation of a theory that, while provocative, has almost no credence in scientific circles'. Why then, did *Nature Neuroscience*, one of the leading journals in the area of neurobiology, decide to publish a paper that appeared to be demolishing a theory that no one believed anyway? The editorial argues that the decision to publish the refutation was influenced by 'the extraordinary – and inappropriate – degree of publicity that the theory has received from uncritical journalists'. The discredited theory, enunciated by Luca Turin (then, at the University College, London) in 1996, proposed that olfactory ('smell') receptors 'respond not to the shape of the molecules but to their vibrations'. Turin suggested that 'inelastic electron tunnelling' provided a 'plausible mechanism for biological transduction of molecular vibrations'. Turin appeared to argue that the nose (and its plethora of receptors) appear to function as a 'biological spectroscope', carrying out 'inelastic electron tunnelling spectroscopy'. Turin's thesis challenged the dogma that biological receptors respond to specific molecular shapes of their ligands. The shape theory of molecular recognition was enunciated by the German chemist Emil Fischer, over a century ago, when he described a 'lock and key' mechanism for enzyme action. The range of odours that can be distinguished by humans appears to be very large; at present it is not clear if the number of genes that code for olfactory receptors will also be very large or if mechanisms exist to enhance the recognition repertoire.

Turin departs from this view of olfaction to propound a more 'physical' model for differentiating odorant molecules. In his paper published in *Chemical Senses* (1996, **21**, 773–791), Turin notes that the simple chemical, acetophenone, and its fully deuterated analogue, acetophenone-d_8, 'smell different despite being identical in structure'. A substitution of hydrogen atoms by the heavier isotope, deuterium, would be expected to affect molecular vibrations. The dismissive editorial in *Nature Neuroscience* notes that his paper was rejected by *Nature*, and it was eventually published (without review, according to Turin's own account) by *Chemical Senses* in 1996. For readers unacquainted with the area, *Chemical Senses* is a specialist journal published by Oxford University Press, with impact factors of 2.6 and 2.5 in 2001–2002. The blurb on the journal's website places it as the top journal in the area of Food Science and Technology, out of a total of 92 journals in this subject area.

The formal discrediting of Turin's work is marked by the publication of a paper by A. Keller and L. B. Vosshall, from the Rockefeller University (*Nature Neuroscience*, 2004, **7**, 337–338). Entitled 'A psychophysical test of the vibration theory of olfaction', the Keller–Vosshall paper

uses a simple protocol of 'smelling' by volunteers and a 'double blind protocol' for odorant tests and concludes that there is 'no evidence that regular and deuterated acetophenone smell different to naive subjects'. The authors however add a cautionary note, that they 'cannot exclude the possibility, however, that olfactory training or experience could alter the outcome of the tests done here'. The editors of *Nature Neuroscience* are less restrained and conclude magisterially: 'A mature body of a scientific theory is like a large building, and the impulse to demolish it is often little more than a form of intellectual vandalism, an expression of frustration by those who did not succeed as architects. Some buildings outlive their usefulness, of course, but the threshold for knocking them down should be high. We hope that the paper from Keller and Vosshall will serve as a reminder of why it is so.'

The editors of *Nature Neuroscience* and the olfaction orthodoxy appear to have been enraged by the almost uniformly approving reviews in the press, received by a book about Turin, his theory and his jousts with the scientific establishment. *The Emperor of Scent: A Story of Perfume, Obsession* and the *Last Mystery of the Senses* by Chandler Burr appeared in 2003 (Random House, New York). I have not read the book, but have read several reviews, all of which suggest that it is eminently readable. Turin's treatment by his peers in the scientific community will strike a chord in many researchers, who quixotically tilt, most often in vain, at windmills. Turin's obsession with the mechanisms of odour discrimination appears to be of some interest to the $ 20 billion perfumery industry, which jealously guards its molecular secrets. Blending mixtures of odorant molecules is indeed the key to the making of perfumes. In Turin's story there is a touch of the underdog versus a powerful and hostile establishment. Unsurprisingly, Turin's experiences as reported by Chandler Burr and the reviewers of his book, show the scientific community's peer review mechanisms in poor light. Understandably, the *Nature Neuroscience* editorial reflects the establishment's dismay: 'This (controversy) is not just about olfaction. It is about the public credibility of the scientific process and the biases that affect science reporting in the popular press.'

In the West, many of the major newspapers and magazines report with considerable accuracy and depth on major advances in science. This is an essential element in enhancing the public understanding of science and in justifying a high level of public commitment to the support of science, scientists and their institutions. In India coverage of science in the press is much less discerning. Very few newspapers and magazines have 'science writers' on their staff. Reporters with little or no knowledge of the subject they write on are assigned the task of reporting on issues of science. The practice of 'researching a topic' is largely unheard of among science journalists, although there are, of course, a few notable exceptions. Science writers in India are often uncritical recipients of doubtful information from contacts within the scientific community. These are then reported as established facts. Some recent stories that I have seen would make Turin's theories and claims appear innocuous. A report in the *Sunday Express* (27 June 2004, p. 32) announces the invention of a 'Nano Geneseq Chip' that 'analyses the entire future genetic proposition of a human at birth itself'. The report talks of a 'genetic horoscope' and holds out possibilities of diagnosing and treating 'a range of disorders, most importantly HIV/AIDS and cancer'. This piece is an example of the kind of gibberish that appears in our newspapers, often misleading gullible readers into believing that major scientific advances have been made in local institutions. Even as I was reading about olfaction and Turin I came across another report in the Bangalore papers. On 4 July 2004 *The Sunday Express* reported that 'city doctors wield magic ward, treat terminal cancer patient'. A new technique called 'Rotational Field Quantum Magnetic Resonance (RFQMR)' was announced and advanced for 'treating osteoarthritis, osteoporosis, joint diseases, migraine and degenerating and destroying tumours'. *The Hindu* (4 July 2004) carried the same story with a more conservative headline: 'A "breakthrough" in cancer treatment'. This report noted that 'clinical trials will shortly begin for establishing the technology's efficacy for treating cancer'. In many such reports the process of scientific review is far from complete, the results of studies unpublished and the efficacies of treatment unestablished. Nevertheless, the press actively promotes the impression that 'cures' are at hand; often raising false hopes in patients who are desperately ill. Sometime ago, unrestrained publicity for stem cell research led to the impression that some Indian laboratories were ready with therapeutic applications. The tendency among scientists and institutions to exploit a lazy and uninformed press, to highlight claims that later on do not stand scientific scrutiny must be deplored.

Scientists and the press have a responsibility to be accurate and circumspect in publicly reporting scientific findings, especially those that have a direct bearing on public welfare. Even in esoteric areas the press can play a role in shaping public perception. The case of Luca Turin and the theory of olfaction is a good example.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Sprinting before we stand

Mass spectrometry is an old technique. The history of the method goes back to the dying years of the 19th century and J. J. Thompson's work on the electron. Old textbooks recount Aston's work with the mass spectrograph. In the 1950s and 1960s mass spectrometry became a method of some importance in chemistry; affording a reliable means of determining the masses of molecules and information about atom connectivity by fragmentation of molecules, upon impact with energetic electron beams. For decades, mass spectrometry was the province of specialists, who spent much of their time in maintaining formidable pieces of equipment, perennially worrying about high vacuum systems. Since measurements required molecules to enter and ionize in the gas phase, the polar, high molecular mass substances, which abound in biology, were off limits to mass spectrometry. A few determined chemists toiled on derivatization methods to enhance the volatility of high molecular weight materials; a task which proved largely unrewarding. The first stirrings of a revolution were visible in the 1980s when new methods of ionization, electrospray techniques and matrix-assisted laser desorption, were introduced. Suddenly, mass spectrometry was ready to be pressed into the analysis of biological molecules; a flood of novel applications flowed in the decade of the 1990s, culminating in the award of a Nobel Prize to John Fenn and Keiichi Tanaka in 2002. Mass spectrometry quickly became the method of choice to analyse the 'proteome'; a term commonly used to describe the total complement of proteins in an organism. The rapid advances in genome sequencing projects meant that translation of gene sequences would yield the sequences of 'virtual proteomes'. Bioinformatics approaches might be expected to provide clues to the biological functions of proteins; although the task of 'functional annotation' has proved somewhat difficult. Since proteins are plentiful in body fluids and tissues, experimental analysis of protein profiles might be expected to be an indicator of biological status. Simply put, it might be possible to differentiate normal and disease states by analysis of subsets of the total proteome. Even more tantalizing is the prospect of developing protein markers, which might signal the early onset of disease; permitting clinical intervention well before physical symptoms are manifest. Inevitably, mass spectrometric analysis of body fluids is being advanced as the method of choice for proteomic analysis. This superficial primer on mass spectrometry and the proteome would be out of place in these columns if it had not been for a news feature entitled 'Running before we can walk', that appeared recently in *Nature* (2004, **429**, 496).

The *Nature* report highlights a proteomic test for cancer, described in a 2002 paper entitled 'Use of proteomic patterns in serum to identify ovarian cancer' (Petricoin, E. F. *et al.*, *Lancet*, 2002, **359**, 572). This paper appears to have prompted a US Congressional resolution 'urging continued funding to drive a new diagnostic test towards the clinic'. Good diagnostic tests must overwhelmingly identify samples from patients known to suffer from disease and have an acceptably low level of 'false positives', in control samples. The test reported by E. F. Petricoin, and L. A. Liotta, the lead authors of the *Lancet* paper, seemed to fulfil these criteria; the first steps to commercialize followed. Unfortunately, independent analyses quickly cast doubt on the claims of the *Lancet* paper, suggesting that the differences in serum mass spectral profiles of cancer patients and normal controls were artifacts and not a result of genuine differences of biochemistry. The critics used raw data available on an Internet site. Curiosity drove me to the original *Lancet* paper and the mass spectral data on the NIH/FDA Clinical Proteomics Program Databank (http://clinicalproteomics.steem.com). Reasonably familiar with conventional mass spectrometry, I was bemused to find digitized spectra, with intensity data reported at unreliable low mass values; a range where the major differences lay between normal and control samples. The language of the spectroscopist had been discarded for the sleight of hand of the statistician. My naive conception of the proteome was conspicuously missing in the 'proteomic patterns' of the *Lancet* paper. Mass spectrometry of a complex biological mixture was reduced to a 'fingerprint'; clinical diagnostics suddenly seemed to be a sophisticated pattern matching game, with proteins, genes and biological chemistry receding into the background. The *Nature* report's title

'Running before we can walk' seemed conservative; 'Sprinting before we can stand' seemed more appropriate. The waters were further muddied when the principal authors of the *Lancet* paper were implicated in a controversy involving inappropriate consulting practices, in a US Congressional hearing.

By itself, the cancer proteomics diagnostic controversy would be only another one of the many contentious scientific debates, that occur with great frequency, in high profile areas of biomedical research. The affair, however, raises a more general issue; the growing tendency to rush headlong into the application of sophisticated instrumental methods to clinical and biological problems, with complete disregard for the technical issues involved. Modern biology is driven by extremely sophisticated equipment and expensive reagents, making competitive research a very costly business. NMR spectrometers, mass spectrometers, DNA microarray scanners, DNA sequencers, confocal microscopes, X-ray diffraction equipment, cell sorters and an assortment of separation and analytical equipment are common currency in modern biological research. Superconducting magnets, lasers, high vacuum systems and optical devices which were once found only in physics departments are now integrated into user-friendly, commercially available instruments. The advertisement pages of many journals are a testimony to the technical sophistication that has come to be commonplace in biology research today.

In the last few years the pace of acquisition of major instruments in university science departments and national laboratories in India has increased substantially. This is a result of enhanced budgetary support, with the FIST program of DST providing a special thrust in the academic institutions. Equipment ranging in cost from $100,000 (Rs 45 lakhs) up to, and sometimes beyond, $1 million (Rs 4.5 crores) have been purchased. But in most centres installation has been slow, with local infrastructure like power, air-conditioned rooms and sometimes, even water being rate-limiting. Progress after installation has also been slow, with the lack of trained technical staff to man facilities, being the most frequently cited excuse. The absence of a cadre of trained, well-paid technical staff is a characteristic of Indian academic institutions. The contribution made by the new facilities to uplifting the quality of ongoing research programs has been limited by the inadequate appreciation of the necessity to learn techniques well, before applying them. The absence of a culture of academic collaboration and the technical innocence of many who are keen to apply the latest tools to their pet problems, further hampers rapid progress. In many places the 'engineers' of the companies that sell equipment are the major advisors to facilities; a situation similar to that in the medical profession, where representatives (invariably unqualified) advise doctors on the virtues of the most recently introduced drugs. But the most disconcerting problem is the tendency of many researchers, students and senior scientists among them, to assume that expensive and sophisticated equipment are the key to solving scientific problems. The need to acquire a minimal degree of technical competence is never emphasized; the ability to look critically at data generated by instruments is rarely stressed, even to beginning research students in India. Many researchers approach major facilities, following an old principle enunciated in his Nobel lecture by one of the founders of chromatography, A. J. P. Martin: 'Nothing is too much trouble if somebody else does it'.

The growing body of uncritical, technically inept professionals is a matter of some concern. In the area of biomedical research (and in other high profile areas like nanotechnology) there is a tendency to claim great success at the slightest provocation. How many diagnostics and vaccines have been launched in the newspapers and at ministerial functions? With the latest tools of modern biology becoming available and with genomics and proteomics becoming commonplace, there will be many claims of dramatic success in these fields in the near future. How can these be assessed in an environment, where the critical faculties are often suspended? The cancer proteomics story with which I began this column is a pointer to the urgent need to promote critical and competent analysis to assess the claims of new technologies; the ability to distinguish fact from artifact must be prized. Even more importantly, it may be essential to enhance the technical training imparted to new students in Ph D programs, so that they are able to cope with the increasing sophistication of instruments in many tumultuously advancing fields. We must learn to stand before we sprint.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Measure and Mismeasure of Science

In 1997 Robert May, then the British government's chief scientific advisor, authored a commentary entitled 'The Scientific Wealth of Nations'. May's analysis began thus: 'The United States took much pleasure from its performance in the Olympic Games, where it won many more medals than any other country. But was this the right measure of performance? Counting four points for gold, two for silver, one for bronze, and calculating the score relative to population size, a different picture emerges. Tiny Tonga was first, Australia led among the larger economies, and overall the United States ranked 37th, well behind most of the European countries (but not the United Kingdom, a lamentable 48th).' May was, of course, unworried about the Olympics; he was focussing on measuring science, stating clearly his intent: 'similar questions arise when we ask about the quality of the scientific research output of countries. For many purposes, most notably overall advance in our understanding of nature, it is total output that matters' (*Science*, 1997, **275**, 793). May's study was substantially based on an analysis of Australian research, which used the *Science Citation Index* based on coverage of '4000 journals since 1981'. He ranked the top 15 countries, with India coming in respectably at position 8, edging in ahead of Australia, The Netherlands, Sweden, Switzerland and China. India's share of papers was 2.4%, its share of citations 0.7% and its share of GDP spent on R&D was listed as 0.7%. The relative citation impact (RCI, citations divided by publications) was low for both India and China, 0.27. The two most populous countries ranked 66 and 65 if RCI was used as an index of scientific performance. Some of May's conclusions based on his data bear retelling. He suggested that the lower ranking of some G7 countries like Germany and France relative to North America or UK may be related to the fact that in the former, basic research is largely done in research institutions (Max Planck and CNRS institutes), whereas in the latter, research is largely the province of the universities. I rather liked his inference: 'The non-hierarchical nature of most North American and northern European universities, coupled with the pervasive presence of irreverent young undergraduate and postgraduate students, could be the best environment for productive research. The peace and quiet to focus on a mission in a research institute, undistracted by teaching or other responsibilities may be a questionable blessing'. May's suggestions could well be relevant in India, where science policy is overwhelmingly dominated by those who are votaries of research institutions, with rigidly defined hierarchical structures. Irreverence would hardly be a prized quality in these surroundings.

May's commentary is over seven years old and I would normally not have revisited it, if it had not been for yet another analysis of 'the scientific impact of nations'. The author this time is David King, May's successor in the British Office of Science and Technology. The raw data remains the number of published research papers and their citations, as logged by the *SCI* (*Nature*, 2004, **430**, 311). In addition to the total number of papers and citations, King uses a more selective parameter; the number of and citation to the top 1% highly cited publications based by division into seven disciplinary categories: clinical medicine; preclinical medicine and health; biological sciences; environment; mathematics; physical sciences; and engineering. King presents data for two periods, 1993–97 and 1997–2001, allowing a view of the growth of scientific output. King notes: 'Governments also need an indication of disciplinary strengths and weaknesses based on international comparisons. Comparing one discipline across different countries should be easier than comparing two disciplines within one country'. The dangers of cross-disciplinary comparisons of scientometric data are often not apparent to many decision makers in India; even more sadly, most scientists do not seem to sense the pitfalls of such comparisons. King represents the enormous data generated by the 'national and disciplinary disaggregation process' as a radial plot, which produces an image of the 'national footprint' in science. According to King: 'The larger the national footprint, the bigger the impact on international science'.

The numbers of papers and citations recorded in this study are staggering; for the period 1997–2001 there are 3,631,638 papers aggregating 41,425,399 citations. The analysis lists 31 countries, which together account for

98% of the world's highly cited papers. King notes, without comment, that the 'remaining 162 countries contributed less than 2% in total'. The 2004 rank order places India at position 22, China at 19, South Korea at 20 and Russia at 16. Brazil at 23 is the sole representative from South America, South Africa at 29 is the only African country and Iran at 31 represents the 'Islamic countries', a classification that appears to be gaining a measure of prominence. Predictably, the United States, Britain, Germany, Japan, France, Canada and Italy are the top 7 countries. Interestingly, this list of 7 is unchanged, when compared to May's 1997 analysis. Notably, Switzerland, The Netherlands, Australia, Sweden and Spain make a strong showing, completing the top dozen countries. King's ranking of nations on the basis of scientific impact, comes closely on the heels of the 'Shanghai ranking' of universities worldwide (*Curr. Sci.*, 2004, **86**, 1347). The tools of scientometrics appear to now provide global comparisons, which merit attention, particularly from those charged with the responsibility of deciding policy initiatives to strengthen the science base in India. A notable feature of King's analysis is an attempt to compare private R&D spending, Ph D production and numbers of full time researchers as a fraction of the population of employed persons. Unfortunately, he provides figures only for the G8 countries, with the statistics for Russia largely missing. These indices complement publication statistics and should encourage Indian analysts to gather and sift through similar data. King emphasizes the utility of these figures: 'The knowledge base and economic fortunes of a nation are maintained and developed through the production of highly trained people. This is a necessary but not a sufficient condition for sustainable economic development: both political and macro-economic factors including infrastructure investment must also be in place' (*Nature*, 2004, **430**, 311).

The discussion on India's scientific output as measured by publications has been going on for sometime in the pages of this journal. The observation that *SCI* indexed publication numbers have been stagnant for some years now, in contrast to high growth rates for China and South Korea has met with responses, which have ranged from the indifferent to the hostile. One view has been that such rankings and statistics are irrelevant; Indian science must address local problems, looking inward in a repudiation of the tendency to make global comparisons. Another, and a more forceful view, is that these indicators are not a true measure of the impact of Indian science; rather we must search for a new set of indices which will provide a different picture. A new game has begun, with groups attempting to define a fresh set of indicators. The motivations are clear; let us redefine the parameters so that obvious indicators of technological success can be factored in. Points must now be assigned for the successes in fields like defence, atomic energy, space technology and agriculture. This urge to search for indicators which will show us in better light is fuelled by the insecurity of science administrators, who are largely drawn from the institutions charged with achieving specific technological goals. Published papers are obviously not a good index of success in these areas. What needs to be emphasized is that published output is indeed the only measure by which academic science can be evaluated on a national or global scale. It must also be admitted that a large fraction of work in our network of national laboratories (CSIR, ICMR, ICAR and the DBT/DST institutions) is academic in nature. Even within the Department of Atomic Energy there is a large amount of basic research, which can be judged only when published. The universities and academic research institutions, of course, are intended to do research that can be published. A decline in output or stagnation is then a matter of concern, especially when an upward trend is observed for other rapidly developing countries. If our stated intention is to improve the quality and quantity of scientific research output, then steps must be taken to ensure a higher level of scientific activity, more evenly across the country. The pressures of competition must be felt in a much larger number of academic institutions than at present; academic positions cannot become a sinecure for non-performers in research. Even more importantly, the right signals must emanate from administrators of science. Searching for new measures of science may be a diversion best left to academic analysts; government would be ill-advised to base policy initiatives on newly minted indicators.

Many years ago I came across a book by Stephen Jay Gould entitled *The Mismeasure of Man* (W. W. Norton & Company, New York, 1981). Gould, in his characteristically compelling fashion, had launched an attack on the measures of human intelligence, studies he believed (and many think, incorrectly) were driven by racist motivations. Initially, I was puzzled by the word 'mismeasure', which did not occur in my dictionary; although 'mis' is listed as a prefix meaning 'wrong'. Gould undoubtedly viewed physical estimates of cranial capacity and intelligence tests as mismeasures of man. In thinking about measuring science, Gould's phrase suddenly came to mind. We must be watchful of mismeasures of science.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Sport and Science: Olympic Musings

The Olympics are a compelling spectacle. Since the invention of the modern version of the Games, by Baron de Coubertin in 1896, the Olympics have come to signify the ultimate in international competitive sport. The events are diverse, providing opportunities for demonstrating individual talent, perseverance and discipline in little known sports, that require special skills. There are also the glamour events, the track and field sports, many of which feature the world's best known athletes, sometimes pitted against unknown performers from small countries. The Olympics celebrate both individual achievement and collective team effort, in traditionally well known sports like hockey, football and basketball. Every four years, for a fortnight, the Olympics dominate our television sets, pushing cricket, Formula 1 racing and football from the viewing agenda of sports fans. Even the journals *Nature* (5 August 2004) and *Science* (30 July 2004) have been caught in the spirit of Athens 2004, devoting several pages to the Games, albeit slanted towards an analysis of the impact of science on sports. Inevitably, the discussion has turned to the use of drugs to enhance athletic performance. In an editorial in *Science*, Donald Kennedy succinctly summarized the issue: 'What we now have is a pharmacological arms race between the detection technology of the antidopers and the inventiveness of the designer steroid mavens. It is a close contest and if past is prologue, we cannot know who is ahead at any given moment. I liked track and field a lot more before they took it into the lab' (*Science*, 2004, **305**, 573). While drug use is a clear case of science abetting athletic misconduct, technology has helped enhance sporting performance in many legitimate ways. Sporting equipment has become wonderfully sophisticated; a dramatic example, as Kennedy notes, is the fibreglass pole, which allows even vaulters of moderate achievement to easily go past records set with traditional poles. Science has also contributed greatly to the understanding of human physiology, permitting naturally talented athletes to scale amazing heights after training regimes, which dramatically boost performance. But, even here the performers push themselves to dangerous limits, prompting an editorial preface in *Nature* (2004, **430**, 602) to conclude ominously: 'In the coming weeks, *Nature* will be watching to see what records – and which athletes – are broken in the process'.

Although doping scandals have inevitably hit Athens 2004, with Indian participants among the tainted, the vast majority of athletes depend only on natural talent, total commitment to their chosen sport and years of hard preparatory work. With training becoming an increasingly specialized and expensive affair, organized state support for sports becomes necessary if countries are to realize the full potential of local talent. China, which many years ago had to be content with limited Olympic success, has propelled itself into the position of a sporting superpower by harnessing talent in a spectacularly professional manner. In marked contrast, India's collective performance is dismal, despite flashes of individual brilliance. The final ranking of nations will soon be available as Athens 2004 draws to a close and lamentably India's position will compare poorly with countries which also used to perform poorly, but have improved. For readers of this column, the sports rankings coming so closely on the heels of rankings of science in nations (*Current Science*, 2004, **87**, 273) must provoke some thought on the parameters that determine success in spheres of human activity as distant as sports and science.

My thoughts turned to the early results from Athens, even as I sat in one of the interminable committee meetings that debate the direction of science funding and priorities for investment in specific research projects. The topics were diverse, the committee large and representative of a diffuse and sprawling discipline and the discussion rambling and unfocussed. I wondered whether there was any similarity to meetings at the Indian Olympic Association, which select events and athletes for participation in major international sporting events. Are chances of success estimated pragmatically? Do the selectors have a deep intuitive feeling for the nuances of a sport or do they have a first hand knowledge of the requirements for success? Are the selectors unbiased judges of performance; or are they promoters of an agenda that institutionalises poor performance? Who will judge the selectors in sport and science?

In science success measured by any yardstick (publication counts, citation statistics, products generated or peer perception) depends on choice of research problems, intrinsic intellectual skills ('talent' in simple terms), commitment to the work at hand, abilities to generate financial and institutional support and in the case of most projects the abilities to manage students, junior colleagues and to keep senior management both happy and at a distance. The equation for success is complex, with different weights for each of these parameters; the situations being vastly different for small basic research projects as compared to the large, purposeful, targeted enterprises. A theoretical physicist pondering 'grand unification' or a mathematician seeking a proof for Fermat's theorem would clearly be a breed apart, their requirements for success completely different from the overwhelming majority of their colleagues in other areas of science. In successfully overcoming technological challenges, putting satellites into space or in generating the promised amounts of nuclear energy safely, team efforts involving very large numbers of people and an enlightened management structure are critical. Unfortunately when science is discussed in a general sense, the diversity of fields and projects is inadequately appreciated; we attempt to impose common yardsticks of management and judgement on projects with vastly different goals and outcomes. In the mood of the Olympics, a parallel can be drawn with sports. Rajyavardhan Singh Rathore's silver medal came in the double trap shooting event, which requires a special combination of individual talent, very hard work, extremely steady nerves and complete concentration. Coaching (and in his case an Australian was involved over the last few months) would have been an important factor, but it may be difficult to give weights to each parameter. In the team events, and for us hockey is a prime example, managers and coaches play a crucial role in welding a medal winning team. In science too, there are areas where individual talent must be fostered by unobtrusive, supportive and modest management. There are also many projects which require a strong and aggressive management style which brings the best out of large teams of scientists. The former would be the area we often call 'basic' research while the latter would fall into the category of 'applied' research (although, I must confess that I do not like the name).

In the area of basic research, one problem which appears to be becoming more evident is the tendency of many starting researchers to continue to work in areas (and sometimes on specific problems) picked up during their postdoctoral years, which in many cases have been spent in the West. A lifelong commitment to imported research problems is not a prescription for success. There is also a distressing tendency to focus on sophisticated methodologies, with little regard for their suitability in a specific research problem. In fields like biology the distinctions between basic ('purposeless') and applied ('purposeful') research are blurred, as the growth of biotechnology seems to have bridged the chasm between discovery and application. A favourite at committee meetings is the 'Cuban Model'; an allusion to Cuba's recent successes in producing and marketing biotechnology products. Interestingly, as I write, the Cuban tally at the Olympics stands at 2 silver and 5 bronze medals, suggesting that determination and discipline may be important parameters in harnessing talent.

Biotechnology is a field in which I have been an interested (if not detached) observer. There is an enormous desire to translate the results of laboratory research into products. Diagnostics, vaccines and drugs seem tantalizingly close in the area of 'medical biotechnology', while in the area of agriculture genetically modified crops promise to resist both drought and pests. When investment decisions are made on projects which are predominantly 'basic' research exercises, the talk turns to 'deliverables'. Scientific papers are increasingly being disregarded as a desirable outcome of a project. The managers (and indeed judges) of research are often out of touch with the realities of laboratory research; although it must be admitted that good managers need not necessarily play the game. Project monitoring committees spend a great deal of time on unrealistic discussions and invariably underestimate the scientific complexity of the research issues involved. In science it may be necessary for large coordinated projects to be driven in a manner that the distinctions between managers, coaches and players are blurred. Our abilities to identify potential champions and nurture them seem limited in both sport and science. At the midpoint of the Olympics, recalling Thomas Gray's immortal lines seems appropriate:

> *'Full many a gem of purest ray serene*
> *The dark unfathom'd caves of ocean bear:*
> *Full many a flower is born to blush unseen,*
> *And waste its sweetness on the desert air'.*

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Revisiting an Old Triumph

Collagen is a very interesting protein.

L. Pauling and R. B. Corey
Proc. Natl. Acad. Sci. USA, 1951, **37**, 272

Before Alex Rich and I worked on . . . collagen, we tended to be rather patronizing about it. 'After all' we said, 'there's no collagen in plants'. In 1955, after we got interested in the molecule we found ourselves saying, 'Do you realize that one-third of all the protein in your body is collagen?

Francis Crick
What Mad Pursuit
Basic Books, Inc. New York, 1988, p. 67

As one drives down the road that stretches from the Raj Bhavan to the centre of Adyar in Chennai (formerly, Madras), an auditorium named as the Triple Helix is clearly visible, on the campus of the Central Leather Research Institute. An emblazoned sign boldly proclaims the name; an oddity in surroundings where roads, buildings and airports are named after famous people, almost invariably politicians. The *Triple Helix* auditorium commemorates a scientific event, the elucidation of the structure of collagen, one of the most abundant mammalian proteins and a major constituent of leather. The conquest of the collagen structure in 1954 by G. N. Ramachandran, then a young professor of physics at the University of Madras, and his postdoctoral assistant Gopinath Kartha was a remarkable achievement for the times. The idea of helical structures for biopolymers was new; only three years had passed since Linus Pauling and Robert Corey had unveiled the helical structures for polypeptides, in a spectacular series of papers in the May 1951 issue of the *Proceedings of the National Academy of Sciences, USA*. Barely a year earlier, the Watson–Crick paper on the double helical structure of DNA had appeared in *Nature*. The deduction of the triple helix in collagen fibers, based on meagre X-ray diffraction data, coupled with model building, required an extraordinary insight, which is the true hallmark of the most gifted. The paper on collagen that appeared in August 1954 (*Nature*, 1954, **174**, 269) announced not only a radically new structure of collagen, the triple helix, but also marked the beginning of an extraordinary burst of creative activity in biophysics (now, more fashionably known as structural biology) at Madras, in the laboratories headed by G. N. Ramachandran.

Not too long ago, I came across a little book, intended for children, which had a collection of biographical sketches of famous Indian scientists. With one exception, the dozen or so famous men featured (and there were no women on the list) had all worked largely in the pre-Independence era. Some were more known for their abilities to build institutions and to organize and promote science. Ramachandran was not mentioned in the book, presumably because his work and accomplishments have not been widely known in India, outside the confines of the scientific community. Ramachandran was the subject of a biography, that appeared three years before his death in 2001 (*Ramachandran. A Biography of the Famous Indian Biophysicist*, by Raghupathy Sarma, 1998, Adenine Press, New York). Unfortunately, the book does not seem to be widely available in India. The 50th anniversary of the collagen structure paper has provided an opportunity to celebrate Ramachandran's achievement. The Council of Scientific and Industrial Research (CSIR) and its constituent laboratory, the Institute for Genomics and Integrative Biology (IGIB) organized a function in New Delhi on August 7 to commemorate the occasion—'50 years of Collagen Triple Helix: A Celebration in Science'. The high profile event had President Abdul Kalam in attendance and drew the country's media in large numbers. Ironically, a report in *The Hindu* on August 8 devotes attention to the views of the President on a number of diverse subjects, while Ramachandran's name is conspicuously absent. Press coverage of events that relate to science is invariably of the poorest quality and lay readers are unlikely to obtain any feeling for the science and scientist being honoured at such functions. I would normally have refrained from commenting on the coverage of the Delhi celebration, if it had not been for a casual reference to the determination of the structure of collagen in the obituary of Francis Crick (A. Rich, *Nature*, 2004, **430**, 845). In this retrospective of Crick's life and times, his one-time col-

laborator Alexander Rich describes the progression of their research in 1955 to discover the coiled-coil structure of collagen. There is no mention of the intellectual origins of the triple helix and the three chain structures. There is a passing reference to competition: 'Crick had a strong competitive approach to science – other groups were working on collagen'. Elsewhere in describing their work on polyglycine Rich notes: 'Crick suggested that we might try to write this up quickly and see if we could get it published in next week's *Nature*. But then he paused because he thought that the authors (*the reference is to a paper reporting a new form of the polymer*) might feel badly, so we invited them over to look at the structure. It was characteristic of Crick that he was sensitive to people's feelings and would not intentionally cause them distress.'

An almost forgotten chapter in the history of the collagen structure is the controversy that followed the publication of the Ramachandran–Kartha paper in 1954. Coincidentally, Francis Crick proposed an incorrect two-chain model in 1954 (*J. Chem. Phys.* **22**, 347). In September 1955, Ramachandran and Kartha published a revised version of the three chain structure introducing the concept of the 'coiled coil' (*Nature*, **176**, 593). As Raghupathy Sarma notes in his biography: 'This turned out to be a seminal paper in the field . . . the idea of the coiled coil motif has remained unchallenged for the past four decades' (Sarma, p. 55). In November 1955, Rich and Crick published a paper, cleverly worded to suggest that the Ramachandran–Kartha structure was wrong: 'We believe this idea ("*coiled coil*") to be basically correct but the actual structure suggested by them to be wrong'. The Rich–Crick criticism, largely unfounded and unsupported by experimental data at that time, was to provoke Ramachandran into exploring the stereochemical limitations of polypeptide chain structures, leading to the now famous Ramachandran map for analysing conformations in proteins and polypeptides. But, in the 1950s the criticism from Cambridge would have been a major tribulation. In the four decades since, the structure of collagen proposed from Madras has been supported by a growing volume of experimental data and is accepted as essentially correct.

Ramachandran's subsequent work on the stereochemistry of polypeptide chains has entered the textbooks of biochemistry. This work emerged in a flood of remarkable publications in the decade of the 1960s, raising Ramachandran to a pinnacle of scientific achievement unmatched in post-Independence India. For those who worry about the mundane details of citations and impact, I might add that the Madras review of the work on polypeptides and proteins was listed as a citation classic in *Current Contents* in March 1981 (Ramachandran, G. N. and Sasisekharan, V., *Adv. Protein Chem.*, 1968, **23**, 283).

Yet Ramachandran remained a reclusive figure, largely unseen and unknown outside the community of scientists in India. When he died in 2001 he had not received any of the honours that are bestowed by the Government on Republic Day; awards that have been conferred with great regularity on lesser men. Foreign honours were limited; his election to Britain's Royal Society in 1977 came over a decade after his work on polypeptide conformations and two decades after the collagen success. In 1993 at the inauguration of the *Triple Helix* auditorium, the then Minister for Science and Technology, the late Rangarajan Kumaramangalam asked: 'If Ramachandran's work is so important, why did he not receive the Nobel prize?'. This is an oft-asked question and one that has no satisfactory answer. I have often wondered what C. V. Raman's place in Indian science would have been if the Nobel Committee had passed over the Raman Effect. Would it have dimmed the lustre of the discovery and limited its utility for the generations to follow? Should our assessments of those who contribute maximally to our science depend on judgements made in Stockholm, London or Washington? Is it not time to develop, foster and actively promote a degree of self-confidence that Raman, Ramachandran and many of their distinguished contemporaries would have emphatically approved?

Although Ramachandran's profound insights into protein structures are widely acknowledged, a recent analysis was dismissive. In celebrating Linus Pauling's remarkable work on protein secondary structures David Eisenberg wrote: 'Today the Ramachandran diagram is taught in all classes on protein structure and is featured in every textbook to give insight into the forces that determine the structures of proteins. But there is nothing in this diagram beyond what Pauling and Corey knew well: they built models of their proposed structures that embodied all features of the Ramachandran diagram. Apparently they understood the principle so well that they felt no need to explain them by a diagram of this sort'. (*Proc. Natl. Acad. Sci. USA*, 2003, **100**, 11207). This entirely unfounded opinion, unsupported by a re-reading of Pauling's papers and much that has followed, suggests that rewriting history is a popular pastime even in the literature of science. In retrospect, the linking of Ramachandran's name to those of Pauling and Crick, even in controversy, must be a matter of pride. Here was one amongst us, who played the game at the highest levels.

Revisiting old triumphs can be inspirational. I write this on Teachers' Day (September 5th) as a tribute to Ramachandran who taught us what is possible. In the CSIR celebration a short film on Ramachandran and his work (*The Immortal Coils*) was shown to the assembled gathering. Towards the end of the documentary, one of Ramachandran's former colleagues on the faculty of Madras University, S. Swaminathan, professor of organic chemistry, describes him in a characteristically emotional manner: 'He was Agni'. For a while, the fire burned brightly and warmed and illuminated our surroundings.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Foreign Hand

In the early 1970s Piloo Mody, then a member of the Opposition, appeared in Parliament, wearing a placard proclaiming 'I am a CIA agent'. He was lampooning the then prevailing tendency to hold the 'foreign hand' responsible for many of the country's ills. At the height of the Cold War, in a sharply polarized world, 'foreign hands' seemed to be everywhere, controlling and shaping events in the developing world. In the years since, the CIA (and many other intelligence agencies) have lost much of their sheen; their vaunted abilities to gather intelligence and influence events has been punctured by highly visible failures. Sometimes, as in Iraq and the mysterious case of the 'weapons of mass destruction', the failures may have been deliberate. But, more often, the 'foreign hand' as represented by covert agencies has been a remarkably inept instrument, at influencing the course of events, in a planned manner. It is only in the popular pulp fiction, for which I must confess an inordinate fondness, that covert agencies act effectively as a 'foreign hand'. The recent controversy on the appointment of foreign advisors and consultants to the Planning Commission suddenly raised the spectre of the 'foreign hand' once again. The singularly maladroit move to appoint 'consultants', particularly Western companies which specialize in dispensing reports and advice, was clearly bound to attract trenchant criticism. Even in this era of globalization, there seemed to be a sense of apprehension at the idea of foreign agencies influencing the process of national planning and governance. The 'foreign hand' had suddenly reappeared in a new guise.

The Planning Commission episode appears closely on the heels of the discussion on 'foreign coaches' following the Olympics. Once the doping scandals broke, fingers were suddenly pointed to weightlifting coaches in Belarus. The foreign coach concept has been readily accepted in sport; even when India loses to New Zealand in cricket, we do not point fingers at John Wright. Even the Pakistanis and Sri Lankans seem comfortable with foreign coaches. Despite an abundance of experienced players who have retired from active sport, our national teams seem to invariably require a 'foreign hand', to weld them together. Presumably, foreign coaches are perceived as neutral professionals, untouched by the prejudices that seem to be inevitable with local coaches. Planning, of course, is a much more serious business than sport; foreign coaches who tell the government how to manage the economy and infrastructure projects must undoubtedly attract adverse attention.

The 'foreign hand' is, of course, also evident in science. An historically interesting example is the case of the American ecologist Dillon Ripley, who worked with India's foremost ornithologist, Salim Ali in the 1950s and 1960s. An article, dramatically titled 'Scientists or Spies? Ecology in a Climate of Cold War Suspicion' details the manner in which the US Army funded a large scale study of bird migration to the Bharatpur sanctuary, from Russia, Central Asia and China (Lewis, M., *Economic and Political Weekly*, 15 June 2002, p. 2323). In a compelling analysis, Michael Lewis details the convoluted thinking that connected 'bird banding' studies of migration to studies of 'transmission of biological pathogens by birds for defensive purposes'; an activity which he concludes 'is only a hair's-breadth from turning that information to an offensive purpose'. Lewis' story details the manner in which PL-480 and Smithsonian Foreign Currency Programs, were used in an irresistible funding net, which drew Salim Ali and the Bombay Natural History Society into a tangled web of intrigue. This controversy was to coincide with a period where there were deep suspicions of foreign intentions, leading to a gradual freeze on foreign funding for science projects, in the early 1970s.

Three decades later, the world has changed in many ways and so have the policies of the government. Foreign grants are no longer taboo; indeed they are a badge of honour to be proudly worn as a mark of successfully negotiating the ostensibly, tough course of 'international peer review'. A close examination of many international grant applications reveals complex requirements for collaborations and evidence for experience in foreign laboratories, limiting these grants to the best of our national institutions. In areas like biomedical research, foreign foundations like the Wellcome Trust, a registered British 'charity', have for some years provided handsome grants

to young investigators. The US National Institutes of Health has also entered the area of direct funding of Indian investigators. The Bill and Melinda Gates Foundation has entered the arena of infectious disease research and the Howard Hughes Foundation may follow suit. Indian agencies cannot hope to match these bodies and it is likely that at some time in the future, many of our most promising biological scientists will depend primarily on foreign funding. Grant salaries, 'fellowships', travel funds and flexibility of operation will prove seductive; a far cry from the stifling bureaucracy that needs to be negotiated by individual investigators, when they deal with some of our national agencies.

Foreign (and private) influence has already entered many of our national laboratories, which have been goaded to earn their keep. Large multinational companies can, with limited investments, recruit teams of scientists to tasks which are of direct interest to the funding organizations. While the infrastructure (manpower included) has been built with public funds, its diversion to a private (or foreign) cause can be readily effected by the infusion of 'project funds'. The lack of direction and purpose in many national institutions makes them ripe for picking. Globalization and the spectre (most often, brandished threateningly) of the World Trade Organization (WTO) and its treaties is enough to banish 'protectionists' from the scene.

The most dramatic impact of the 21st century 'foreign hand' is in the opening of the higher education sector to foreign institutions. Most recently, the Ministry of Human Resource Development (MHRD) is reported to have withdrawn the requirement for prior clearance, before Indian institutions enter into memoranda of understanding (MOUs) with foreign institutions. This is undoubtedly a pragmatic step, which reinforces the autonomy of universities and institutions. At the same time, this delegation of power places a considerable demand on the decision-making processes in our institutions. The education sector is flooded with foreign universities eager to gain a foothold in India, which can provide an inexhaustible supply of students. The substantial fees charged for foreign degrees (including those granted collaboratively with an Indian partner) make higher education an attractive business opportunity and India an irresistible market. Most newspapers have a weekly education supplement, carrying a remarkably diverse range of advertisements. Foreign universities, many largely unknown institutions, announce impressive courses of study; local agents who recruit students are also featured. Some of the more well known universities seem to be lending their names to 'collaborative' degrees, presumably for a price. The entire education sector, deregulated and cut loose in the new economy, presents a bewildering picture. Our own institutions, barring a select few, seem curiously non-competitive.

The growing pressure on Indian institutions to enter into MOUs with foreign universities is a phenomenon that needs to be carefully examined. The most fruitful academic collaborations are those where partners contribute equally and benefit equally. This ideal is rarely achievable. There is a danger that Indian universities will become mere conduits through which foreign partners will recruit students and tap faculty for teaching. The lure of foreign travel and handsome honoraria will prove difficult to resist; institutional interests will slowly become subservient to personal gain. In most collaborative arrangements it is necessary to clearly enunciate institutional interests. For many of our scientific laboratories, programs which quickly bring in new technologies would be beneficial. Unfortunately, it is these very technologies that are often jealously guarded and often denied.

In thinking about foreign coaches and 'foreign hands', I must not forget the ubiquitous non-resident Indian (NRI). Many distinguished expatriates are keen to contribute to local development. There are also many who are keen to dispense advice, finding opportunities in the many programs initiated by the government. Some bodies, the Department of Biotechnology (DBT) is one, even have a formal mechanism for a Scientific Advisory Committee (Overseas). Here the foreign hand is decidedly of Indian origin. Is such advice useful? Are these now the foreign coaches, who will train our biologists for the competitive world of international science? Or are such bodies an anachronism, a reminder of past insecurities?

In considering foreign influences in today's context, it is clear that we cannot return to a xenophobic past. Neither can we allow paranoia about globalization to dictate decision-making. A degree of caution about the apparent benefits of 'globalization of science' to countries like India is merited. Richard Gallagher notes in an editorial: '...governments view science at best, as a cutthroat business venture and, at worst, as a political pawn' (*The Scientist*, 15 March 2004, p. 6). In both science and higher education it may be necessary to dispassionately evaluate the effects of the growing presence of foreign institutions. We must learn to manipulate the foreign hand to suit our collective purpose.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Drug Discovery: Myth and Reality

An editor penning an editorial column has a wonderful advantage; the hurdles of referees and sub-editors do not stand as obstacles before the article is published. For a regular columnist, the most difficult task is to find a topic to write about. Then comes the almost equally formidable task of gathering at least a smattering of facts. Finally, there is the threatening spectre of the deadline for the printers. Committed, as I am, to writing a fortnightly column for this journal, I am often rescued in the difficult task of finding a subject, by papers sent to me by friends, colleagues and, most importantly, readers whom I have never met. A few days ago a friend sent me a marvellously provocative editorial by Gerry Higgs in *Drug Discovery Today* (2004, **9**, 727). The polemic entitled 'Molecular Genetics: the Emperor's Clothes of Drug Discovery' was a frontal attack on the prevailing view 'that knowledge of the human genome sequence when harnessed to automated high-throughput technologies will result in drugs being made more easily'. Higgs was challenging a view expressed in the same journal by Peter Goodfellow, an academic geneticist-turned-manager of drug research at GlaxoSmithKline (*Drug Discovery Today*, 2003, **8**, 1017). Goodfellow presents an extremely optimistic view of the impact of high throughput screens (HTS) and high throughput (HT) chemistry, on the traditionally slow process of drug discovery. The drug discovery cycle is the process where a promising laboratory finding is converted into a marketable product; an uncertain process that resembles a game of snakes and ladders, potential products often failing in the final stages of clinical trials. The timescales involved are immensely long, estimates of 10–15 years being common. Indeed, the gulf between the laboratory and the clinic can be formidable. The mainstays of the conventional drug industry have been medicinal chemistry and microbiology. While the chemists have toiled ceaselessly producing molecules, structures patiently modified in incremental fashion, the microbiologists have provided both antimicrobial screens and an inexhaustible supply of culture filtrates, which provide a treasure trove of new molecules. The history of antibiotic research is replete with wonderful molecules that emerged from microbial cultures, penicillin, streptomycin, tetracycline and erythromycin among them. Once 'leads' have been identified, the task of moving forward requires careful pharmacological and toxicological evaluation, before the clinical trials can begin. But, as the drug industry has matured and as the regulatory controls have become more stringent, the process of discovering promising new molecular entities has become more difficult, more time consuming and significantly more expensive.

Over the last decade or so, the advances in genome sequencing, protein analysis and computational biology have been spectacular, giving rise to the expectation that the new technologies will hasten the process of drug discovery. 'High throughput' is a magical prefix that promises to make chemical analysis and pharmacological screening a matter for robots; raising hopes that automation will indeed aid innovation in the search for new pharmaceuticals. Combinatorial synthesis, a favourite with pharmaceutical R&D managers, coupled with automated high throughput screening has often been touted as the driving technology of future drug discovery. With genes and pharmaceutical targets appearing in profusion, it seemed only a short step to screening tens of thousands of ligands to select new 'lead' molecules. These 'leads' would then be optimized by computational scientists using 'docking' techniques, which would reveal the best fit of low molecular weight ligands to macromolecular receptors. This 'in silico' (an infelicitous phrase) approach would presumably save medicinal chemists years of toil; quickly directing their energies towards producing optimal molecules. The new 'paradigm of drug discovery' has been relentlessly propagated at innumerable conferences and in articles strewn across the sprawling literature of chemistry, biology, medicine and computer science. The biological revolution has spawned many new disciplines, genomics, proteomics, metabolomics, systems biology and bioinformatics. The confluence of these areas is expected to accelerate the drive towards new drugs. Enthusiasts for the new technologies have looked into the future and announced that 'pharmacogenomics', a term that promises customized drugs for individuals, is just around the corner. The rhetoric of drug discovery research has been raised to fever pitch. It is in this context that Higgs' assault on molecular gene-

tics and its role in drug discovery makes interesting reading. Indeed, over the last couple of years even the US Food and Drug Administration (US FDA) notes a fall in the number of new molecules entering the regulatory pipeline. This decline appears at odds with the optimistic projections of the proponents of the new technologies. With the timescales for discovery lengthening and the costs of innovation rising (one estimate that I came across was as high as $ 800 million as the cost for introducing a new drug), the game of mergers and acquisitions has become an integral part of the pharmaceutical industry.

In his essay, Higgs questions the assumption 'that studying the genome at a molecular level will reveal new targets for drug intervention and that molecular biology will provide the relevant tools for indentifying new drugs. The surprisingly poor success rate of this approach suggests that these assumptions should be questioned' (*Drug Discovery Today*, 2004, **9**, 727). I particularly liked his emphatic dismissal of 'the assumption that the best new medicines will be the most potent and selective against a particular target'. Higgs argues that this view is based on a 'misunderstanding of how drugs work'. He supports his thesis by pointing out that 'some of the most successful medicines are remarkably weak or non-selective. For example, aspirin, ibuprofen and cimetidine are blockbuster drugs with potencies in the micromolar range'. The reference here is to the hunt for 'nanomolar' receptor ligands, molecules that seek their targets with remarkably high affinities. Higgs drives his point home by pointing out that 'drugs such as fluticasone and budenoside, which are widely used to treat asthma, belong to a class of anti-inflammatory steroids, that are so non-selective that nobody is quite sure how they work. In the high throughput screen regimes of today, these drugs would not even be rated as "hits" '. Higgs goes on to question the validity of genetically engineered cells as disease models and argues that gene 'knock outs' are 'highly vulnerable to the creation of misleading artefacts'. In stating 'that the reductionist structure-based approach of molecular biology is a poor starting place for drug discovery', Higgs clearly argues for a greater emphasis on approaches that move forward from clinical investigations. To bolster his case, Higgs cites the case of infliximab, an anti-inflammatory cytokine used for treatment of rheumatoid arthritis, developed over the period 1989–1998. Here the initial leads in seminal work by Ravinder Maini and Marc Feldman at the Kennedy Institute, Imperial College, London came from work with inflamed tissues from patients. Maini and Feldman received the 2003 Lasker Award for this achievement.

The history of drug research over a period of a century, since Paul Ehrlich introduced the concept of chemotherapeutic agents, is a marvellous record of accomplishment. The great victories of the antibiotic era were often serendipitous; penicillin, streptomycin and tetracyclines followed in quick succession. The successes of the 1950s and 1960s were spearheaded by the work of Gertrude Elion and George Hitchings at Burroughs Welcome and James Black at King's College, London, who between them were responsible for developing a large number of drugs, many of which are widely used today. To Elion and Hitchings goes the credit for introducing azathioprine, the first immunosuppressive agent, allopurinol for gout, pyrimethamine for malaria and trimethoprim for bacterial infections. Later, Elion went on to introduce the antiviral, acyclovir using the principle of exploiting differences in nucleic acid metabolism between target and host; an approach that would later be followed in developing the AIDS drug, azidothymidine (AZT). Black used an incisive approach, relying on a deep understanding of physiology, to discover propranolol, the 'beta-blocker' used for heart disease and cimetidine used for treating gastric ulcers. Black, Hitchings and Elion received the 1988 Nobel Prize for Physiology or Medicine; one of the rare instances where drug discovery has been honoured in recent times.

In reading Goodfellow's optimistic view of the future of drug discovery and Higgs' characterization of the new technologies as a case of the 'Emperor's new clothes', I was struck by the evident tensions between the classical disciplines of pharmacology, physiology and medicinal chemistry on one hand and the new offshoots of molecular and cellular biology on the other. Higgs notes that the 'large pharmaceutical companies are increasingly dependent on old products that have a rapidly expiring patent life. Alarmingly, the direction of discovery research is often governed by scientists who are unfamiliar with the origin of these drugs. The industry needs to rediscover the discipline of deductive pharmacology. Above all, it needs to promote clinical pharmacology'. Ironically, although 'drug discovery' is a widely trumpeted activity, pharmacology is a low-profile discipline with none of the glamour associated with modern biology or medicine. Curiously, in his Nobel lecture, James Black advanced a view on a discipline that he termed as analytical pharmacology: 'What we are allowed to see of a new molecule's properties is totally dependent on the techniques of bioassay that we use. The prismatic qualities of an assay distort our own views in obscure ways and degrees. Our only defence lies in restless improvement in technique and experimental design in the hope that collimation of several techniques will improve the reliability of our vision. We would make changes self-consciously today, but then it was intuition.'

Maybe some of the classical disciplines that have contributed to drug discovery in the past, may well do so again in the future. In drug discovery research today, it is becoming increasingly hard to distinguish between myth and reality.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Deepening Tragedy of the Commons

'Where then, ah! where, shall poverty reside,
To 'scape the pressure of contiguous pride?
If to some common's fenceless limits strayed,
He drives his flock to pick the scanty blade,
Those fenceless fields the sons of wealth divide,
And even the bare-worn common is denied'.

— Oliver Goldsmith
The Deserted Village

'Freedom in an unmanaged commons leads inevitably to ruin. In a crowded world our only real freedom lies in joining with others in choosing and implementing the forms of coercion – mutual coercion, mutually agreed upon – which describes the result of any law in a democracy. Voluntarism will not save the whales or keep the skies unpolluted. Neither will it make possible population control by purely personal birth control in a welfare state: in this case, the compassionate rule of welfare, 'to each according to his need', creates a commons, with the usual prognosis of ultimate ruin'.

— G. Hardin
'This Week's Citation Classic' Current Contents
14 May 1979, p. 316

'Commons' is an uncommon word. It is found in the name of the lower (and undoubtedly, more important) house of the British Parliament – the House of Commons. My first encounter with the word was when I came across Oliver Goldsmith's 1770 poem, *The Deserted Village*, in a high school English class. The poem is a riveting, but melancholy, ode to a rural Utopia ('Sweet Auburn! loveliest village of the plain'), which appeared to be rapidly disappearing in mid-18th century England. Several years later I came across Garret Hardin's famous essay, *The Tragedy of the Commons* (*Science*, 1968, **162**, 1243). Hardin (1915–2003), an ecologist raised questions about the sharing of common resources, land, water and the atmosphere among them, in the context of the increasing pressures of growing populations. He began his analysis by stating the thesis that there are classes of human problems that defy technical solutions. Hardin, borrowed the concept of a 'problem with no technical solution' from an analysis of the future of nuclear war by J. B. Wiesner and H. F. York (*Sci. Am.*, 1964, **211**, 27), which appeared at the height of the Cold War. For Hardin, an ecologist concerned with the effects of rapidly growing human populations, the 'population problem' belongs to the class of 'no technical solution problems'. He stated his thesis clearly:

ation, as Malthus said, naturally tends to grow 'geometrically', or, as we would now say, exponentially. In a finite world this means that the per capita share of the world's goods must steadily decrease. Is ours a finite world?' Hardin wrote these words when space exploration promised much; science fiction, where distant planets were colonized, was extremely popular in America. Even within terrestrial limits, the problems of the growing energy requirements of burgeoning populations appeared amenable to technical solution. The promise of atomic energy in the 1960s seemed limitless. But, Hardin pursued his argument relentlessly. 'However, given an infinite source of energy, population growth still poses an inescapable problem. The problem of the acquisition of energy is replaced by the problem of its dissipation.' He clearly foresaw the problems of environmental degradation and pollution, which are worldwide issues today.

In his analysis Hardin raised a key question: '...Can Bentham's goal of "the greatest good for the greatest number" be realized?' The reference here is to Jeremy Bentham, English philosopher and author of *An Introduction to the Principles of Morals and Legislation* (1789). Bentham's views are likely to leave modern readers bemused: 'The purpose of government is to foster happiness of the individual and that the greatest happiness of the most people should be the goal of human existence'. Hardin quickly dismissed Bentham's goal. His theoretical argument was based on an axiom which is a critical element in the von Neumann–Morgenstern elaboration of 'game theory' – 'it is not mathematically possible to maximize for two (or more) variables at the same time'. A more 'biological' argument based on energy requirements led Hardin to the inexorable conclusion 'that maximizing population does not maximize goods'. Worrying about the effects of unchecked growth of human populations, led Hardin to consider the problem of sharing common resources. Hardin outlined the logic that leads to the 'tragedy of the commons', by arguing that in a common pasture it

is in the interest of an individual herdsman to increase the size of his flock. This is, of course, because the benefits of a larger herd accrue to the individual. The disadvantages of overgrazing are shared by all, as the common pasture degrades. For an individual, the choice is clear. Following this argument leads to Hardin's conclusion: 'Each man is locked into a system that compels him to increase his herd without limit – in a world that is limited. Ruin is the destination towards which all men rush, each pursuing his own best interest in a society that believes in the freedom of the commons. Freedom in a commons brings ruin to us all'.

In reaching this judgement Hardin felt compelled to 'exorcize the spirit of Adam Smith' whose 1776 book *The Wealth of Nations* had popularized the idea of an 'in- ... the public interest'. While Smith and his followers did not advance the 'invisible hand' as an ever-present guiding force of public policy, there has always been a tendency to imagine 'that decisions reached individually will, in fact, be the best decisions for an entire society'. My quick search of the Internet using *Google*, quickly revealed that Adam Smith is most often misquoted; deliberate misquotation appears a powerful weapon of scholarship in both natural sciences and the social sciences. Hardin, even in the late 1960s appeared concerned with the 'policy of laissez-faire' in human reproduction. He argued that the existing population policies were correct only if 'we assume that men will control their individual fecundity so as to produce the optimum population'.

The growth of populations and dwindling natural resources, water and oil preeminent among them, and the unchecked pollution of the air, earth, rivers and even oceans is a pressing problem today. The glaring inequalities in resource sharing between nations and even between populations within a country have made the world an unstable and dangerous place. Life in an unequal world is not going to get easier. Every element of Hardin's 'tragedy' is evident in our immediate surroundings. Unchecked growth of population, uncontrolled use of common resources in activities which are both acquisitive and dissipative (following Hardin) and the absence of 'social arrangements' (rules and laws that are generally followed) is the norm in many parts of the world; India is no exception. For Hardin the only way out of the impending tragedy appeared to be the introduction of 'coercive measures'. He noted (and we must remember he was writing at a time when American university campuses were in turmoil over Vietnam): 'Coercion is a dirty word to most liberals now, but it need not forever be so.... To many, the word coercion implies arbitrary decisions of distant and irresponsible bureaucrats; but this is not a necessary part of its meaning. The only kind of coercion I recommend is mutual coercion, mutually agreed upon by the majority of people affected'. Hardin's concern with unchecked population growth led him to question the Universal Declaration of Human Rights which 'describes the family as the natural and fundamental unit of society' and goes on to state 'that any choice and decision with regard to the size of the family must irrevocably rest with the family itself and cannot be made by anyone else'. He admitted that 'it is painful to have to deny categorically the validity of this right' but he was bold in challenging a UN assertion at a time when it was taboo to do so in liberal quarters, to which he felt he belonged. Thirty years after his original essay Hardin observed: 'Individualism is cherished because it produces freedom, but the gift is conditional. The more the population exceeds the carrying capacity of the environment, the more freedoms must be given up... Yet to come are many other restrictions as the world's population grows' (*Science*, 1998, **280**, 682–683).

Hardin used the word 'tragedy' in the sense that Alfred North Whitehead did: 'The essence of dramatic tragedy is not unhappiness. It resides in the remorseless working of things'. He did not claim to be the first to worry about the degrading commons, but quotes Whitehead again: 'We give credit for an idea not to the first man to have it, but to the first man to take it seriously' (*Current Contents*, 14 May 1979, p. 316). Hardin's essay is now cited in many fields, ranging from discussions of open access publishing to privatizing universities (cf. Brown, J. R., *Science*, 2000, **290**, 1701). A special section of *Science* (12 December 2003) entitled 'Tragedy of the Commons' had the editors suggest that contrary to Hardin's assertion 'science now has a central and urgent part to play' (*Science*, 2003, **302**, 1906). In addressing the problems that Hardin posed, both technical and political solutions appear distant and at times, unreachable. The issues he raised are relevant in India today (and everywhere else). The National Population Policy is ambiguous and state laws that impose a 'coercive' two-child norm are being challenged. Alternative solutions, education and women's empowerment, are being advanced. 'Voluntary and informed choices' are seen as the most reasonable approaches. Development appears to be the most promising contraceptive. India's degrading environment is also being hastened by unrestrained consumption; voluntary denial is not an attribute of modern life. The tensions between free markets and centralized management of the 'commons' are evident.

The issues that Hardin raised appear to have no technical or equitable political solutions. The 'tragedy of the commons', that he so evocatively described, continues to deepen. As we rush towards lopsided development, driven inexorably and tragically by free markets, a return to Goldsmith may be appropriate:

'Ye friends to truth, ye statesmen who survey,
The rich man's joy increase, the poor's decay,
'Tis yours to judge, how wide the limits stand
Between a splendid and a happy land'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Gerontophobia, Ageing and Retirement

Gerontophobia is defined as the fear of growing old or a hatred of old people. The definition is curiously double-edged. It encompasses both the fears and self-doubt of individuals as they age; even more importantly it describes a sense of hostility that sometimes appears directed at an older generation, by a new and upcoming order. A recent letter in this journal (K. Subba Rao, *Current Science*, 2004, **87**, 1029) draws attention to growing gerontophobia in our scientific institutions. In the correspondent's perception the attitude of younger colleagues towards recently retired senior colleagues seemed to be characterized by 'visible apathy'; although as I read the letter it seemed that 'undisguised hostility' may have been the phrase the author might have liked to use. But the letter raises many general questions on retirement from science, ageing and the ever-present tensions between generations. Retirement is a difficult proposition in most professions; in academia, professors and researchers find it almost inconceivable that they have reached a stage, where they may no longer be able to continue the work that has occupied them for decades. Unlike in many other occupations, in science practitioners follow their own instincts in research; most often becoming attached to trails of investigation, which others may not always find exciting. In some sense, scientists in academia are their own masters, much like artists and authors, who do as they please. There is, of course, an important difference; artists and authors earn a living determined by their talent, while academics lead a secure existence working under the umbrella of publicly (or privately, in some cases) funded institutions. In addition, the practice of scientific research is expensive, requiring substantial inputs, which invariably come from public resources. Academics are therefore employees of organizations, which are constrained to impose an age of retirement, which ensures the process of institutional renewal. But, it is hard to tear professors away from their surroundings. In institutions across the world, we have 'emeritus' academics who continue to come to their places of work, long after their names have been deleted from the payroll. Old professors, to borrow a phrase from an army ballad quoted famously by General Douglas MacArthur on his retirement, just 'fade away'; at times, very slowly.

What should be the age of formal retirement? What facilities must be extended to productive and scholarly academics, who may enrich their surroundings and their disciplines with their work, even as they advance in years? What responsibilities must rest on those who seek to continue to maintain offices and laboratories in the institutions where they have spent their careers? These are difficult questions which confront administrators of academic institutions and heads of departments, who need to make judgements on senior colleagues. The mandatory retirement age for scientists and academics in India is presently 60 or 62 years; most centrally funded institutions follow the higher age limit. In Europe, the age of retirement varies from 65 to 67 years, while in the United States there is no formal requirement that compels academics to retire. The age of retirement was not a major issue when the average life expectancy was limited. Now, with the relentless advance of modern medicine, average life expectancies have risen dramatically. A 60 or 70-year old today is far fitter and more active than was the case half a century ago. Human populations are ageing worldwide, spectacularly so in the developed world. A visit to the website of the United States Census Bureau provides interesting pictorial projections of the age and sex distributions of populations in countries across the world, comparing data in 2000, with that expected in 2025 and 2050. The 'population pyramids', constructed with age represented along the vertical axis and population along the horizontal axis, are remarkably revealing. 'Pyramids' are visible in 2000 in most countries, but the change in shape seen in some developed countries, is predicted to be a worldwide phenomenon in coming years. Countries like Japan are ageing dramatically, as are the countries of Western Europe. The rate of ageing is very large in China too; and I suspect this would be the case in India, although the effects may be quite different depending on the nature of the population group. Much of the discussion on 'retirement' in the West centres on the wasteful practice of 'putting out to pasture', healthy, alert and competent individuals. Over the past few years many prominent European scientists, compelled to retire in their own countries, have moved to the United States, often setting up new and productive schools of research. The most vigorous researchers (often with Nobel prizes under their belts) have been enticed to the US, with the promise of laboratories and facilities for continued research. The 'brain drain' from Europe thus seems to have two compo-

nents; the young scientists drawn away at the start and the stars pulled away at the pinnacle of their careers.

The world of sport presents some of the most compelling moments of drama, where a remarkably successful individual 'voluntarily' retires, when the high standards of their own past performances, highlight the inevitable process of decline. The public demands on successful sportsmen are immense and we have often witnessed the agony of timing a retirement. But, in most professions, the standards of judgement are far more modest and expectations are almost non-existent. Retirement is not a consequence of the automatic decline of performance; rather it is an administrative deadline which is intended to ensure optimum (or in most cases, minimum) performance of organizations. Universities and other academic institutions in India must necessarily impose retirement ages; this is the only way that a new generation of academics, teachers and researchers, can be accommodated. Many of our institutions have also provided shelter for a large number of non-performing individuals, who sometimes even acquire a degree of administrative power. Retirement is the only escape route for the institutions. But the question raised by the correspondent in the columns of this journal really addresses a different issue: 'Can we create an environment in our institutions where the most productive and scholarly members of our profession are allowed to continue to pursue their interests in a congenial atmosphere?' The plea here is not to do away with retirement deadlines, but to ask if selective encouragement of scholarship is possible.

The causes for 'gerontophobia' in our institutions are not hard to see. Many retiring professors acquire the trappings of administrative power in the late stages of their careers. Often, their relationships with their younger colleagues are tense; competition for resources always favours those who wield authority. Academic judgements, and these are the most difficult, are the responsibility of those who are senior. In our surroundings, the credibility of these judgements is sometimes questionable. Not surprisingly, many senior academics end their careers in an environment where 'gerontophobia' is widespread. Retaining academics after retirement requires investments of space and funds for research, which most institutions cannot easily provide. But, sadly in many of our universities, large buildings are very sparsely populated, since hiring of new faculty has been extremely slow. One would imagine that encouraging continued scholarly activity by retiring faculty might contribute in some measure to keeping alive an academic tradition. The limited mobility of scientists, both old and young, has also contributed to the problems I have discussed. Retired scientists are invariably treated with a greater degree of congeniality when they move to another institution. The burdens of history are automatically erased. The temptation to be intrusive also disappears.

A possible approach to encouraging movement would be to make 'emeritus scientist' schemes especially attractive in terms of research grants, when a change of institution is made. The real difficulty we would face is to make academic judgements. It may be easier and less hazardous to identify promising 'young scientists' than to select productive 'old scientists'. In India, scientific eminence, research ability and even more importantly, academic scholarship, are often seen to be attributes possessed by those who occupy administrative positions. Retiring administrators are thus, most often, the beneficiaries of schemes devised to encourage continued academic activity by productive scholars.

When I began writing this piece my intention was to quickly digress to the subject of ageing. Having used my quota of words on the more mundane subject of keeping retired professors active, it is appropriate to return to a more general theme. Ageing research has acquired a new importance and a great sense of urgency. A visit to the website of the journal *Science*, leads to a section titled 'Science of Ageing Knowledge Environment (www.sageke.org). Ageing is one of the few certainties in life. For those who are victims of gerontophobia, there may be some consolation in the fact that a similar fate inevitably awaits their tormentors. No subject has attracted the romantic poets as much as ageing, with the possible exception of love. Two old favourites are worth recalling:

'The glory and the freshness of a dream
It is not now as it hath been of yore; -
Turn, wheresoe'er, I may,
By night or day,
The things which I have seen I now can see no more.'

— W. Wordsworth
'Ode on Intimations of Immortality'

'We are not now that strength, which in old days
Moves earth and heaven; that which we are, we are
One equal-temper of heroic hearts,
Made weak by time and fate, but strong in will
To strive, to seek, to find, and not to yield.'

— A. Tennyson, '*Ulysses*'

Gerontophobia, in both senses of the word, must be banished.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Stages of Scientists

'All the world's a stage,
And all the men and women merely players:
They have their exits and their entrances;
And one man in his time plays many parts,
His acts being seven ages'.

— W. Shakespeare
As You Like It

Shakespeare in his inimitable style traced the 'seven ages of man'. From the 'infant, mewling and puking in the nurse's arms' to the 'second childishness... sans teeth, sans eyes, sans taste, sans everything', the poet described every stage of life. In casually reading this verse, I was drawn to wonder about the stages in a scientist's career. How many stages are clearly identifiable? How many variations can there be, based on a common general theme? Undoubtedly, chance plays a determining role in every scientist's career, just as it does in every aspect of life. In every distinct stage, there are choices to be made, decisions to be taken and circumstances to be confronted. The course of a scientist's career (and indeed any career) is hardly predictable. Although students study science in school, the average scientist begins to develop an interest in a discipline only at college or in a university course. In India, meaningful exposure to specific disciplines of science is first obtained in B Sc or M Sc courses. Despite an all round decline in the quality of many of our older institutions, there are several opportunities available today for students to gain an exposure to the world of research. Projects are fashionable, summer training programs are on the increase and many colleges conduct workshops and seminars, a practice unheard of some years ago.

The first formal stage of a scientist's career might well be the period spent as a research student, working for a Ph D degree at an academic institution. In our surroundings the institutions can vary widely in their ambience, ranging from traditional universities and research institutions to national laboratories, whose primary mandate is usually focussed, directed research. The Ph D is a passport to a research career; an almost mandatory degree for an aspiring scientist. The Ph D period can also be a 'make or break' stage in a scientific career. The choice of a 'guide' or 'advisor' is a critical step. Most often, students have few choices and little or no control; chance invariably plays a major role. Guides come in various forms; they are, of course, at a defined stage in their own careers. There are the supervisors, who are intense about their science and teach by example. There are guides who have no interest in their discipline and leave their wards to their own devices. Surprisingly, it is these 'guides' who produce some of the most resilient scientists; self-taught men and women, who develop great confidence in their own abilities. There are guides who interfere and discourage their students. And, of course, in today's competitive world there are ambitious researchers, who use Ph D students as a means of self-advancement, relentlessly pursuing predetermined results at all costs. While the budding scientist's career is shaped at the Ph D stage by interactions with a 'guide', departmental and institutional ambience can be important determinants. In the United States, graduate schools are extremely well organized, with a Ph D student's academic development being monitored by mechanisms, involving courses, seminars and cumulative examinations. Very few institutions in India require a basal level of academic performance by a Ph D student. Most are content to leave all training and judgements to individual supervisors, leading inevitably to widely varying academic standards.

The second stage of a scientific career is probably the most uncertain phase. This is the period of postdoctoral research. Although the number of 'postdocs' in Indian institutions is small, the number is growing. The support for postdoctoral fellowships is increasing; a step in the right direction. The vast majority of Indian Ph Ds in science, of course, travel abroad to foreign laboratories for a post-doctoral period; the United States being a favoured destination. The first fellowship is obtained by correspondence, with both mentor and student having little direct contact. At this stage, chance can play a decisive role. A fellowship in a vigorous and agreeable laboratory can be a major career boost. A stint in a difficult or unproductive group can signal a downslide in a career path. Postdocs lack the institutional support mechanisms that are sometimes available to registered students. They are invariably completely dependent on the professors, who pay their salaries. In advising postdocs, Gottfried Schatz, a prominent Swiss biochemist, writes: 'Of course you should

look for scientific excellence, but do not forget the human angle. Some famous laboratories are snake pits, which can suck the joy out of science and may warp you for life' (*FEBS Lett.*, 2004, **568**, 1–3). The postdoctoral stage can often be the most enjoyable period of a life in science. Unburdened by the pressures of a Ph D thesis, untouched by the responsibilities of a new faculty member, postdocs are free to devote all their time to research, learning techniques at an age, where assimilation is still relatively painless. Even at this stage, mentors play a major role, nudging a scientific career towards a critical point. Choices have to be made at the end of this period; industry or academia, research or teaching or a combination of both, and of course, the future field of work. It is not easy for scientists to change fields at later stages of their careers. The postdoctoral years often represent a last chance to switch disciplines.

A permanent, independent position is the third stage. In India there are generally none of the tensions associated with the process of acquiring 'tenure'. Faculty members once hired are almost never fired. 'Permanence' is prized in a country, where both opportunities and mobility are limited. Unfortunately, many Indian scientists enter a cycle of postdoctoral fellowships in the West, postponing the process of hunting for jobs in India. Many quickly pass the age where entry level faculty appointments are generally made. Jobs in science are also not always easy to get; many of our universities find it hard to process appointments, with 'hiring freezes' of one kind or other being operative, most of the time. An independent research position in academia can also be daunting. Unlike in industry, scientific problems have to be self-generated. Suddenly the realization dawns that choosing the right problem to pursue can be difficult. Peter Medawar's most readable collection of essays, *Advice to a Young Scientist* (Harper and Row, New York, 1979) contains many nuggets, but I particularly like one piece of advice: 'After graduate students have taken their Ph Ds, they must on *no account* continue their Ph D work for the remainder of their lives, easy and tempting though it is to tie up loose ends and wander down attractive byways'. This advice might be extended to the work done during postdoctoral periods. It is sometimes inadvisable and unproductive to remain tied to problems, that have their intellectual origins in a mentor's laboratory. Imported problems sometimes do not flourish in a new environment. In many of our institutions there is a pronounced tendency to inbreed; new faulty members are often old students. In some institutions this practice has led to overwhelming dominance by a few senior professors, who successfully pack departments with their proteges. New entrants, students, postdocs and faculty are the lifeblood of an academic department. It is important to ensure that this process of infusing new talent into the scientific pool is maintained.

In the fourth age of scientists, a degree of stability has been achieved. Laboratories are running, students are at work, publications accumulate, invitations to speak appear, but many insecurities remain. For the successful, a craving for recognition replaces the craving for permanence. In Medawar's words: 'Scientists like sportsmen and writers are in the running for a whole variety of prizes and other rewards' (p. 78). For those who quickly move to the sidelines there are the insecurities associated with advancement in their jobs. Fortunately (?) in many of our institutions research performance is not a prerequisite to be promoted through the academic ranks. The length of tenure in a given position is often a necessary and sufficient condition to reach the level of a professorship.

A fifth stage is identifiable when a scientist reaches comfortable middle age. Committees and administrative duties beckon; the laboratory becomes an unfamiliar place; new techniques appear formidably complex and best left to students. This is the stage where a transition occurs from a thinking scientist to a manager of science. Committees are a wonderful place to learn how to tell others what they should be doing. It is at this stage that those who have acquired administrative power suddenly realize that major scientific problems can be solved, only if big groups can be organized and directed. Unfortunately, good teams function well only when their captains are stellar performers and also inspirational. In India, there has been an unfortunate tendency to correlate administrative position with academic ability; sadly, many 'directed projects' flounder because of a lack of appreciation for the science involved. Of course by this stage, the majority of scientists are biding their time, waiting for the exit cue. Milton's famous line – 'They also serve who only stand and wait' – might be an appropriate descriptor.

The sixth stage is the post-retirement phase. There are regrets for problems left unfinished, opportunities missed, a sense of loss that students are no longer available to act as an elixir and the inevitable feeling of a participant transformed into a distant spectator. The seventh and final stage is not unique to scientists, it is Shakespeare's

'Last scene of all,
That ends this strange eventful history'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Law, Science and Climate Change

Ordinary people usually have a deep-seated fear of the law and lawyers. Laws are usually broken with impunity by the rich and powerful, lawmakers and law enforcers pre-eminent among them. Good lawyers, presumably, ensure that their clients are protected from the consequences of their transgressions. Lawyers in the pages of crime fiction are usually glamorous stars, who often double as detectives. My first youthful acquaintance with lawyers was through the entertaining character of Perry Mason, in Erle Stanley Gardner's prolific output of books, most of which seemed to have the same plot and the same cast of characters. More recently, lawyers in fiction (American, of course) have come to life through John Grisham; curiously, here again it is difficult to tell the difference between one book and another. The most unlikely and unforgettable lawyer in fiction is the admirable Atticus Finch, who provides this wonderful piece of advice to his ten-year-old son: 'I'd rather you shot at tin cans in the backyard, but I know you'll go after the birds. Shoot all the bluejays you want, if you can hit 'em, but remember it's a sin to kill a mockingbird' (Harper Lee, *To Kill a Mockingbird*, Heinemann, 1960; Arrow Books, 1997, p. 99). Many public figures are lawyers; a background in law often providing the right platform for an entry into politics. Lawyers and courts of law have been very prominently featured in the newspapers and on TV in recent weeks; the Best Bakery case has taught us about 'hostile witnesses', the Shankaracharya case has made otherwise quiet people suddenly debate the merits of bail applications, the Birla feud has lawyers arguing the merits of wills, while the Ambani saga will undoubtedly provide much fodder for future legal battles. In this environment, I was fortunate to be a mute spectator to a discussion between a professional lawyer and some very intelligent students of law. The students seemed admirably self-assured, clearly able to argue on both sides of any contentious issue; there was little of the tentativeness that is a characteristic of many aspiring scientists. Intriguingly, most of the students seemed to be interested in international law; criminal law, which is so glamorously portrayed in popular fiction, TV serials and cinema seemed to be a field that had few takers. Listening to their exchanges, it seemed that some aspects of international law may have a direct relevance to subjects that concern scientists.

Lawyers seem to be everywhere nowadays, helping to draft international agreements, treaties and conventions, many of which deal with issues, whose origins are rooted in science. The General Agreement on Trade and Tariffs (GATT) and the World Trade Organization (WTO) immediately conjure up visions of crumbling trade barriers, the decline of protectionism and patent regimes, which will eventually have an impact on our pharmaceutical industry, presumably driving up health care costs. The shift from process patents to product patents will greatly influence (and indeed it already has) the attitude that Indian companies have towards basic research. The hunt for new molecules is undoubtedly fuelled by changes in patent laws. Scientists must also become more patent-friendly, a transformation that requires at least a nodding acquaintance with patent attorneys. In turn, patent lawyers must benefit from at least an intuitive feeling for the ways of science and scientists, if communication is to be effective. The increasing use of scientific tools in forensic investigations, DNA fingerprinting foremost among them, has also brought a section of scientists into greater contact with the legal system. At a global level, attempts to draft new international conventions (laws, maybe an inappropriate word) have brought lawyers, politicians and scientists to common platforms; the Biodiversity Convention and the Kyoto Protocol are two hotly discussed examples.

The Kyoto Protocol is a wonderful name, reminding those who like thrillers of plots, where spies chase one another through exotic locations, hunting for copies of secret treaties, which will determine the world's future. It is, of course, a mundane document, which attempts to hammer out an international agreement on restricting the emission of gases, which give rise to the 'greenhouse effect' and cause 'climate change'. The Kyoto Protocol and the problems of global warming have become widely debated issues. Science meets up with science fiction in the film *The Day After Tomorrow* (I have only read about it), which portrays the disasters in store as unpredictable and extreme climatic changes occur as a consequence of rapid global warming.

Interestingly, the scenario envisages a descent into an ice age as the polar ice caps melt, a possibility that is talked about even in the general scientific literature.

Global warming occurs as the concentration of atmospheric carbon dioxide (CO_2) increases, trapping the sun's heat. Before global industrialization, the level of CO_2 was about 280 parts per million (ppm). In 1997, the year that the Kyoto Protocol appeared, the level was 368 ppm. In 2004 the figure is 379 ppm. Acceptable levels, once international laws come into force, may range between 450 and 550 ppm (Pearce, F., *New Sci.*, 9 October 2004, p. 6). Methane is another greenhouse gas, which appears to contribute less to 'climate forcing' at present emission levels, as compared to CO_2. Ironically, the greenhouse effect produced by methane given off by early microorganisms, methanogens, might have been among the first steps that led to the rapid evolution of life on Earth, over 2 billion years ago (Kasting, J. E., *Sci. Am.*, 2004, **291**, 52). CO_2 emission is primarily linked to human activity; rapid industrialization, higher living standards, growing populations and conspicuous consumption are among the contributory factors. Special problems, like the burning peat bogs of Borneo, compound the CO_2 emission problem (Aldhous, P., *Nature*, 2004, **432**, 144). Much of the literature on climate change focuses on the issue of strategies to mitigate CO_2 and other gas emissions. Curtailing human developmental activity is unacceptable. The contributions of different countries to the overall emission load are vastly different. For example, the United States 'emits eight times as much CO_2 per head of population as China and 18 times as much as India' (*New Sci.*, 9 October 2004, p. 7). Clearly, the world is an inequitable place. In this scenario, hammering out international agreements will not be easy. The Kyoto Protocol appears to be a first preliminary step and its recent ratification by Russia seems to have breathed some life into an agreement, which is already under severe attack. Bjorn Lomborg, the controversial author of *The Skeptical Environmentalist*, declares categorically: 'The urgent problem of the poor majority of this world is not climate change' (*New Sci.*, 30 October 2004, p. 23). Few can argue against this position. At the other end of the spectrum, David King, the UK government's chief scientific adviser states emphatically: 'In my view, climate change is the most severe problem that we are facing today—more serious even than the threat of terrorism' (*Science*, 2004, **303**, 176). King argues that 'poor people in developing countries are likely to be the most vulnerable', citing studies that forecast extensive 'flooding in the river delta areas' and 'coastline cities and villages' of many countries, including India by 2080.

There is a heady mix of politics, economics, science and law in discussions of climate change. 'Carbon trading' is a phrase that the uninitiated would associate with organic chemistry; instead it is a global business, that is slowly emerging from the negotiating tables of intergovernmental meetings on climate change. Like most ideas for business, this one too seems to have its origins in a scheme generated in the United States 'for trading sulphur dioxide emissions, which is widely regarded as a triumph of market forces enforcing environmental rules' (Hopkin, M., *Nature*, 2004, **433**, 268). In the post-Kyoto schemes that appear to be taking shape 'carbon credits' can be traded. The underdeveloped world would be given 'incentives' to develop slowly, while the richer countries would pay and pollute, with complex formulae controlling this 'new market'. The available data suggests that per capita CO_2 emissions are highest in the most developed countries. Similar development in the rest of the world is clearly not a sustainable goal. Even within countries, the contributions to global pollution vary. Even as the rhetoric on climate change heats up, it is clear that the poor will eventually pay for the excesses of the rich. Coal as a fuel will become expensive in terms of 'carbon credits' (or should it really be debits?), while countries with military and political muscle will tightly control global oil supplies. It will eventually be easy to compel poorer countries to accede to almost any treaty, subtly forced by political and economic pressures.

Interestingly, two of the holdouts against the Kyoto Protocol are the United States and Australia. David King notes that the US accounts for more than 20% of the world's emissions 'although it contains only 4% of the world's population'. A recent article by the US Secretary of Energy entitled 'The Bush Administration's Approach to Climate Change' makes no mention of the Kyoto Protocol, but talks about carbon capture and sequestration as a central element in a climate change science program (Abraham, S., *Science*, 2004, **305**, 616). Even if new technologies for carbon capture emerge, it is clear that efforts to control global CO_2 emissions will remain a high priority on the international scene.

The world is still a long way from being governed, effectively and benignly, by enlightened international laws. The conventions and protocols that diplomats, lawyers and science policy makers labour over today may be the forerunners of the laws of the future. In thinking about law I was irresistibly drawn to an old favourite. Mr Bumble is admonished, 'the law supposes that your wife acts under your direction'. He then responds, famously and ungrammatically: 'If the law supposes that, the law is a ass—a idiot' (*Oliver Twist*, Charles Dickens). But, the last word on law must belong to Lewis Carroll: 'No, no!' said the Queen. 'Sentence first—verdict afterwards' (*Alice's Adventures in Wonderland*).

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science and Music: Truth and Beauty

'The simple is the seal of the true
Beauty is the splendour of truth.'

— Subrahmanyan Chandrasekhar
Nobel Lecture, 1983

'The story of Meera is the story of India, the story of Indian faith and devotion and ecstasy. Subbulakshmi's performance shows that she is not an interpreter of Meera, but Meera herself'.

— Sarojini Naidu quoted in *MS: A Life in Music* (T. J. S. George, HarperCollins, New Delhi, 2004, p. 176)

1984 was a difficult year in India. On the 31st of October, Indira Gandhi had been assassinated by her own personal security guards. The riots that followed in the next few days resulted in the killing of large numbers of innocent people; acts of barbarism that have been emulated in later years. On the 3rd of December, methylisocyanate leaked out of the storage tanks in the Union Carbide factory at Bhopal, killing thousands of people and maiming thousands more. We have just marked the 20th anniversary of these tragic events. 1984 was also the year that marked the Golden Jubilee of the Indian Academy of Sciences. Founded in 1934, the Academy planned to celebrate the passage of half a century since its founding, with a meeting at Bangalore scheduled to begin on 7 November 1984. The date was chosen to coincide with the birth anniversary of the Academy's founder, C. V. Raman (1888–1970). Subrahmanyan Chandrasekhar, fresh from the 1983 Nobel prize in Physics, was to deliver the inaugural lecture. Indira Gandhi's death resulted in a postponement of the meeting, pushing it to the first week of February 1985. When the meeting began on February 6 at the Indian Institute of Science, the Academy marked the completion of 50 years, returning to the site of its inaugural meeting, which had been held on 31 July 1934. Almost exactly twenty years later, I remember little that was discussed at this meeting but two events remain vividly in my memory; Chandrasekhar's inaugural lecture and M. S. Subbulakshmi's Carnatic music performance in Bangalore's Chowdaiah Memorial Hall. I have no pretensions to understanding the nuances of astrophysics or music, but that day, 20 years ago, provided a remarkable glimpse of creativity, genius and accomplishment in science and music.

The Academy's Golden Jubilee meeting surfaced in my mind when M. S. Subbulakshmi (MS to her legion of admirers) died in Chennai on 11 December 2004. That day, the TV channels featured clips from many of her past performances, including a screening of the 1945 film *Meera*, which really launched MS on the national stage. I saw the Hindi version, which had an introduction by Sarojini Naidu, curiously enough, delivered in English. While a young and shy MS stands by her side, Sarojini Naidu hails her as a star, who will soon be known all over India; a prophecy that was to quickly be proved correct. I could not help wondering if scientific genius has been so quickly and publicly recognized, with such grace and generosity in recent times. I do not have a ear for music and would consider myself a Philistine in matters of art, culture and music. At times when I have been in the company of connoisseurs of classical music, I have wondered whether Shakespeare's famous warning might indeed apply to me:

'The man that hath no music in himself,
Nor is not moved with the concord of sweet sounds,
Is fit for treasons, stratagems and spoils;
The motions of his spirit are as dull as night,
And his affections dark as Erebus:
Let no such man be trusted.'

Lorenzo in *The Merchant of Venice*

But, my secret worries have been dispelled when I heard MS singing, most notably twenty years ago in Bangalore in the midst of a scientific meeting and again a few days ago when I watched *Meera* late into the night. Of the many tributes to MS that have appeared in recent times, I must draw attention to an essay by the historian and writer, Ramachandra Guha on 'Redeeming the Bharat Ratna'. Guha writes about the moment he heard that India's highest civilian honour had been conferred on MS and notes that H. Y. Sharada Prasad had once written that 'MS had long ago been anointed Bharat Ratna by the people of India, with her art recognized and cherished by anyone with a modicum of cultural intelligence' (*An Anthropologist Among the Marxists and Other Essays*, Permanent Black, New Delhi, 2001, p. 168).

The other memory that remains from the Academy's anniversary meeting in 1984 is Subrahmanyan Chandrasekhar's lecture '*The Pursuit of Science: Its Motivations*', the text of which has appeared in this journal (*Current*

Science, 1985, **54**, 1) and also as part of a marvellous collection of essays by Chandrasekhar on *Truth and Beauty: Aesthetics and Motivations in Science* (The University of Chicago Press, 1987, 1990). So much has been written about Chandrasekhar, including the eminently readable biography by Kameshwar Wali (*Chandra*: *A Biography of S. Chandrasekhar*, Penguin Books, 1992), that there is little new that can be said. He was the dominant figure of 20th century astrophysics, a man of formidable scholarship, and unmatched achievements in his areas of research. When he died in August 1995 in Chicago, a truly remarkable and inspirational chapter in science ended. Writing about Chandrasekhar, a year after his death, S. Ramaseshan notes: 'He had chosen perceptively, fields which were seemingly unfashionable which later were to blaze new trails. He had produced 50 or more remarkable students who were dotted all around the world. He had written essays on science, literature and the arts which were masterpieces. He had said to me – My work is finished and it seemed to me as though he had arranged with Providence for his passing away' (*Bull. Astron. Soc. India*, 1996, **24**, 537). But Chandrasekhar's approach to science is best summarized in his own words, in the autobiographical sketch that is found on the website of the Nobel Foundation: 'After the early preparatory years, my scientific work has followed a certain pattern motivated, principally, by a quest after perspectives. In practice, this quest has consisted in my choosing (after some trials and tribulations) a certain area which appears amenable to cultivation and compatible with my taste, abilities and temperament. And when after some years of study, I feel that I have accumulated a sufficient body of knowledge and achieved a view of my own, I have the urge to present my point of view, *ab initio*, in a coherent account with order, form and structure'.

Chandrasekhar's discipline was legendary, his mathematical abilities and physical insights of the highest order and his perseverance unmatched. In his autobiographical note he lists seven monographs which cover seven periods of his scientific life, ranging from the 1939 *An Introduction to the Study of Stellar Structure* to the 1983 *The Mathematical Theory of Black Holes*; a truly unbeatable record of scholarly achievement in science. His final book on Newton's *Principia* was completed just before his death. Although the 1983 Nobel prize appeared to recognize his classic work done over fifty years earlier (1931–1936), few scientists have matched his ability to produce scientific work of the highest quality over a period of six decades.

Chandrasekhar stands apart from most of his contemporaries in bringing to discussions of 'truth and beauty' a rigour of approach that is truly his own. His Nora and Edward Ryerson Lecture on *Shakespeare, Newton* and *Beethoven or Patterns of Creativity* (1975) must be required reading for all those who wonder about human creativity. In this lecture, Chandrasekhar, more than anyone else, before or since, bridges the gap between C. P. Snow's *Two Cultures*. He quotes T. S. Eliot's assessment of Shakespeare: 'The standard set by Shakespeare is that of continuous development in which the choice both of theme and of dramatic and verse technique in each play seems to be determined increasingly by Shakespeare's state of feeling by the particular stage of his emotional maturity at the time'. Reading this, I could not help but feel that Chandrasekhar too set a standard of 'continuous development' in which each body of work appeared to surpass his earlier accomplishments. Chandrasekhar seemed to be uncomfortable with the view that scientists do their best work when they are young; he was himself a wonderful exception. In his analysis of Shakespeare and Beethoven he marvels at their growing creativity as they age and concludes that there are differences in the patterns of creativity in science and the arts: 'And this to my mind is the center and the core of the difference; the apparent inability of a scientist to continually grow and mature'.

M. S. Subbulakshmi and Subrahmanyan Chandrasekhar were truly great contemporaries. They came from our part of the world, a fact that we must celebrate. Chandrasekhar spent his life in the West, but considered himself a foreigner in the surroundings he called his home. In thinking about these two remarkable Indians (and I deliberately ignore the mundane details of citizenship), I must turn to T. J. S. George's biography of MS. In considering the factors that turned 'Subbulakshmi into an exemplar of "Indianness"' George notes that 'the contrast between Mahatma Gandhi and Jawaharlal Nehru was itself illustrative. Gandhi pulled at the heartstrings of the masses because he symbolized the ethos of the Indian ideology in full measure. Nehru did not; he stood apart as a patrician prince because of his Westernization and agnosticism'. I could not but help wonder at the contrasts between Subbulakshmi and Chandrasekhar; the former loved and celebrated across the length and breadth of India, while the latter a truly patrician figure, remote, austere and undoubtedly one of the scientific greats of the 20th century. At a function in Chicago in April 1984, honouring the 1983 Nobel laureates, Chandrasekhar reflected on judgements: 'On an occasion, now more than fifty years ago (Edward Arthur) Milne reminded me that posterity, in time, will give us all our true measure and assign to each of us our due and humble place; and in the end it is the judgement of posterity that really matters. And he further added; he really succeeds who perseveres according to his lights, unaffected by fortune, good or bad. And it is well to remember that there is in general no correlation between the judgement of posterity and the judgement of contemporaries' (Wali, pp. 297–298).

In 1985, at the end of M. S. Subbulakshmi's concert before several hundred scientists, the Academy's president, Sivaraj Ramaseshan thanked her: 'I must call you Dr Subbulakshmi (a reference to her many honorary degrees), but today there are many doctors and there is only one Subbulakshmi'. I believe Ramaseshan would have approved if I concluded this essay by saying that there is also only one Subrahmanyan Chandrasekhar.

P. Balaram

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EDITORIAL

The science of the earth

The United Nations has declared 2005 as the International Year of Physics, commemorating the passage of a century after Albert Einstein's miraculous year in 1905, when he produced insights which forever transformed physics. I had intended to begin the year reflecting on the unreasonable influence exercised by physics (and physicists) on the public perception of science globally, and in India in particular. I was standing in Chennai on 26 December, a typically calm and placid morning, when even this normally hot and humid city seemed cool and pleasant, thinking about a column on physics. I noticed some activity outside the building as people seemed to rush about in some excitement, but nothing really registered in my mind. A few minutes later everyone was talking about the tremors that had been experienced. The shock of the great Sumatra earthquake had been felt over two thousand kilometers away. But no one was prepared for what was to follow. A couple of hours later the sea rose across the Andamans and the Nicobar islands, the southeastern coast of India and across much of Sri Lanka, sweeping thousands of people to an instant death. In the days that followed, the loss of life across the affected regions of Asia climbed to a staggering 1,50,000, Indonesia being the hardest hit, followed by Sri Lanka. The word 'tsunami' had entered our vocabulary.

Tsunami is a Japanese word; in literal translation its meaning is given as a *wave in port* or a *harbour wave*, conveying little of the menace and ferocity that we have now come to associate with the waves of 26 December. Tsunamis have been widely studied and generally considered to be a phenomenon of great interest and concern in the Pacific Ocean. The Indian Ocean states have had little cause for worry until the recent disaster. Tsunamis can be triggered by major undersea earthquakes and by every count the Sumatra upheaval must rank as one of the great earthquakes of all time, some estimates placing it as high as 9.0 on the Richter scale. Once in motion, a tsunami can travel with surprising speed, up to 700 km/h, the wave height increasing as land approaches, even as the speed decreases. Tsunamis can be tracked, modelled, simulated and, in principle, a warning system can be put in place. In the aftermath of 26 December, a greater focus on tsunamis will undoubtedly become inevitable. It is of course debatable as to how quickly warning systems can be translated into administative action on the ground, in evacuating those at the greatest risk. In poor and populous countries, the logistics of evacuations in the face of impending natural disasters, is formidable.

The tsunami tragedy has brought forth a remarkable public response in terms of relief efforts; a clear sign that in India the participation of non-governmental bodies is growing in strength. The reactions have been spontaneous and substantial. There have, of course, been the inevitable discussions. How much of the tragedy was avoidable? Would a warning system have been effective? Could quicker action have saved more lives? Are there purposeful strategies to mitigate natural hazards? In the many tutorials on tsunamis that have appeared, in the last fortnight, it is clear that the physics of tsunami waves is well understood. However, the prediction of earthquakes remains beyond the reach of modern geophysics and seismology. This is a factor that seems inadequately appreciated, as witnessed by the false alarms of new seismic disasters in the days after 26 December. Inevitably, there were unhappy moments for the government departments, which had difficulty in assessing warning data received from overseas institutions. The reactions in the days that followed the tsunami underscored a known fact; the coordination between ministries and science departments in Delhi is almost non-existent and any emergency exposes this unfortunate state of affairs.

In many ways the discussions that have begun resemble those that followed the Bhuj earthquake of 26 January 2001. The Prime Minister used the inauguration of the 92nd Indian Science Congress at Ahmedabad on 3 January 2005 to ask if 'the country could have made better use of science and technology to alleviate, if not prevent, human suffering'. In calling for a 'better understanding of the natural phenomena that led to such disasters and of human activities that aggravated them' he was indeed echoing sentiments expressed after each brush that we have with the forces of nature. As with Bhuj, the enormity of the Sumatra earthquake and the accompanying tsunami waves will fade quickly from public consciousness, as the world moves on. But these natural upheavals are a sharp reminder that the Earth and our environment are subjected to strains and pressures, which are sometimes relieved in cataclysmic fashion.

Understanding the science of the earth, oceans and atmosphere must of course be a high priority and the disciplines of physics, chemistry, biology and mathematics must contribute in substantial measure. However, at present the study of earth sciences in India is at its lowest ebb. Geology is a subject that is taught classically and traditionally in many places, dull, descriptive and untouched by the excitement of modern science. These courses have few takers, students coming towards the earth sciences only after many other options have been exhausted. While science courses in universities have been generally struggling, I suspect the plight of earth sciences is particularly poor. There are national laboratories that have a mandate to do research in earth and ocean sciences; the National Geophysical Research Institute (NGRI), Hyderabad and the National Institute of Oceanography (NIO), Goa, both constituents of the CSIR. There are also institutions of the Department of Ocean Development. But, a hard look at the output of research in earth sciences (and I use the term in its broadest sense to cover the atmosphere and the oceans) will undoubtedly reveal a published record that is limited both in terms of quality and quantity. Earth scientists contribute quite substantially to this journal and in my years at the editorial desk, I have developed some feeling for the issues they address. My, admittedly prejudiced, view would be that most often the work carried out does not seem in tune with mainstream activity of the international community of earth scientists.

The depressed situation of the earth sciences in India must be a matter of serious concern in circles which consider science and academic policy. The community has very limited presence in most major institutions. There are very few research groups that appear to be training a new generation of students in oceanography, geophysics, geochemistry or seismology. The quantum of hard scientific research that emerges from India in these areas is not commensurate with their importance to the surroundings. Much of the discussion in the area of atmospheric sciences and climate change seems driven by conference agendas and international pressures. Even in these areas there is limited effort at hard data collection and analysis. The softer option of discussing policy issues based on international data seems to be the preferred alternative. The earth sciences have suddenly become the meeting ground for a diverse array of interest groups – environmentalists, policy makers, climate change activists and a host of others who wish to influence global policies to their advantage. In this rather confused scenario it is necessary to have a generation of well informed, thoughtful earth scientists, who specialize in diverse technical issues. For this to happen a concerted attempt to revitalize some of our major earth science departments and also the starting of new and modern programs in some of our best institutions may be a step worth considering. India has benefited enormously from its investments in the Indian Space Research Organization. A similar commitment to earth science may pay rich dividends in the future. In considering the science of the earth my attention was drawn to a recent issue of *Nature* (23/30 December 2004), which reproduces comments on oceanography that appeared 50 years earlier: '... the change of emphasis in oceanography from exploration and survey to research directed towards the precise understanding of the basic physical and biological processes makes it necessary to devote more effort to theoretical and experimental work.... It would no doubt be wrong to suggest that oceanographers took up the subject because they found the mathematical and physical sciences too difficult and unattractive; but they have been rather slow in applying the precise techniques of these sciences to marine research.... To increase our knowledge the subject must be made attractive to men who do not mind facing up to the difficulties of fluid mechanics (*Nature*, 25 December 1954)'.

Nearly four years ago in the aftermath of Bhuj I wrote: '...modern earth science is a melting pot of disciplines; our own institutions are notably insular. At the level of basic research the earth sciences maintain a low profile. In the hierarchy of desirable professions, science itself ranks low; the earth sciences (and this may be a prejudiced view) appear to be even less sought after than sister disciplines' (*Current Science*, 2001, **80**, 317). Now in the aftermath of another major disaster, I can only say that nothing has changed for the better. The earth science scenario in India remains bleak.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Scientific Writing and English

The Hatter opened his eyes very wide on hearing this; but all he said was, 'Why is a raven like a writing desk?'
'Come, we shall have some fun now!' thought Alice. 'I'm glad they've begun asking riddles. – I believe I can guess that', she added aloud.
'Do you mean that you think you can find out the answer to it?' said the March Hare.
'Exactly so', said Alice.
'Then you should say what you mean', the March Hare went on.
'I do', Alice hastily replied; 'at least – at least I mean what I say – that's the same thing you know'.
'Not the same thing a bit!' said the Hatter. 'You might just as well say that "I see what I eat" is the same thing as "I eat what I see"!'
'You might just as well say', added the March Hare, 'that "I like what I get" is the same thing as "I get what I like"!'
'You might just as well say', added the Dormouse, who seemed to be talking in his sleep, 'that "I breathe when I sleep" is the same thing as "I sleep when I breathe"!'

—Lewis Carroll
Alice's Adventures in Wonderland

The *Mad Tea-Party* and the combined assault on Alice's language, by that admirable trio of the Mad Hatter, the March Hare and the Dormouse, came to my mind as I struggled to read two Ph D thesis drafts and, of course, a bundle of correspondence for *Current Science*. Doctoral theses describe years of work by students enrolled for Ph D degrees, often running to a couple of hundred pages of text. Ph D theses are difficult to read and I have often wondered whether examiners really wade through the entire text. In our institutions, the Ph D thesis is sometimes a collective responsibility of the student and the research supervisor. At the level of day to day research, students and their guides usually develop a convenient working relationship, where individual responsibilities are often clearly defined. Usually the students do the laboratory work; supervisors help in analysing results and write manuscripts. Rarely does one come across a student, who produces a clear and readable manuscript, without any help. Correcting a draft Ph D thesis can be an ordeal. Even as I try to complete two such exercises at the same time, I must reflect on why most students (and indeed many authors who contribute to this journal) write so poorly.

The Indian Institute of Science, where I work, is truly an all-Indian institution. Students are drawn from all parts of the country and come from widely varying social and economic backgrounds. They have been educated at schools, colleges and universities dotted across the length and breadth of India. Most of them are bright, motivated, hard-working and committed to their research. But, a large number share a disability; they are unable to write clearly and correctly in English. They are unable to follow the March Hare's dictum and 'say what they mean'. I suspect a similar situation must exist across the country, with clear scientific writing skills becoming a rare commodity. The teaching of English in schools has deteriorated dramatically over the years, even in the so-called English medium schools. Students who study in schools that use the state language as the medium of instruction, face a formidable hurdle when they enter university science courses. Curiously, even students who appear to speak English fluently, write very poorly. Grammar and spelling are not considered important. This slide in style is being catalysed by e-mail and SMS, where complete words and sentences are to be avoided and punctuation is forbidden. Even when writing skills are adequate, the task of writing a Ph D thesis can be daunting. A book entitled *Writing Your Thesis* by Paul Oliver (Sage Publications, 2004) recently crossed my desk. In a little less than 200 pages, the author provides a detailed set of instructions on preparing a thesis, including a short section entitled *Grammar, Punctuation and Conventions of Academic Writing*. In discussing writing conventions, he has a nugget to offer: 'Consistency is perhaps more important than the choice of convention which has been made' (p. 78). There is nothing more distracting in manuscripts than undefined abbreviations, multiple systems of units (for example temperatures quoted in degrees Celsius and Kelvin, interchangeably) and ever-changing nomenclature of molecules. It is also common to see a complete disregard for the significance of numbers. The terms, precision and accuracy, mean little to most authors. In many manuscripts submitted to this journal and in many theses I have read, references are cited with a cava-

lier disregard for accepted style; even more annoyingly citation style is often inconsistent within a single list of references. A lack of discipline in writing, coupled with linguistic disabilities, makes most manuscripts, both theses and papers, hard to read and sometimes impossible to correct. An example that I might cite is the case of the *Correspondence* columns of this journal. In many letters (and these are intended to be brief) that are received it is hard to decipher what the author is trying to say. Sometimes these are edited (with considerable difficulty) and published with the author's concurrence. But, I am left wondering; did the author eventually say what he meant?

Scientific writing, theses or papers, is intended to present clearly the purpose and outcome of a specific research investigation. It is important to be both succinct and explanatory. Authors do not usually have the luxury of following the King's sage advice to the White Rabbit in *Alice's Adventures in Wonderland: 'Begin at the beginning and go on till you come to the end: then stop'*. When the moment of writing arrives many authors confront a formidable mental barrier. Laboratory research seems so much simpler and enjoyable. There is an urge to postpone writing and both students and supervisors suddenly find so many more important things to do. Oliver notes in his introductory paragraphs: 'Writing is a largely solitary process, and progress may seem to be very slow. The task may seem to stretch away to infinity'. He, of course, notes optimistically that 'this book will help you with writing your thesis, from the moment you type your first word, to when you walk into the viva voce examination to defend the completed work!' The general reading habits of most students in our institutions is limited and I am not sure too many will have the patience or the inclination to study Oliver's prescriptions. It may indeed be easier to transfer the burden of writing correctly to a supervisor. This attitude also surfaces in a lot of correspondence that crosses my table at *Current Science*. Many authors believe that their results are so important that the task of editing, tightening and correcting language must be, automatically, the responsibility of the journal. A most annoying class of authors are those who prepare manuscripts carefully for 'high impact' journals, but treat our own journals with contempt, by submitting manuscripts that do not seem to have been read carefully by any of the authors. There are also 'senior authors', academicians and managers of science occupying positions of power and authority, who believe they cannot be asked to condense or correct the most verbose and poorly written manuscripts.

Science communication, both spoken and written, is carried out overwhelmingly in English. Even in the 1960s and 1970s German and French seemed important, with many major journals publishing in these languages. Russian was becoming important; libraries paid an enormous cost to buy cover-to-cover translations of journals emanating from the erstwhile Soviet Union. The influence of 'market forces' was evident in scientific publishing even before the phrase became popular. German and French journals, many with a long and proud history that dated back to the 19th century (a time when modern science began its triumphal march in Europe and in England), slowly converted themselves into journals that published papers in English. *Annalen der Physik*, *Liebig's Annalen der Chemie* and *Chemische Berichte*, famous journals of yore, simply disappeared from our consciousness. The highest impact journals published from Europe today appear in English; even professional societies that were constrained by language and national boundaries have integrated across Europe. In a delicious irony of history, English is the language that binds European science, even as England's political and economic influence has declined. The events of the early 1990s, particularly the dramatic disintegration of the Soviet Union, has hastened the process of acceptance of English as the language in which science is transacted. Globalized economies require a link language; the evolutionary forces of history have selected English. In preparing for future challenges, Russia and China invest heavily on the teaching of English, a language that is necessary if the next generation has to be competitive and successful. In the countries of Eastern Europe, liberated from Soviet dominance, English has moved smoothly into the position of an essential second language.

In India, English was taught widely and, possibly, well. The divide between urban and rural schools was, inevitably, very substantial. But, in exorcising the ghost of Macaulay, the language policies of the 1960s converted, the teaching of languages in our school education system into a formidable obstacle course. The average child burdened with the study of three languages, most often taught indifferently, does not learn any of them well. Unsurprisingly, we produce students, who enter the higher education system with a significant handicap, despite their intrinsic talent. The language policies in India were framed in the backdrop of the intense anti-Hindi agitation that took place in the state of Madras (now Tamil Nadu), almost exactly forty years ago. The drive to impose Hindi, which had considerable political support, was halted and English retained as a link language in which the Central Government transacted its business. The formulae which emerged in the cauldron of the 1960s have placed a major educational burden on future generations. In a provocatively entitled article 'Hindi against India' (a title borrowed from a book by Mohan Ram on the 'official language controversy'), Ramachandra Guha writes about the political transformation in Tamil Nadu, in the years that followed: 'Not for the first (or indeed last) time, linguistic chauvinism has carried with it a massive political cost' (*The Hindu*, 16 January 2005).

In evaluating the role of English in our educational system, we must ensure that sentiment does not force us to pay an unaffordable developmental cost.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Popularizing Science: Memories of an Icon

Malgudi, the magical town that came to life in R. K. Narayan's novels, may have been inspired by the author's life in Mysore of a bygone era; its name, however, is a felicitous blend of two of Bangalore's oldest suburbs, Malleswaram and Basavangudi. Even in the early 1970s these areas retained an old world charm, largely untouched by the frenzied pace of development that was soon to follow. Bangalore today is, arguably, India's 'science capital'. Delhi may boast of a larger number of institutions of science and Kolkata of a tradition of research that goes back to the 19th century, flowering into the high point of the 1920s; but it is Bangalore that is home to some of our finest institutions of science, both venerable and young. The city is also home to a remarkable institution, *The Bangalore Science Forum*, which was founded by H. Narasimhaiah in 1962. For well over four decades, the *Science Forum* has held weekly lectures to bring science to the people. Its record of an unbroken string of lectures, interspersed with film shows on science (slide shows in the early years) is unlikely to be rivalled; in July 2004 the forum conducted 'Lecture No. 1956'. When its founder-president died in Bangalore on 31 January, an extraordinary chapter in the history of science in India, ended.

Hosur Narasimhaiah (HN to his legion of admirers) was born on 6 June 1920 in Hosur (Kolar district) and grew up in extremely modest circumstances. According to legend, he walked to Bangalore in 1935 to join the National High School in Basavangudi. A year later, Mahatma Gandhi on a visit to Bangalore spoke to the young Narasimhaiah; a meeting that was to transform the boy into a life-long follower of Gandhi's ideals. He obtained his B Sc and M Sc degrees at Central College, Bangalore and joined the National College, Basavangudi in 1946. His career was interrupted by stints in prison at Bangalore, Mysore and Yerawada jails, during the Quit India movement. Later, he was to obtain a Ph D in physics from the Ohio State University. He served for several years as Principal of the National College and was later Vice-Chancellor of the Bangalore University (1972–1977). During his long career as an educationist, HN remained completely committed to Gandhian ideals and the cause of promoting science and spreading a scientific temper among the people at large. He lived a life of extraordinary simplicity. A bachelor, he spent over half a century in a small room in the National College hostel. Always dressed in white khadi with a 'Gandhi cap', he seemed at times a figure from a chapter of Indian history, that appears to be fast fading from public memory. His contributions to the National Education Society and its institutions were undoubtedly immense, but I believe it is his indefatigable championship of the cause of promoting science that really extended his influence well beyond the National College.

I first met HN in the mid-1970s when I was invited to speak at the *Science Forum*, probably as a substitute for a truant speaker. Young, brash and undoubtedly arrogant, I encountered an entirely new world at National College. Simplicity and humility seemed to be prized virtues. Over the last three decades, I have gone back repeatedly, never refusing an invitation to speak. HN and his dedicated colleagues conducted summer schools for 10th standard and Pre-University students and in my younger years I was a regular teacher. On my long rides in an autorickshaw from Malleswaram to Basavangudi, I could not but help think of the gulf that separated my own concerns at the Indian Institute of Science from those of HN and his colleagues at National College. I seemed more worried about personal advancement and success; HN and his more dedicated followers appeared to be driven by a nobler purpose. The *Science Forum*, for the past quarter of a century, has also conducted an 'Annual Science Festival', where lectures and film shows on science are held every day in the month of July. Year after year, some of Bangalore's most senior and influential scientists speak at this festival, drawn undoubtedly by HN's personal charm and charisma. It was almost impossible to say no to him; although it was sometimes possible to negotiate on the date of a lecture. Some years ago the *Forum* marked the centenary of Schrödinger's birth. Bangalore has a galaxy of physicists and HN had little difficulty in rounding-up speakers. But, with characteristic thoroughness he wanted to cover Schrödinger's views on biology. Having failed to persuade either physicists or biologists of standing to speak on the topic, he turned to me. My protestation that I was ignorant did not make him relent; he simply told me to study and prepare to speak. It was only then that I really read Schrödinger's book *What is Life?*, which greatly influenced the physicists who turned to biology in the 1950s. Until HN instructed me to

read about Schrödinger, I had only glanced through the book, untouched by the context of the times in which the author had worried about genes and the transfer of hereditary information. But, after all these years what has remained with me is the quote that Schrödinger uses in his preface: *There is nothing over which a free man ponders less than death. His wisdom is to meditate not on death but on life* (Spinoza). This is a sentiment that HN would have heartily endorsed. The last time I saw him was in July 2004 at the Science Festival. He had telephoned me in early June and announced simply: 'July is coming'. He noted that it was the 50th anniversary of G. N. Ramachandran's famous paper on the structure of collagen and suggested I should talk about Ramachandran and his contributions. I readily agreed, since the topic was close to my heart. He was present, as always, at the *Science Forum*, frail and weakening but still determined to introduce the speaker. Before the talk, he offered me the *Forum's* famed coffee, lukewarm and in a small cup, which had seen better days, in surroundings simple enough to shame even the most insensitive. It was always a privilege to meet him. Talking to him was both humbling and ennobling. But, on this occasion it was clear that he was fading. My one regret is my crippling inability to converse in Kannada, a language that HN loved and used so effectively. Separated by barriers of age, lifestyle and language, I could only admire him from a distance, coming away enriched and inspired by every visit to the *Science Forum*.

Narasimhaiah's drive to spread the message of science was fuelled by a desire to eliminate the crippling superstitions that abound in our society. He was tireless in his campaigns, taking on politicians and those who profess to perform miracles. Ironically, even as he entered the final phases of his struggle against the illness that was to claim him, Bangalore was engulfed in a controversy over the mammoth meetings held by the American faith healer, Benny Hinn. While searching the Internet, I came across an interesting website of an international organization, *The Committee for the Scientific Investigation of Claims of the Paranormal*. Here the list of 'fellows' includes some of the most easily recognizable names in science: Richard Dawkins, Martin Gardner, Murray Gell-Mann, Douglas Hofstadter, Leon Lederman, John Maddox, Marvin Minsky and Steven Weinberg. The list also includes Narasimhaiah, who I suspect more than anyone else, worked tirelessly, but unsuccessfully, to test the claims of paranormal phenomena that crop up in our midst.

In thinking about Narasimhaiah and his lifelong quest to spread the message of science, in his surroundings, we may do well to recall that Jawaharlal Nehru's unstinting support of science in newly independent India was also motivated by his perception, that a rapid spread of a scientific temper would speed the process of development. The document on the government's Science and Technology Policy–2003 begins with a quote that bears reproducing: '*...we must take science to the people. All of us are fond of quoting Pandit Jawaharlal Nehru's famous words paying a tribute to your community – namely, that "Scientists are a minority in league with the future". This is true. But let us also remember that a bright future can be realized only when science is in league with the majority of our society*' (Atal Bihari Vajpayee). The S&T Policy–2003 specifically promises support for 'programmes that seek to popularize and promote science and technology in all parts of the country'. HN's *Bangalore Science Forum* was far ahead of all government initiatives and was spearheaded by an intensely dedicated group of people. In its early years the lectures were held in an unprepossessing room of the National College. Later, a larger lecture room with modest furniture became available – the 'HN Hall', which I suspect will become a symbol of the science popularization movement in India. In recent years, the audience at the weekly evening lectures has been dominated by elderly citizens, with an interest in science. HN was often concerned with the declining interest of college students (even those studying science) in general topics of scientific interest. He was, of course, pragmatic enough to recognize that the pace of modern life left little time for academic interests; Bangalore of the 1960s had retreated to a past that seemed as fictional as Narayan's Malgudi.

Narasimhaiah died almost exactly 57 years after his idol, Mahatma Gandhi. Throughout his life, HN pursued the apparently contradictory visions of a Gandhian ideal and the task of promoting the public understanding of modern science. The words used by an American admirer to describe Gandhi seem particularly appropriate for HN: '*... he was a man in whom opposing virtues and interests were held in living tension and reconciliation... You cannot think of him without an inner sense of pleasure and gratitude. The incense that arises from the memory of his life is a "sweet savour". But while the savour is sweet, the preponderant impression he leaves is not sweetness but strength*' [E. Stanley Jones in *Profiles of Gandhi* (ed. Cousins, N.), Indian Book Co, Delhi, 1969, p. 137]. H. Narasimhaiah's formidable discipline, commitment and ascetism set him apart from the rest of us. Like his idol Gandhi, he was an easy man to admire, but a hard man to follow.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Reinventing our universities

Our Universities are so engrossed today with the task of conducting examinations and with innumerable meetings of Boards and Faculties, Courts and Councils, Senates and Syndicates that they have no time to perform the highest function of a University which is to stimulate intellectual activity and advance knowledge. There is a danger today of its being forgotten that examinations and Faculty meetings are only a means to an end and not an end in themselves. There is a danger today of the production and advancement of knowledge receding into the background in the intellectual outlook of our Universities, of their being regarded as something beautiful and great, like the white snow in the top of the Himalayas, to be admired from a distance but not to be grasped or touched.

—C. V. Raman

Convocation Address, Benares Hindu University
January 1927

Intellectual activity of the highest type such as every University should strive to develop is a force of incalculable power and importance for the national welfare. Apart from the direct results of such activity in promoting agricultural, industrial or commercial progress, its indirect results are even more important. Intellectual stagnation is equivalent to national decay and death. Intellectual activity, on the other hand, leads to a quickening of the national life in all its aspects. ...The mainsprings of intellectual activity in every country are education and the spirit of enquiry, and its quality varies with the standard set by the thinkers and educators of the nation. Thus, in the last analysis, it is the leadership offered by the Universities that determines the level of intellectual activity in the country and therefore also the national efficiency.

—C. V. Raman

Convocation Address, Mysore University
24 August 1929

A scholarly friend, recently discovered the Convocation Addresses delivered by C. V. Raman at the Universities in Mysore and Benares, over seventy years ago. Raman's concerns seem even more relevant today. The Supreme Court recently declared the Chattisgarh Private Universities Act, 2002, unconstitutional, stripping an extraordinarily large number of 'universities' of their legal status. How did a small and young state like Chattisgarh suddenly sprout so many 'universities'? The answer is of course, obvious. Avariciousness, when disguised as populism can be a powerful instrument. The state government was ostensibly driven by the noble desire of making higher education available to all. The easiest course seemed to be to throw the higher education sector in the state open to private enterprise. This move was clearly designed to make the peddling of 'university degrees' a financially attractive proposition, with every local tutorial college and teaching shop being elevated to the status of a university. With hundreds of thousands of gullible students as customers for degrees, there is clearly a wonderful opportunity for free marketeers in higher education. Selling 'education' is undoubtedly an attractive commercial proposition; the customers are uncritical and undemanding, interested only in degrees, which they hope will be a passport to a better future. With money to be made, the inevitable nexus between politicians and profiteers has been quickly forged in the higher education sector. The Chattisgarh episode is only an extreme example, because the state, suddenly sprouted dozens of universities. Fortunately, Yash Pal, a former chairman of the University Grants Commission (UGC) petitioned the courts, in the public interest. The Chattisgarh case demonstrates the absence of control by central bodies like the UGC, over events in the states. Since 'higher education' remains a 'state subject', its quality varies widely across the country and the gulf between central and state universities can be substantial.

We need to urgently reflect on the state of our publicly funded universities as the field of higher education is in the throes of a major transition. The agreements reached under the World Trade Organization have opened the area of higher education to foreign participation. Globalization and privatization are words that now affect the higher education system; foreign 'education providers' can now set up shop in collaboration with Indian institutions. Degrees bearing the names of foreign universities can be acquired by studying in local institutions, many of which are of relatively recent origin, with credentials that merit careful scrutiny. The National Institute of Educational Planning and Administration (NIEPA), Delhi, an autonomous organization funded by the Government of India, has recently com-

pleted a study of 'Foreign Education Providers in India' (Prabhu, N., *The Hindu*, 14 February 2005). The results reveal that six states, Tamil Nadu, Maharashtra, Delhi, Andhra Pradesh, West Bengal and Karnataka account for 108 of the 131 collaborations listed. The foreign institutions are almost exclusively from the United States and Britain; other exporters of 'education' like Australia, New Zealand and Canada appear to be waiting in the wings. The overwhelming number of foreign courses offered in the collaborating institutions has so far been vocational in nature; hotel management, business management and the ubiquitous courses on human resource management. There are several technical courses including some in areas that seem improbably advanced, like Micro Electromechanical Systems (MEMS). Very few courses provide general education. The glamour of a foreign degree draws students, who often pay handsome fees running to about Rs 2–3 lakhs per year. Peddling degrees, both Indian and foreign, can be a lucrative business. The dramatic transformation of the higher education scene in India is reflected in the 'education supplements' produced by major newspapers. Every conceivable course is advertised, with marketing professionals undoubtedly contributing their skills; selling 'education' may soon be as profitable as selling soap.

Even as private universities and foreign institutions jostle for the higher education market, the vast network of state-funded universities in India appears to be sinking into a state, where academic performance, both teaching and research, is slowly becoming irrelevant in the conduct of university affairs. Controversies dog the appointments of Vice-Chancellors with alarming regularity; testimony to the levels of politicization in the universities. Senates and Syndicates often work at cross purposes with the university administration. C. V. Raman, whom I have quoted at the beginning of this commentary, would shudder to think that many of the mindless meetings that he deplored, have today become active instruments which work hard to prevent any reform. The most distressing feature of our university system is the negligible importance given to academic performance of the faculty, who really are the backbone of the university. Erudition and scholarship are no longer pre-requisites for advancement through academic ranks. Universities are now full of professors, who do no research and do little teaching. Undergraduate teaching was largely banished to constituent colleges a long time ago; a sharp contrast to the situation in Western universities. Research is considered an activity best left to national laboratories and all attempts to infuse funds and facilities into university departments have met with only limited success. Research, both in science and the humanities, requires an ambience where academic pursuits are encouraged, scholarship is prized and mediocrity is not consciously elevated. Such an environment is sadly lacking in a majority of our universities. State universities, with a few notable exceptions, have been sliding downward in their academic performance at a rate somewhat greater than their centrally funded counterparts. The fall in scholarly output is most evident for universities which were once India's pride. Output today is measured in terms of number of degrees awarded; research output is hardly demanded by university administrators. Raman was prescient when he said, at Mysore in 1929: 'In thus drawings attention to the relationship between national welfare and the work done in the Universities, I would wish to emphasize the danger of allowing such work to degenerate into a deadly routine of formal teaching and formal examination. The tendency of all formal schemes of education is to regard the human mind as a kind of soft metal to be squeezed into shape by intense pressure into the hard steel moulds of syllabuses and examinations. This kind of mechanization of education is, I believe, most deadly in its effects, and is responsible for not a little of the intellectual sterility of the finished products of our Universities... The ideal of University work is the provision of opportunities for special abilities, of teacher as well as students, to express themselves'.

Raman was untiring in his efforts to propagate the idea that Universities must be centres of research. In his Mysore address he was eloquent: 'Indeed knowledge as embodied in a formal treatise or lecture is apt to convey an impression of static perfection which is deceptive and induces the mind to adopt an attitude of uncritical acceptance. The highest qualities of mind are evoked by knowledge which presents itself in a dynamic but imperfect form, calling for criticism and personal investigation for its adequate appraisal. This is the real value of research as an instrument of education in itself'. A few years later at Benares, Raman was more emphatic: 'You must be one of the seekers, or else you will get left behind... You cannot tell the depth of the river or learn to swim in it by standing on the bank and watching it flow by, but must pluck up courage and plunge into it. So long as our teachers and our students are not inspired in their daily work by such ideals, so long as it is not research but administration that dominates the outlook of our Universities, we can hope for no great advance either in the intellectual output of India or the quality of the work done in our centers of learning'.

Our universities are threatened today, both from within and without. The internal threat of degeneration can be met only if there is a confluence of interest between administrators and academicians. The bodies of the government charged with the responsibility of overseeing our university system need to act quickly and decisively in rejuvenating our system of higher education. Reinventing the Indian university will neither be an easy task, nor will it be pleasant.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Budget Reflections

The annual budget is presented invariably on 28 February, a date that coincides with National Science Day, which celebrates C. V. Raman's historic discovery of the effect which bears his name. Science is inevitably overshadowed, the morning after, when the press pronounces whether the budget was a dream or a nightmare. Economists appear on our TV screens, often repeating whatever they had said in previous years. The Sensex moves up or down, a measure of approval or disapproval of the Finance Minister's complex act of juggling the figures. For a brief moment everyone is alerted to the perils of excessive government spending and the need for fiscal responsibility. The public (that large formless body whose reactions are captured in interviews of a few people and then extrapolated) is largely interested in personal taxes; a good budget is one where the relatively affluent tax-payer pays less to the exchequer. Excise duties are also watched with some interest; the prices of common goods being linked to budgetary policy. In the days preceding the budget, the Finance Minister (often, though not always) meets with groups which represent various sections of the economy, industrialists prominent among them. Science and technology (S&T) is also one of the 'interest groups' and invariably the plea is for greater investment in S&T. Representatives of industry argue for greater tax concessions for expenditures in research and development. The pre-budget meetings are usually a case of everyone 'asking for more' from the Finance Minister's plate; for a brief moment even the rich and the powerful resemble Oliver Twist. The Minister and his aides are usually affable and receptive; secure in the knowledge that budgets can rarely be influenced by last minute pleas for concessions. The consultative process having been served, everyone waits for budget day, which is one of the rare days when a long speech can be made (and possibly heard) in Parliament, without significant interruptions. This year, Mr Chidambaram's speech was made in the backdrop of the state elections in Bihar, Jharkhand and Haryana and the inevitable political confusion that followed. Politics and the budget drove Science Day completely out of the newspaper pages on the following day. Even the announcement of the India Science Award (the country's newest and most prestigious recognition of scientific achievement) to C. N. R. Rao did not merit a mention. There was no mention in most newspapers of President Abdul Kalam's long speech devoted to science and technology. Ironically, but happily, the Finance Minister put science on the pages of the newspapers, by announcing a special grant of Rs 100 crores to the Indian Institute of Science (IISc), Bangalore.

IISc, which has been my home for over three decades, is one of India's oldest research institutions. Approaching its centenary in 2009, the institution ranks as the foremost centre for research in science and engineering in this country. A quiet and conservative institution until the 1960s, IISc has grown dramatically in size and the breadth of its scientific activity over the past thirty years. Its character, which blends the ambience of a university with the advantages of a research institution, has helped IISc to maintain a steady level of academic performance, even in difficult times. In announcing this special grant Mr Chidambaram raised the bar of public expectation: '*Government believes that investments in institutions of higher education and Research and Development organizations are as important as investments in physical capital and physical infrastructure. What we need are world class universities, and we must make a beginning with one institution. We must have a university that will be ranked alongside Oxford and Cambridge or Harvard and Stanford. I am happy to inform the House that we have selected the Indian Institute of Science, Bangalore, which enjoys a high reputation as a center of excellence in research and development. We shall work to make IISc, in a few years, a world class university. I propose an additional sum of Rs 100 crore as a grant for this purpose.*' Budget speeches often provide Finance Ministers an opportunity to philosophize or display their tastes in poetry; this year was an exception leaving the press with enough place to highlight this focus on higher education and research. The reactions appeared quickly enough. Editorials in the Bangalore papers termed Mr Chidambaram's generosity as a boost for science in the 'information technology capital' (*Indian Express*, 2 March 2005); implicitly differentiating between scientific research and the IT boom, which has not really required frontline research as a fuel. The editorial was quick to note that, 'it would be foolhardy to believe that the Rs 100 crore grant is enough financial fuel to speed rocket IISc into the top

10 bracket where universities of Oxford and Harvard, Stanford and the Massachusetts Institute of Technology figure'. Another commentator noted thoughtfully: 'One of the attractive features of Mr Chidambaram's fourth Budget is the way it reflects the sense that India can now raise its sights. There is the Rs 100 crore provided to the Indian Institute of Science so that it can match the best universities in the world... But the budget of Harvard ...is in many multiples of Rs 100 crore. Harvard hopes to raise $5 billion (Rs 22,000 crore) as additional money to fund its own reform program and already has an endowment of $22.6 billion (Rs 100,000 crore!). This is not to argue that all costs are comparable, they are not, and the rupee will go much further than the dollar... But the gap remains vast. And the non-financial elements of institution building are even more important than money. Still, it is good to benchmark against the best in the world, like India's best companies now do' (T. N. Ninan, *Business Standard*, 5/6, March 2005). But, not all reactions were favourable. There were letters in the correspondence columns which noted, with some degree of justification, that a substantial proportion of IISc's alumni drifted irreversibly to the West. Should not the alumni make a greater financial contribution to the institution and the country that had provided them a wonderfully subsidized education? This is an oft-repeated criticism that has been levelled at the IITs and, at times, also at IISc. The only defence is that a significant fraction of the alumni have contributed very substantially to many spheres of national development. It is also likely that as a new thrust is provided to the scientific enterprise in India and industrial R&D moves into higher gear there will greater and more attractive employment opportunities for highly qualified scientists.

This year's budget has also provided a significant increase in the outlay for the various science departments: 2005–2006 (2004–2005), Rs crores, Department of Atomic Energy (R&D) 872 (704); Department of Space 2800 (2400); Department of Biotechnology 445 (310); Department of Ocean Development 340 (200); Council/Department of Scientific Industrial Research 846 (650); Department of Science and Technology 1250 (900). These amounts are still small when compared to countries with which we measure ourselves, like China. Nevertheless, there is a clear 'expression of intent' to drive the engine of science into higher gear. The figures projected in the US budget are, of course, staggering. A total figure of $132 billion in 2005, includes $28.6 billion for the National Institutes of Health and $5.4 billion for the National Science Foundation (*Science*, 2005, **307**, 832). India can afford only a limited investment in many sectors and this must be made to go a long way. One of our most optimistic protagonists of science noted sometime ago: 'Our overall S&T budget in 2002–03 was less than $3 billion. Pfizer's R&D budget was over $5 billion. For a national budget that was smaller than the budget of a single company, India has achieved so much' (R. A. Mashelkar, Convocation Address, Delhi University, 2003).

Increasing investments in science and technology place a great responsibility on the scientific community. IISc, as the guinea-pig in this experiment, will be clearly expected to raise its level of academic performance. Money is only one ingredient in scientific success. An ambience which challenges and demands a high level of faculty and student performance is undoubtedly essential to raise the standards of academic output. It must not be forgotten that the best universities can also be the most demanding; rarely condoning shortfalls in research or teaching. In India, it has become easy to camouflage academic non-performance by strenuously citing financial and administrative difficulties. It is not uncommon to see major facilities underused, with little effort to raise the level of technical competence which is necessary for much of modern day research. Scientists need to be more conscious of their responsibility in contributing to a collective enterprise of enhancing institutional performance. The importance of collaboration and cooperation is clear; enlightened self-interest points to the advantages of collaborative efforts. Even in basic research it should be possible for some groups to define some 'overarching objectives' (a phrase I heard in a recent meeting), which if even partly achieved can provide a stimulus for greater performance. Indian institutions do not generally worry about voluntarily raising the bar on performance. They do not also indulge in planning for the future. The government provides a convenient whipping horse which can be blamed for most ills. This situation is a marked contrast to that which prevails in the West. Harvard, which has so influenced Mr Chidambaram, is in the midst of a major plan to 'overhaul the university'. The plan for change is intended 'to move science center-stage in an institution that, although diverse, has traditionally enjoyed particular pre-eminence in the humanities' (H. Pearson, *Nature*, 2005, **433**, 190). This ability to constantly examine and reexamine institutional functioning and performance is one of the West's greatest assets.

The 2005 Budget signals a renewed public committment to higher education and research in science and engineering. Mr Chidambaram's grant to IISc challenges the institution to raise its level of performance. In a flight of budgetary rhetoric the Finance Minister has raised visions of a new Harvard of the East. Comparisons can tell us how large and unbridgeable the gulf can be and Mr Chidambaram surely knows this. However, raising the bar and raising it high can certainly be a powerful catalyst for improvement.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Chemistry: A Discipline in Decline

Two years ago the biologists celebrated the 50th anniversary of the Watson–Crick paper, announcing the structure of DNA. The birth of molecular genetics might have been viewed as a triumph of physics and chemistry, but the revolution in molecular biology that followed, swept away the bridges to the origins of the double helix. This year the physicists celebrate the centenary of Einstein's remarkable publications in *Annalen der Physik* in 1905. Quantum mechanics and relativity may be subjects far removed from the concerns of common people; nevertheless Einstein, more than anyone else, remains science's enduring icon. John Barrow analyses Einstein's special appeal: 'Einstein restored faith in the unintelligibility of science. Everyone knew that Einstein had done something important in 1905 (and again in 1915) but almost nobody could tell you exactly what it was.' In assessing the impact of Einstein on our perception of science, he concludes: 'Most amazing of all is that – despite the hullabaloo and the inevitable cynicism about celebrity in our age, especially in response to media-created icons – Einstein's scientific legacy is greater than ever.... His early scientific work was an unqualified success and his personal demeanour and response to fame an object lesson to all. That is why 2005 is the World Year of Physics – Einstein's Year' (*Nature*, 2005, **433**, 218). There is a clearly stated hope that pursuing Einstein's legacy, both in relativity and his oft-expressed apprehensions about quantum mechanics may yet lead to a new revolution in physics. There is a great sense of promise and optimism in the future of theoretical physics in the collections of commentaries that mark the Einstein centenary (*Science*, 2005, **307**, 865–890; *Nature*, 2005, **433**, 213–259). With both biology and physics celebrating famous anniversaries and looking at the future, I am inevitably drawn to ask: 'What about chemistry?'

Chemistry has sometimes been called a 'central science'. It straddles the vast terrain between biology and physics and shares borders with geology, metallurgy and medicine, among others. Yet, chemistry has a poor public image. Chemistry is associated with poisons ('strychnine in the soup' is an old favourite) and pollutants. The beneficial uses of pharmaceuticals and plastics fade into insignificance in the public mind, in comparison with the constant barrage of discussion on chemical dangers. Chemistry lacks the romance of biology or physics; the former promises to unlock the secrets of life, while the latter probes both the farthest reaches of the universe and infinitesimally small constituents of matter. There is an undeniable glamour in chasing after a mysterious new 'particle' or in discussing a 'theory of everything'. The technological advances that have flowed from the discoveries of physics and biology have in large measure been accorded a degree of public recognition, that has eluded some of chemistry's most useful contributions. The declining perception of the usefulness of chemistry is best illustrated by the steady closure of chemistry departments in the United Kingdom (UK). A recent estimate suggests that 'less than half of all UK universities offer undergraduate degrees in chemistry' (Clery, D., *Science*, 2005, **307**, 668). Public discussion of the declining fortunes of chemistry has been catalysed by the decision of the University of Exeter to close its chemistry department. The costs of teaching undergraduate chemistry and physics have escalated, with laboratory courses becoming a major drain on resources. Even as students are drawn away to more attractive courses, chemistry and physics departments must wrestle with declining student strengths and falling budgets. The marketplace of university education is becoming more competitive and the UK example may merely be a forerunner of what is to happen worldwide. The UK funding mechanisms, which tie university grants to research output, appear to be hitting hard at teaching departments, leading the report in *Science* to link 'Darwinian funding' to the 'demise of physics and chemistry'.

In India too, the signs of a decline in chemistry and physics have been evident for some time. Biology departments, both botany and zoology, have reinvented themselves as biotechnology departments. Students flock to biotechnology courses in colleges, which charge high fees, in the hope that these degrees will open the door to employment in the biotechnology sector, which is growing at a moderate pace. Physics and chemistry appear to offer little for the future. Unfortunately, many B Sc and even M Sc biotechnology courses have a minimal laboratory component, producing thousands of poorly trained students. The situation with respect to experimental training is also very poor in both undergraduate and postgraduate courses in chemistry and physics.

The decline of chemistry (and physics) departments in Indian universities has been easily noticeable. Several areas of traditional strengths, notably natural products chemistry and inorganic chemistry have withered away. Organic chemistry still appears to be moderately popular, with both students and teachers. As long as faculty retirement rates outstrip recruitment, there is little doubt that the decline will continue. The laboratory, which was once so central to B Sc and M Sc courses, is slowly disappearing; 'computer experiments' may soon be the only exposure that students may have in many areas of chemistry and physics. At present, one of India's strengths is its trained pool of organic and medicinal chemists, who have been critical to the success of the chemical and pharmaceutical industries. Synthetic chemistry is an area where specialized experimental skills may be far more important than theoretical understanding; performance at the bench more prized than articulation. This is also an area which is central to drug discovery and development. As major multinational pharmaceutical companies move towards cutting the costs and hastening the process of drug discovery, outsourcing the chemistry of pharmaceuticals may become commonplace. In the West, chemists will be hard to find; biologists are far more available. India may then be one of the most promising sites for outsourcing (Marris, E., *Nature*, 2005, **433**, 902). Organic and medicinal chemists will undoubtedly be in demand. It would then be important for academic chemistry departments to project these growing avenues for employment and enhance the intake of students.

Even as the fortunes of branches of science wax and wane, it is inevitable that interdisciplinary research is the mainstay of the future. The time has come to seriously consider restructuring the courses in science to permit greater flexibility for undergraduate and postgraduate students. The archaic system of 'main subjects' and 'ancillaries' or 'subsidiaries' may have outlived its use. Employable chemists, biologists and physicists of the future will need a greater appreciation and familiarity with sister disciplines. This is only possible when departmental boundaries are decisively breached. Unfortunately, the state of most of our academic institutions, precludes any serious, enlightened internal debate on academic matters. Reform must necessarily come from within.

In thinking about the state of chemistry, I could not but return to the theme of chemistry as an experimental science. For young students, in high school, there is a certain excitement when chemical experiments are performed. Fire, explosions, dramatic changes of colour, evolution of pungent fumes and the indescribably beautiful process of crystal formation can all fire a young imagination. I was fortunate to have played with a chemistry set as a child (they are exceedingly difficult to find nowadays). The joys of chemistry in a pre-adolescence age are wonderfully described in Oliver Sacks' *Uncle Tungsten. Memories of a Chemical Boyhood* (Vintage Books, New York, 2001). Sacks, a distinguished neurologist and highly acclaimed writer describes his infatuation with chemistry, in compelling style. His joys are simple: *'We made a "volcano" together with ammonium dichromate, setting fire to a pyramid of orange crystals, which then flamed furiously, becoming red-hot, throwing off showers of sparks in all directions, and swelling portentously, like a miniature volcano erupting. Finally, when it had died down, there was, in place of the neat pyramid of crystals, a huge fluffy pile of dark green chromic oxide'* (p. 78). Sacks notes that safety concerns have eliminated most reagents from chemistry sets and school laboratories in the West. He quotes Linus Pauling: *'Just think of the differences today. A young person gets interested in chemistry and is given a chemical set. But it does not contain potassium cyanide. It does not even contain copper sulfate or anything else interesting because all the interesting chemicals are considered dangerous substances. Therefore, these budding young chemists do not have a chance to do anything engrossing with their chemistry sets'*. Pauling and Sacks talk of an age that disappeared a long time ago in the West and barely touched India. In his superbly readable account, Sacks describes with a tinge of sadness the end of his affair with chemistry: *'But it was not sudden – I did not wake up one morning and find that chemistry was dead for me; it was gradual, it stole upon me bit by bit. It happened at first, I think without my even realizing it'*. Sacks' infatuation ends when he senses the inroads of theoretical understanding: *'This new quantum mechanics promised to explain all of chemistry. And although I felt an exuberance at this, I felt a certain threat, too'*. He quotes William Crookes: 'We shall be set free from the need for experiment, knowing, *a priori* what the result of each and every experiment must be'. Sacks is clearly bewildered: *'I was not sure I liked the sound of this. Did this mean that chemists of the future (if they existed) would never actually need to handle a chemical; might never see the colour of vanadium salts, never smell a hydrogen selenide, never admire the form of a crystal; might live in a colourless, scentless, mathematical world'* (pp. 312–313).

Even after two centuries of progress, chemistry is rooted in experiments. There is still considerable joy in creating new substances and in deciphering the sophistication of nature's chemistry. The discipline is also central to development, for new materials necessarily fuel new technologies. Chemistry's fortunes must surely take a turn for the better in the not too distant future.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Women, Science and the X-Chromosome

International Women's Day is celebrated every year on 8th March. The day has been institutionalized by the United Nations, although various dates in March have been previously associated with the struggle for equality of women. The women's rights movement has its origins in the struggle to vote by women in the United States in the latter half of the 19th century, most dramatically highlighted by the 1873 trial of Susan B. Anthony in the United States for illegal voting, Women's Day appears to have firmly rooted beginnings in Europe. March 8 presumably commemorates the famous strike in St. Petersburg organized by Russian women in 1917, a spontaneous uprising for 'bread and peace' in a Russia ravaged by war and ripe for revolution. Today, Women's Day is used to focus attention on the many issues that ensure that women continue to live in an unequal world. This year, I received an invitation to participate in a panel discussion organized by women students at the Indian Institute of Science. At first, I pleaded ignorance of the complexities of the issues involved. But, the student who came to invite me changed my mind with the candidness that sometimes is the defining characteristic of the young. She asked: 'Sir, you have been around this institution for a very long time. Surely, you must know something'. Disarmed by such a compelling argument, I gathered courage to face an audience, which was undoubtedly well prepared to articulate the problems faced by women in science. The discussion touched many familiar but difficult issues: security for working women, discrimination in recruitment, harassment at the workplace, problems of reconciling the demands of families, with the sometimes impossible demands of a research career and the disappearance of highly qualified women scientists from the scientific workforce. Many issues struck a chord. Cannot our institutions be more women-friendly and run high quality child-care centres (creches)? Is there no easy way in which continuity of a research career can be ensured in the years in which young children are to be raised? Is there a 'glass ceiling' which prevents the best of women scientists from rising to the top of their profession? Is there an invisible (or sometimes, barely disguised) men's club, which ensures that the representation of women, in the upper levels of administration and in bodies like academies, is limited? Should there be an 'equal opportunity' movement, which introduces a level of positive discrimination in favour of women? These are familiar issues which have been oft-debated. As is customary, the discussion sparked interest in the issues raised, but I suspect these will remain alive even in future discussions, as the definition of the problems and contours of their solutions still remain on the drawing board.

Gender issues have been discussed widely in India over the last few years. An analysis of women scientists points out rather forcefully that 'women scientists form ... a minor proportion of all working class women'. Using biology as a field for the analysis, Vineeta Bal concludes that 'despite the increase in the number of women scientists in biology, gender-based disadvantages continue to be the order of the day even in academic performance-based competition'. She notes that many women 'lose out in the pursuit of their profession in the early phases. Even for those who cross this hurdle successfully, the profession can make them invisible – not rising far enough to step into leadership roles and not getting recognition. Rumbler strips on the road function as more efficient controllers of speed than a single speed breaker bump, a situation depressingly familiar to women biologists' (*Economic and Political Weekly*, 7 August 2004, pp. 3647–3652). A recent report produced by the Indian National Science Academy (INSA) examines 'Indian women's access to and retention in scientific careers'. The word 'retention' is clearly important. Most M Sc courses and Ph D programs in national institutions have a large proportion of women students. In some areas like biology women can even outnumber men. Inspection of available data on scientists and academic faculty members in diverse institutions, however, reveals that very few women continue to struggle up the scientific ladder. This attrition is undoubtedly a result of the difficulty in pursuing a career that demands an excessive investment of time, in the face of increasing family demands. The INSA report outlines 'steps to reduce stress on women scientists and students and facilitate study and practice of science by women'. Some measures like 'flexible working hours and part-time jobs' and 'age relaxation in recruitment, and 2 mid-career breaks' may increase the number of women scientists, but will do little to enhance the competitiveness of women scientists in an area where peer pressure can be intense. It appears easier to institute welfare measures rather than to level the playing field. The report acknowledges with a tinge of despair: 'Alas – INSA may not be able to change the "patrifocal" (a curious word indeed!) mind-set of society, which has become part of the human psyche'.

Women's issues came to the fore a few weeks before International Women's Day. In a speech delivered on 14

January 2005 at a conference on 'Diversifying the Science and Engineering Workforce', the President of Harvard University, Lawrence Summers made a set of controversial remarks that seemed to imply that innate differences between men and women might account for the lower presence of women in science and engineering. Summers, an economist and a former Treasury Secretary under President Clinton, set the cat among the pigeons with remarks that were soon dubbed as outrageous, leading to a snowballing controversy. *Nature* noted in a cautious editorial that 'what Summers actually said has been harder to discern', since no transcript was available. Following a series of apologies that seemed to satisfy none, the transcript appeared on the Internet. In a long and rambling speech, the offensive sentence suggested 'that in the special case of science and engineering, there are issues of intrinsic aptitude and particularly of the variability of aptitude, and that those considerations are reinforced by what are in fact lesser factors involving socialization and continuing discrimination'. Summers went on to add that he 'would like nothing better than to be proved wrong' suggesting that he was unsure about his 'nature versus nurture' argument. What did Summers imply? The consensus seemed to be that he took the politically incorrect view that women were intrinsically less endowed in practicing the sciences. Summers of course, is no geneticist and his critics quickly went on a rampage. Coincidentally, even as the controversy on the relative abilities of the sexes intensified, the DNA sequence of the human X-chromosome appeared (*Nature*, 2005, **434**, 325).

The X-chromosome, dubbed on *Nature*'s cover as 'the chromosome that unites the sexes' is really the female characteristic; women have two X-chromosomes, one inherited from each parent. In one of biology's magical events, most genes on one of the two X-chromosomes are silenced. Men possess an X and Y pair of chromosomes, with the latter possessing relatively little by way of genetic information, although it does determine 'maleness'. While the X and Y chromosomes evolved from a common ancestor, the latter has 'disintegrated to a shadow of its former self' (Gunter, C., *Nature*, 2005, **434**, 279). To the non-specialist descriptions of the 'male' Y-chromosome convey an impression of a bleak, molecular wasteland, limited to controlling a few defining characteristics of sex. The X-chromosome is more fertile, displaying a genetic richness which surely must yield many new insights in the years to come. Men who harbour critical mutations in the X-chromosome may be more susceptible to X-linked disease; women less so because of their good fortune in inheriting a healthy copy of X from one of the parents. The coin, of course, has another side. There are the inevitable predictions 'that genes on X can benefit males even at the expense of females as the detrimental effects would be masked in females' (*Nature*, 2005, **434**, 279). With 1098 genes identified, the X chromosome presents a formidable challenge to those who wish to make a correlation between genotype and phenotype. The waters are further muddied by interactions between gene products derived from different chromosomes and subtle control of genetic information by differential gene expression. It is unlikely that molecular genetics will provide insights into complex attributes like 'aptitudes for science and engineering'. For answers to the Summers' questions (and these may have been framed in an unwise moment of rhetoric) we must necessarily avoid falling into the trap of blaming nature.

I like to think that what may be loosely termed 'an epidemiological approach' tells us more about the skewed sex ratios as one moves up the totem pole of science and technology. Statistics are available in plenty; worldwide reports, national surveys and even state-wise figures for the presence of women in higher education institutions in India. The data is compelling. Women hold their own successfully, in some fields outdoing men, until the Ph D stage which is reached in the mid-to-late twenties. There is clearly no evidence for any 'intrinsic difference', except where selections may be biased or environments perceived as 'unfriendly'. The decline in numbers is steep at the level of entry into a professional career, with a steady drop-out rate thereafter, till the mid-30s. Women who struggle and stay the course break free of a major barrier imposed by nature and custom, the imperatives of raising a family. There are other hurdles in place, but I suspect that the glass ceiling will crumble, much as the Berlin Wall did many years ago, under the weight of undeniable aspiration.

In optimistically viewing a future that provides a level playing field for women in science (I must here confess that I have invariably been labelled as a pessimist on many other issues), I must draw attention to a book that is compellingly readable, *Nobel Prize Women in Science* by Sharon Bertsch McGrayne (Carol Publishing Group, 1998). The book contains 16 portraits of women, who have made major contributions to science, not all Nobelists. There are three generations; the first includes Marie Curie and Lise Meitner; the second is larger and the names are familiar, Gerty Cori, Barbara McClintock, Dorothy Hodgkin, Chien-Shiung Wu, Rosalind Franklin and Rita Levi-Montalcini among them; the third, small but undoubtedly growing, includes Jocelyn Bell Burnell and Christiane Nüsslein-Volhard. The sketches are inspirational, the hurdles faced clearly stated. Determination and commitment appear to be the key ingredients for success and I suspect this is true for men too.

The book is dedicated to two women whose names were unknown to me, Hilde Proescholdt Mangold and Frieda Robscheit-Robbins. The latter worked for thirty-eight years as the research assistant of George Whipple, who discovered the factor that would 'cure pernicious anemia', work for which he received the Nobel Prize for Medicine in 1934. Whipple was generous in his acknowledgements but Robschiet-Robbins is an unknown figure. But her views on research bear repetition 'You become possessed of a magnificent obsession and determination to learn the truth of your scientific theory if it takes sixteen years or many times sixteen. If you are successful, you really deserve no great credit, for by that time experiment has become the only thing in life you care to do'. This is the sentiment that truly propels science in its forward march, but it is one that can move only the exceptional individual. For the rest of us, the environment must be supporting and encouraging.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Obituaries and Biographies

There is something about obituaries and biographies (even self-serving autobiographies), which makes them eminently readable. Whenever a volume of the *Annual Reviews* crosses my desk, for the purpose of assigning a prospective reviewer, I invariably turn to the autobiographical essay which forms the introductory chapter. The discipline does not seem to matter. The experiences of men and women of accomplishment, from biochemists to astronomers and particle physicists to entomologists, seem always interesting. When a new issue of *Nature* or *Science* appears I can never resist reading an obituary first and these are usually of scientists, who have left rich legacies. Beyond a certain age, newspaper readers often turn to the column that records local deaths, scanning the announcements for familiar names. Even here, the stark three or four line announcements often convey a sense of loss, marking an event of great significance in the lives of otherwise unknown individuals. This column is a sharp reminder of mortality and the fragility and impermanence of life. There is a certain sadness at a premature death; a sense of admiration for a long and well lived life. Obituaries are surprisingly popular amongst readers of newspapers, magazines and journals.

The stimulus for this column was the arrival on my desk of a publication produced by the Geological Society of India. Entitled *Random Harvest* this book is a collection of 'biographical sketches' written by B. P. Radhakrishna, the indefatigable editor of the *Journal of the Geological Society of India*. Radhakrishna has periodically written about the people he knew or of public figures and, of course, eminent earth scientists over a period of two decades. In this collection, there are obituaries and biographical sketches, all eminently readable. The author notes, with becoming grace, of many of his subjects: 'Association with them has enriched my life and not a day passes without my remembering their many acts of love and kindness to me. Most of the persons whose life sketches are included here are no more, but my recollections of their magic presence make the long hours of old age less dreary and burdensome'. Radhakrishna writes with compelling clarity about those whom he knew and of others whom he admired from afar. In reflecting on great personalities, he follows Jane Austen's maxim, which he quotes approvingly: 'Think only of the past as its remembrance gives you pleasure'. This is advice that most of us would do well to follow; selectively erasing unhappy events from our memories.

There have been many famous collections of biographical sketches, one of the most celebrated being Winston Churchill's, *Great Contemporaries*. My own favourite is C. P. Snow's slim volume, *Variety of Men* (Penguin Books, 1969). Snow's prose is soothing and his subjects men of considerable achievement, although in one case, Stalin, the judgement of history may well be very harsh. There are nine sketches in Snow's book; three are of scientists, Rutherford, Hardy and Einstein; there are four men in public life, Churchill, Stalin, Lloyd George and Dag Hammarskjöld and two men of letters, H. G. Wells and Robert Frost. In his preface, Snow notes that he 'met them all except Stalin, though Churchill only at committees, which didn't teach me much'. He states his motives as an author, with disarming simplicity: 'I wrote the book for fun, not because of the grandeur of most of my subjects, though of course that helped. The real fun was in the variety of human beings'. Snow was fortunate to have been in Cambridge in the 1920s and 1930s, in the years that transformed physics. He recounts that 'in 1923 at the meeting of the British Association for Advancement of Science Rutherford announced at the top of his enormous voice: "We are living in the heroic age of physics". He went on saying the same thing loudly and exuberantly, until he died, fourteen years later'. Snow's sketch of Rutherford is an extraordinarily perceptive assessment of one of the most exciting periods in physics; Cambridge, as Snow explains, 'was the metropolis of experimental physics for the entire world'. The men around Rutherford, some of the most famous names in physics appear; Chadwick and his neutron, Blackett and the positron and, of course, Kapitsa. Snow reserves his highest praise for Kapitsa: 'If he hadn't existed the world would have been worse: that is an epitaph that most of us would like and don't deserve'. Some of the most famous stories about Rutherford are recounted here. Two favourites bear repetition. 'You are always at the crest of the wave', someone said to Rutherford. 'Well after all, I made the wave, didn't I?' Rutherford replied. Rutherford once said in a speech: 'As I was standing in the drawing

room at Trinity, a clergyman came in. And I said to him: 'I am Lord Rutherford'. And he said to me: 'I'm the Archbishop of York'. And I don't suppose either of us believed the other'. Snow's assessment of Rutherford's place in science is clear: 'Was Rutherford the greatest experimental scientist since Michael Faraday? Without any doubt. Greater than Faraday? Possibly so'. But it is Snow's judgement of Rutherford as a man that is required reading for aspiring writers: 'He was a great man, a very great man, by any standards which we can apply. He was not subtle: but he was clever as well as creatively gifted, magnanimous (within human limits) as well as hearty. He was also superbly and magnificently vain as well as wise – the combination is commoner than we think when we are young. He enjoyed a life of miraculous success. On the whole he enjoyed his own personality. But I am sure that, even quite late in his life, he felt stabs of sickening insecurity'.

Snow's account of the mathematician G. H. Hardy, known widely as the discoverer of Ramanujan, is an exceptional essay. He calls Hardy's book *A Mathematician's Apology* 'a book of such haunting sadness'. Snow's assessment is beautiful: 'Yes, it is witty and sharp with intellectual high spirits: yes, the crystalline clarity and candour are still there: yes, it is the statement of a creative artist. But it is also, in an understated stoical fashion, a passionate lament for creative powers that used to be and will never come again. I know nothing like it in the language; partly because most people with the literary gift to express such a lament don't come to feel it: it is very rare for a writer to realize, with the finality of truth that he is absolutely finished'.

In a short essay, several years ago, Eugene Garfield considered *The Human Face of Science*. He asked: 'Why do we wait until the death of our colleagues to commemorate the achievements of their lives? Among scientists, the first biographical account is too often the obituary notice'. Garfield argues that for understanding 'the human element in science', autobiographies are important. Often, autobiographical essays provide uncommon insights into personalities and scientific problems; sometimes they are plagued by the problems of selective memory. Nevertheless, as Garfield notes, 'even when written by a well-informed associate, the biography or obituary, being essentially a view from the outside, cannot substitute for the rich personal details and revealing statements found in first-person accounts' (*The Scientist*, 15 December 1986, **1**(3), 9; *Essays of an Information Scientist*, 1991, **14**, 214). My personal preference would still be for the more neutral biographical sketch; autobiographical essays can almost never hope to achieve the penetrative analysis provided by Hardy's *Apology*. Obituaries remain the only published assessment of the work of scientists of moderate distinction. Most will pass on without drawing more than a brief announcement. Curiously, sometime ago, the *British Medical Journal* carried an article entitled, *It's Never Been a Better Time to Die. A Look at the Changing Art of Obituary Writing* (Bullamore, T., *BMJ*, July 2003). The author notes (possibly, tongue in cheek) 'it's not just the dry bones of a life lived' that the obituary enthusiasts want; rather 'a little bit of gossip goes a long way; a light sprinkling of skepticism over what otherwise appears to be a blameless life makes for an interesting read'. Clearly obituary writers are being urged to remember Mark Anthony's famous words: 'The evil that men do lives after them, the good is oft interred with their bones' (*Julius Caesar*).

Reading balanced and well-written obituaries or biographical sketches of men and women of accomplishment can be inspirational; at times it can be educational and revealing. It is always comforting to be reassured that our heroes are also human. In India, good biographical writing is rare; hagiographies are the norm rather than the exception. B. P. Radhakrishna's essays are therefore important both for scientists and writers, who would like to chronicle the lives of men of achievement. His essay on C. V. Raman, written years after Raman's death, is sympathetic, but honest. Raman comes across as an immodest, often intolerant man. 'Humility was not part of his make-up'. He is also portrayed with admiration as a man of boundless energy, a compelling orator, possessing remarkable physical insights and driven by a 'fervent desire to see his country advance in the field of science'. Radhakrishna quotes, approvingly, Raman's rather forlorn lament: 'I thought I would build true science in this country, but all we have is a legion of camp-followers of the West'. There is indeed much to be learned from biographies and obituaries.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Plagiarism: A Spreading Infection

'Critics of science make a lot of mileage out of the manifest discrepancies between the private and public actions of scientists. They also fasten on examples of scientific behaviour that obviously deviate from the norms – fraud, plagiarism, partisan disputes over priority, and so on. These are serious matters of concern, but they are not so widespread and prevalent that they completely corrupt the whole enterprise. Indeed, the fact that such episodes are regarded as both deviant and scandalous is a tribute to the continuing moral authority of the ethos that they flout.'

–John Ziman

Real Science. What it is and what it means

Cambridge University Press, 2000, p. 32

In his sympathetic treatment of the philosophy and ethos of science John Ziman notes that 'plagiarism is as infamous as fabrication in a scientific paper' (p. 40). Plagiarism appears to be on the increase. Several manuscripts submitted to this journal have substantial portions of text which are taken verbatim from published sources, with no attribution or credit assigned to the original authors. Detection of plagiarism is difficult. Hard pressed editors and referees can hardly be expected to act as policemen, preventing violations of commonly accepted ethical practice. Even as I wrestled with a couple of manuscripts that appeared to transgress all acceptable limits, I was pleased to receive a copy of an editorial entitled 'Plagiarism in the age of electronic publishing' (Sota, T., *Population Ecology*, 2004, **46**, 219). Clearly, I was not alone, sinking in editorial distress. The case discussed, involved a review article published in an 'online journal', which appeared to have expropriated large tracts of a paper previously published in *Population Ecology*. The editorial analysis provided interesting statistics; the plagiarized paper used 29% of the original text without modification, while '93% (in terms of words)' were common between the two papers. The editorial noted that 'the prevalence of plagiarism in scientific papers is an acute problem, but tackling it is not easy when considering the rapid expansion of scientific journals supported by the worldwide propagation of the Internet'.

Has the Internet contributed to the rising incidence of plagiarism? Coincidentally, while I was thinking about deviant behaviour in science, *The Hindu* (27 April 2005) reproduced (of course, with permission) an article from *The Guardian* entitled 'Ethics and plagiarism'. The report by John Crace describes the rise of Jude Carroll, who is dubbed as a 'leading authority on plagiarism'. Carroll's research has focussed on plagiarism practised by students who turn in essays and reviews of literature as part of their coursework at high school or university. Copying has become easier, given the power of modern search engines and the volume of digital information readily available on the Internet. *Google* and the download can permit assignments to be completed in record time; a new generation of students is quickly seduced by the ease of electronic plagiarism. Ironically, the search engines have also made the detection of plagiarism a lot easier. However, constant suspicion hardly facilitates any scholarly activity. Journal editors (and those at this journal are no exception) rarely attempt to establish plagiarism before a manuscript is reviewed. Authors are trusted implicitly. Cases of plagiarism are uncovered, usually by accident, long after publication, often leading to prolonged correspondence between the editors and the offending authors. Demands for redressal from the victims of plagiarism must also be addressed. These episodes are unpleasant and consume a great deal of time. In the end, there are no winners. After a couple of difficult experiences, most editors (and I must count myself amongst them) begin to view some submissions with suspicion. My colleagues and I worry about manuscripts in which the linguistic style is uneven; grammatically perfect in part and replete with errors, elsewhere. An odd figure that appears to have a distinctly higher quality than others is another tell tale sign.

Plagiarism is like an infectious disease and can spread rapidly amongst students, if the environment is conducive. The first signs of infection are evident in seminars, where wonderfully prepared slides are displayed (*PowerPoint* is another great facilitator), without any citation of the source. A little investigation usually reveals that most illustrations have been simply downloaded from readily accessible websites. Non-citation of the source is usually an oversight; unfortunately this quickly becomes a habit and eventually intellectual theft becomes a routine practice. Plagiarism

of term papers and assignments has long been rampant in American universities, leading most institutions to circulate detailed definitions of plagiarism to students. A cursory search of the Internet revealed that plagiarism is an issue that has been seriously addressed in the West, with considerable effort expended on educating new students in colleges and universities. So far there appears to have been little effort in India to consciously educate students on ethical issues involved in the practice of science. An exercise to instruct students (and indeed more senior practioners) of science, on proper citation practices and a clear definition of what constitutes plagiarism, may be worthwhile. Such instructions must form part of pre-Ph D training programs. Sadly, in most of our institutions, the Ph D program appears to be private contract between students and research supervisors; academic administrators usually favour a course of 'benign neglect'. Indeed, educating the new generation may be the only vaccine available to stem the spread of the virus of plagiarism.

Plagiarism in India was dramatically highlighted by the Kumaun University affair a couple of years ago (*The physics of plagiarism*, Ramachandran, R., *Frontline*, 26 October–8 November 2002). Deviant behaviour is, of course, a worldwide problem. Some years ago, several plagiarism cases rocked the Chinese scientific establishment (*Science*, 1996, **274**, 337). The issue was alive several years later when plagiarism and unethical practices were discussed at the 2003 meeting of the Chinese Association of Science and Technology. More recently, plagiarism has been established in several papers in the area of materials science, published from Cambridge University (Giles, J., *Nature*, 2004, **427**, 3). Plagiarism appears to be a quick route to scientific success; fabrication of data is another deplorable practice adopted by unscrupulous climbers in science. In his perceptive and scholarly analysis of science John Ziman notes: *'The scientific culture depends fundamentally on personal honesty and mutual trust. For this reason, deliberate plagiarism – the expropriation of genuine research results – is almost as reprehensible as their fabrication. And yet, in spite of peer review and other safeguards, it is relatively easy to get fraudulent research claims into the literature, and to profit from them careerwise for a while. Such cases, when found out, stimulate much institutional turmoil and public comment. The contrast between their condemnation as instances of grave social deviance and the relatively lenient sanctions applied to those who perpetrate them tells us a lot about the internal sociology of research communities.'* (p. 267). As the scientific community has grown worldwide and competitive pressures have increased, the incidence of unethical practices in science has become more common. Today, even students in India who wish to enter Western universities for Ph D degrees attempt to spend summers in research laboratories as apprentices in science. Some years ago, this short stint was viewed as 'research experience' that could be flaunted on an application form. Now, there is pressure to obtain publishable results in a very short period, inexperience notwithstanding. A research publication can quickly smoothen an admission process. The pressure to publish increases as one moves up the academic ladder, presumably easing only at the top. Tenure, promotions, awards, grants, recognition and even self-esteem seem to depend critically on publications. It is this unremitting pressure, coupled with the absence of deterrent punishment for offenders, that has led to the increasing corruption of the scientific enterprise.

Plagiarism and fabrication of results are among the most common and clearly recognized forms of deviant behaviour in science. There is, however, on the horizon a new issue which muddies the distinction between acceptable and unacceptable practice. Many areas of science require the presentation of images. For example, microscope images and gel photographs are staple features of papers in molecular and cell biology. 'Digital photography and image manipulation software allow biologists to tweak their data as never before' proclaims a news feature in *Nature* (2005, **434**, 952). *Photoshop* and competing programs now permit researchers to 'massage their data', blurring the line between 'acceptable enhancements and scientific misconduct'. As the *Nature* report highlights, 'many scientists are oblivious to the consequences of such actions, because they have only a rudimentary knowledge of the sophisticated equipment involved'. Since presentations of data are invariably prepared by students and postdoctoral fellows, who are usually more familiar with the latest software for image manipulation than senior professors, it will become essential to clearly define the limits of acceptable practice.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Patents, Laws and Science

Until a few years ago researchers in academic institutions and national laboratories rarely worried about 'intellectual property (IP) rights' and patents. The pharmaceutical industry, in India, thrived on a patent regime that protected processes, but not products. The drug discovery process is a long tortuous and expensive undertaking, which results in new, clinically useful molecules. Once a valuable molecular structure is uncovered, its production and marketing was legal, under the 1970 Patent Law, as long as an alternate process was developed. Indian industry built its skills in process chemistry and upscaling production, resulting not only in a phenomenal growth in the area of pharmaceuticals, but more importantly in making drugs available at affordable costs. The discovery process requires a great deal of complex, inter-disciplinary basic research; whereas, local strengths in medicinal and organic chemistry and in process development could be readily exploited in a 'process patent' regime, which underplayed the discovery effort. Globalization and the rise of the World Trade Organization (WTO) has changed this scenario. For the last few years the shadow of the Agreement on Trade Related Aspects of Intellectual Property Rights (better known by its curiously apt acronym, TRIPS) has loomed on the horizon. With the WTO serving as an enforcer, India has been nudged firmly towards a new set of patent laws, which now bring into place a product and process patent regime.

When Parliament finally voted in the Patents (Amendment) Bill 2005, towards the end of March, few commentators were unguarded in their praise. Most analysts seemed critical of a new law which seemed to have conceded far more than demanded by international agreements. In a well-argued critique, the important point was highlighted that 'the Patents Bill made 74 amendments to the Patent Act of 1970, thus taking it much beyond the requirement of TRIPS. As the bill now stands, ... it fails to protect the public from the aggressive monopolies that patents confer on the right holders' (Gopakumar, K. M. and Amin, T., *Economic and Political Weekly*, 9 April 2005). I rather liked the use of the term 'aggressive monopoly', which conjures up an image (that often is all too real) of an excessively powerful multi-national, protecting the beneficial products of science beyond all reasonable limits. In the pharmaceutical industry every known trick is employed to extend the patent life of products; science sometimes comes to the aid of the legal departments, most notably in the growing importance of polymorphs. To the uninitiated, a polymorph is merely another physical form of a substance in the crystalline state; its molecular structure and chemistry remain untouched. Polymorphs are patentable, resulting in a new industry, which magically produces polymorphs to extend a legal hold on products (although the same device can sometimes be used by a powerful competitor to muscle in on a large market for a promising drug). In an assessment of the new law a commentary in the *Economic and Political Weekly* concluded that the 'result is a more complicated and confused law', which seemed to be a consequence of not having 'any overall policy objective to achieve, other than pleasing all sides' (16 April 2005). While the US patent system is a model that countries, the world over, seem compelled to follow, the commentary noted that 'there is increasing skepticism around the world over the patent system as it has evolved so far, more particularly in the US'. This critique reaches a damaging conclusion that 'India has swung from one extreme to the other, moving from the 1970 law that was clearly anti-patent to a law that is pro-patent applicant but not necessarily pro-innovation'. It is clear that we now have a 'TRIPS-compliant' law; its effects on drug prices, especially the products of the most recent research, remain to be reliably assessed. There is an almost universally shared perception that the prices of many life-saving drugs will rise, maybe even to levels that make them largely unaffordable in the poorer countries of the world. This is a problem that will eventually have to be addressed by governments and international agencies. Undoubtedly, public subsidy and international aid will be the available weapons of redress; unfortunately, contributing excessively to private profit from the public exchequer is not going to be a strategy that will make everyone comfortable. An editorial in *The Hindu* emphasized that 'the Government must not treat TRIPS as a closed chapter. There is a process of review and India must effectively and persuasively advocate a much greater relaxation of key Articles relating to public health' (24 March 2005).

The culture of patenting scientific research results (I deliberately do not use the word 'innovation' here) is well developed in the United States. Protecting areas of activity with a complex array of patents is a practice that has been made acceptable by multinational corporations. Even research tools and techniques, that would have once passed seamlessly into the public domain, are now protected by a patent firewall. Academic institutions sense that there is money to be made from their research output and rightly feel that a fair share must accrue to the institutions, which provided the ambience and facilities for the research. American universities are doing well as demonstrated by a recent survey. The Universities of California (and there are several institutions lumped together here) top the list with 438 patents in 2003, while Caltech and MIT follow with 139 and 127 patents, respectively. The top 10 contains some well known names, Stanford, Columbia and Cornell. Surprisingly, Harvard, Yale and Princeton were missing; a sign, perhaps, that the Ivy League is a little less affected by the inroads of industry-driven research. The statistics on national rankings make interesting reading. A list of international patent filings under the Patent Cooperation Treaty (PCT) in 2003, shows the United States at the top with 39,250 patents followed by Japan (16,774), Germany (13,979), United Kingdom (6090), France (4273), The Netherlands (4180) and the Republic of Korea (2947). Sweden, Switzerland and Canada make the top ten, while China, India and Brazil are missing (Harding, A., *The Scientist*, 27 September 2004, p. 52). A summary of the fees levied by the US Patent and Trademark Organization (USPTO) suggests that the fees for filing and minimal maintenance would be about $3500, which may act as a deterrent to indiscriminate filing from poorer countries. The patent world and the governmental and legal systems that enforce the rules of the game are clearly dominated by the developed world.

In India, the Council of Scientific and Industrial Research (CSIR) has been the prime mover in pushing for a vigourous program of generating and protecting 'intellectual property', a term that leaves me with an uncomfortable feeling; 'exploitable research results' may be a more appropriate descriptor. The CSIR has successfully propagated the philosophy of protecting potentially useful research and holds the largest patent portfolio amongst Indian organizations. An article in this issue (Shukla, D. B., p. 1553) provides interesting statistics, which suggest that our academic institutions (IITs and IISc prominent among them) are slowly realizing the importance of being alert to the possibility of practically exploiting interesting research results. The absence of a professionally run 'IP cell', in most institutions, can be inhibitory to spreading the 'culture of intellectual property protection in research and development'. Filing patents can also be an expensive and slow process, involving documentation that can drive many creative scientists to distraction. Most institutions do not readily have access to patent attorneys, who can smoothen the process of IP protection. Eventually, a substantial patent portfolio can be expensive to maintain, especially if patent licensing does not lead to significant income. Even as scientists get used to the idea that patents may count as much as publications in assessments of research output, there is a danger that frivolous patenting may become a reality that our institutions may have to confront.

Universities and public institutions can benefit from a spread of the culture of patenting research outcomes, if they become partners with industry. A share of the profits from a discovery that has led to a marketable product is something that most academic administrators would welcome. In the United States, legislation has ensured that 'government renounced intellectual property claims on research supported by federal funds in universities or other non-government institutions' (Kennedy, D., *Science*, 2005, **307**, 1375). In an editorial marking the 25th anniversary of the Bayh–Dole Act, the editor of *Science*, Donald Kennedy notes that 'this legislation has had a profound impact on science in the United States and, indirectly, in other nations as well'. While the Bayh–Dole Act opened the way for universities to profit from publicly funded research and speeded up the process of innovation, there is some concern, a quarter of a century later, that 'corporatization of universities' may entail an unaffordable academic cost. A recent book *University Inc: The Corporate Corruption of Higher Education* (Washburn, J., *Basic Books*, 2005) addresses the issues raised by university–industry links. I have not obtained a copy of the book, but a review suggests that the book must be required reading for analysts and policy makers. The concluding sentence is provocative: 'For the world outside the United States – particularly southeast Asia, where research is more related to health and wealth creation – there are lessons to be learned... The financial imperatives need to be kept in check to avoid serious damage, not just to science but to people's lives' (Richards, G., *Nature*, 2005, **434**, 824).

In India many problems related to patenting activity in public institutions remain unaddressed. There is no legislation that governs the role of governmental funding agencies in the ownership of 'intellectual property', generated in research sponsored at academic institutions. The multiplicity of funding agencies, which do not coordinate their activities, can lead to a great deal of confusion. The situation in projects which involve multiple partners, government agencies, academic institutions and industry can be bewildering. There is a clear need to learn some lessons from the Bayh–Dole Act. Government must act as a facilitator of technology transfer from universities, while ensuring that public funds are invested in areas that may lead to notable progress in science or in fields that have the greatest possibility of benefiting the country.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Winds of Change

Despite advancing age, I have never been able to give up the habit of reading the sports pages, immediately after picking up the morning newspaper. Television, with its innumerable sports channels, has only widened my interest in sporting activities, that were unheard of in my youth. Over the last few weeks I can sense the winds of change blowing over the world of sport. The men's final of the French Open tennis tournament, which I watched even as the deadline for this column slipped away, saw two new stars battling it out for the title. Rafael Nadal, fresh faced and boundlessly enthusiastic seemed to represent a new resurgence of tennis. Formula One racing, which has now acquired new meaning in India, thanks to Narain Karthikeyan, has bounced back from the seemingly monotonous and predictable string of victories by Michael Schumacher, last year. Suddenly, the reigning champion is struggling; a new king is on the horizon. In football, a star studded Real Madrid, boasting of a line-up that includes Beckham, Ronaldo and Zidane, is not even in the running for the UEFA Championship. It is only in our country's favourite sport, cricket, that change seems minimal and slow. Teams and players remain largely static; only coaches seem to make the news. Curiously, the hiring of a foreign coach appears to be the main strategy of the Indian team in preparing for the 2007 World Cup. There is an enveloping sense of tiredness, an absence of fresh and exciting prospects, as the Indian team looks at the two years ahead. However, change is not limited to the sporting arena. Economic liberalization has dramatically unshackled the economy, with many sectors expanding in an unprecedented manner. The IT boom and the prospects of a new surge in biotechnology have raised expectations of young professionals, to limits that were completely unknown. Even Ph D scientists, normally a group that struggles to find employment in industry, seem to be wanted. Multinational companies now offer salaries that scientists never dreamed of; an indicator that the winds of change are blowing stronger than ever. In considering change, I am of course constrained in this column to eschew a more elaborate analysis of sports and business and to return to science.

Are the winds of change sweeping over the world of science? A cursory scan of the literature of science over the last few years will confirm that the international scene in science is already in the throes of a major transition. Exactly a decade ago a provocatively titled analysis highlighted the 'Lost Science in the Third World' (Gibbs, W. W., *Scientific American*, August 1995, p. 76). This well-documented study drew an uncomfortable conclusion: 'Many researchers in the developing world feel trapped in a vicious circle of neglect and – some say – prejudice by publishing barriers they claim doom good science to oblivion'. The poor status of Third World journals, prejudice in the editorial procedures of First World journals, poor communication technology and the absence of worldwide interest in many problems studied by developing country scientists were cited as factors, that seemed to conspire to consign Third World science to a world of neglect and irrelevance. China, India, Brazil and South Korea were among the countries which appeared to produce a lot of science, without any impact on the international scene. Ten years later, the pressures of international competition are even stronger; the global economy, powered by a new set of rules, demands that countries, especially the poorer ones, must raise their level of scientific and technological innovation. Almost imperceptibly, research and development has moved to the central position in any national strategy for economic progress. The responses of the most populous countries of the Third World, China and India, are now monitored closely in repeated analyses of science indicators.

China and South Korea have responded dramatically to the demand for change. As early as 1997, an analysis by the Institute for Scientific Information (ISI) revealed a doubling of Chinese publication output over the period 1989–1996 (*Nature*, 1997, **389**, 113). The Chinese output has increased substantially since then; although a citation-based analysis is less favourable (King, D. A., *Nature*, 2004, **430**, 311). The Chinese thrust has been very visible in some areas, most notably in biotechnology. The creation of large new institutes, initiation of programs to involve expatriate scientists and participation in international ventures like the human genome sequencing project have been measures that have signalled China's clear intention of competing as vigorously as possible in international science, despite an enormous starting handicap. The change of policy has

been backed by substantial state investment in the education and science and technology sectors. A recent report on health biotechnology describes the changes in China as the 'reawakening of a giant' (Zhen-Zhen, L. *et al.*, *Nature Biotechnology*, December 2004). The spurt of scientific activity in China and Korea has inspired an intriguingly titled paper: 'Are the contributions of China and Korea upsetting the world system of science?' (Leydesdorff, L. and Zhou, P., *Scientometrics*, 2005, **63**, 617). The conclusions are worth reproducing: '...The size of the increasing Chinese contribution makes this disturbance of the world system of science historically unprecedented. The exponential growth rates indicate a self-reinforcing growth pattern which is possible because of a virtually unlimited reservoir of human resources with scientific competences that seem to flow into the world system with increasing speed'. China and Korea are suddenly a scientometrician's delight. Their sudden surge of productivity make projections for the future very interesting. Will the balance of science tilt eastward? In reflecting on this question, I found an interesting perspective in an interview with the outgoing president of the US National Academy of Sciences, Bruce Alberts. In talking of the rise of science in Asia Alberts says: 'It seems likely that China or India will become the dominant scientific power. They take science and technology seriously, their young people are hungry to learn it and they have such large numbers of people. But as we all know, there are many ways to make a mess of it. My favourite example is the recent science strike in France. They want more resources for science which is good. But at the same time, you'd hope that they could adjust their system to make it more merit based. Now, after your Ph D, the first job gives you lifetime tenure. That's nuts. That's the perfect way not to run a scientific system. So I think the countries that will lead the world in science and technology are not just those with the most people. That's important. But you also need a system that allows the most talented people to have access to what they need to function effectively. Encourage the collision of ideas and reward risk-taking and innovation' (*Science*, 2005, **308**, 1108).

Does science in India feel even the hint of a gentle breeze that may be the harbinger of the winds of change? For the past several years there have been signs of a heightened awareness of the pivotal role of scientific research and technology development as key elements of a competitive economy. R&D investments in the pharmaceutical sector have risen and major research centres of multinational companies have come up in India; a sign that trained manpower might be lured away from the glamour of overseas opportunities. Change has been slower in the government sector, although investments in science have been steadily rising over the past few years. Governments of different political persuasions have reaffirmed their commitment to enhance efforts in science and technology. There are perceptible signs of change, prompting an upbeat assessment that the brain drain may even be reversible – 'There is a silent scientific repatriation taking place in India' (Mashelkar, R. A., *Science*, 2005, **307**, 1415). I am, of course, much less optimistic. Declining scientific productivity in scientific institutions and university departments does not augur well for the future if strong corrective steps are not taken. Shoring up the base of academic science is the prime motive for two recent governmental initiatives, which have been discussed in the press. The first is the move to establish an autonomous National Science and Engineering Research Foundation (NSERF), with an estimated budget of Rs 1000 crores and a mandate to rejuvenate the academic science system in India. While critics have argued that the money might be better spent elsewhere, an assessment of existing S&T expenditure suggests that this would constitute a modest, but significant, transfusion of resources into this sector. If private participation in research were enhanced the effects could be appreciable. The second initiative is the move to establish two National Institutes for Scientific Education and Research (NISER) in Pune and Kolkata, which will begin a new experiment in integrating high quality undergraduate education in science, within the ambience of a research university. Here again critics have asked: 'Should not the resources for these institutions be channelled instead into our old and venerable universities?' This is a legitimate question, but I suspect the return on investment, in academic terms, is usually far greater in newly created institutions. Dedicated science universities may also be able to move faster in many rapidly developing areas. These new initiatives should not in any way detract from efforts to repair and reconstruct existing academic institutions. Eventually, the success of such experiments will be determined by the management structures that are evolved and the atmosphere that is created by the first generation of administrators and academicians. Can the country afford such experiments? My, admittedly prejudiced, view would be that India is at a stage, where several such experiments are necessary if we are to keep up with global trends in science. Investments alone are not enough. The new institutions must be carefully nurtured and the tendency to decline, after an initial growth phase, will have to be firmly arrested. I can't do better than quote Albert's pithy statement: 'But as we all know, there are many ways to make a mess of it.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Marking a Transition

This will be my last regular appearance in this column. Over the last few years, writing this fortnightly essay has consumed me as no other activity has. I must admit, I have never enjoyed any other activity as much. Readers of this journal have generally been an encouraging and forgiving audience, who will, I am sure, bear with me if this last editorial is largely personal. In 1988, Sivaraj Ramaseshan appeared in my laboratory, unannounced and really unknown to the young students, who surround me. He had retired a few years earlier as the Director of the Indian Institute of Science (IISc). I knew him reasonably well then, although as a relatively junior faculty member age and position were natural barriers. Ramseshan was, as always, charmingly persuasive. He had a new mission; the task of rejuvenating *Current Science*. The journal had fallen on bad times, with diminishing numbers of manuscripts received and accepted; each issue becoming slimmer in size. He needed help and had arrived in person to recruit me. I had (and still have) no idea how Ramaseshan felt that my assistance would be useful; his presence was enough to draw me into a venture to which I have devoted a fair part of my energies over the last seventeen years. Ramaseshan was an enthusiast in the area of scientific publishing in India, among many others. He brought to this journal a new sense of mission. His goals were simple. An interdisciplinary Indian science journal must be widely read in India. It should publish the best of science carried out in this country. It should have a wide reach, penetrating the far corners of our country, drawing readers and authors, who represented the wonderful diversity of India. The journal should offer a forum for discussion of issues that concern science and scientists; providing a level playing field, where opinions could be expressed both by established authorities and unknown readers. These were goals, I could readily identify with. Ramasehan's offer allowed me to fulfil an adolescent ambition of becoming a journalist; here was an opportunity to work and eventually write for a journal, where the management doubled as the editorial staff. For a while, I helped Ramaseshan anonymously, until he reconstituted the Editorial Committee in 1990. Like all committees the mundane burdens of carrying the routine work forward were limited to very few members and I was quickly drawn in to much closer association with Ramaseshan. As he grew older the inevitable problems of age began to intrude, compelling me to slowly usurp his duties at this journal. In 1995, I joined him as an Editor at a time when he was decidedly becoming frailer. In looking back on the decade that has sped by so quickly, I am struck by the reassurance that Ramaseshan provided me by his mere presence, as the journal grew in size and entered a new era, where discussion of contentious issues could be freely entertained. Ramaseshan had begun the process of liberalization in the early 1990s, opening the pages to a free-ranging discussion of the National Science University, which had then been proposed as a non-resident Indian initiative. Ironically, over 13 years later the government is now in the process of establishing two National Institutes devoted to science teaching and research, an experiment which may revive the concept of research universities.

For several years, this journal did not have a regular editorial column. Ramaseshan and I did write on occasion and looking back I found only 7 columns in the three years between 1995 and 1997. At an editorial board meeting held in July 1998, a senior member suggested that any good interdisciplinary science journal (presumably he had *Nature* and *Science* in mind) must have a regular editorial; the unstated assumption was that editors must have opinions and must not be afraid to express them. It was this hint of challenge that was provocative. A few days later I penned the first of many columns, entitled 'Funding basic science' (*Current Science*, 1998, **75**, 77). In rereading my initial attempt, I am struck by how frequently I have repeated this refrain over the years. Written in the background of the euphoria over Pokhran, the editorial emphasizes a favourite theme: 'Increased budgets for "strategic research", the absence of any new, major initiative for academic science and complete identification in the media and in political circles, of science with the activities of the defence, atomic energy and space establishments do not augur well for the future of basic science in universities and research institutions'. Much water has flown under the bridge since then; public funding for science has steadily increased. Since that first essay which appeared on 25 July 1998, there have been 166 editorials that have followed and it has fallen to my lot to author all except

one. The fortnightly cycle has been trying at times, although deadlines have somehow always induced a surge of adrenalin. The column has had many faithful readers and it is the critics amongst them, who have always provided the most important insights on issues that I treated inadequately.

The most difficult part has been to find a topic each fortnight; the clock always seems to tick more rapidly than it should. In trying to avoid being repetitive I have ranged far and wide, at times writing on topics in which I have little or no expertise. Books and book reviews have always been inspirational. Writing about them has been very fruitful; kind and generous readers sometimes sending me copies of books they feel I should read. Committee meetings, often indescribably dull, can sometimes throw up a marvelously provocative phrase and I have tried to be alert to every opportunity. The canteen, where my colleagues argue about the state of science has provided both sustenance and stimulation. It is here that I have heard of the demise of disciplines, chemistry, physics and even genomics amongst others. At times, an editorial column has appeared as an aftermath. Controversies are always welcome, when editorial deadlines are impending and I have discussed my share, ranging from inappropriate awards to missing generations of leadership in Indian science. Another favourite has been the rise of the culture of citation counting and the insidious stranglehold of the impact factor. Writing about science and scientists has been particularly fulfilling. An easily recognizable failing is that I have over-indulged in the practice of using quotations, poetry being a special favourite. Critics have pointed out that at times the columns appear to have been written by a school teacher, intent on completing an English lesson; a charge that does appear to be legitimate. This column has also, albeit infrequently, been used to mark the passing of men I knew; men who greatly influenced the environment, where I have worked all these years. In writing about Satish Dhawan, G. N. Ramachandran, Sivaraj Ramaseshan and Hosur Narasimhaiah, I have been sharply reminded of their commitment, dedication and influence. Reflecting on their lives, I have shed a tear, in the solitude of the night, sometimes from a sense of personal loss, at others from the realization that even the most admirable men are only mortal. When one chronicles greatness, Thomas Gray's sombre line resounds: *The paths of glory lead but to the grave*.

Many of my columns have dealt with problems of practicing science in India. They have, at times, been critical of government agencies, institutions and procedures. I have been elitist on occasion and populist at other times. Contradictions and inconsistencies will be obvious but these are the inevitable corollaries of excessive writing. In thinking about writers as conscience keepers and articulators of dissent, I was drawn to the description of an orator by Winston Churchill: 'An orator is the embodiment of the passions of the multitude. Before he can inspire them with any emotion he must be swayed by it himself. When he would rouse their indignation his heart is filled with anger. Before he can move their tears his own must flow. To convince them he must himself believe. His opinions may change as their impressions fade, but every orator means what he says at the moment he says it. He may be often inconsistent. He is never consciously insincere'. I believe whenever I have written on emotive issues, the description of an orator might also fit the writer. In writing publicly on issues of science in India I realize that I have tried to acquire a position that can sometimes be comfortable; an ability to influence without responsibility. This is a natural attribute of the press, whose desire for power was devastatingly described by Stanley Baldwin (who borrowed his turn of phrase from Rudyard Kipling): 'Power without responsibility is the prerogative of the harlot through the ages'. Readers who have a weakness for invective might do well to read an elegantly written essay in *Frontline* (17 June 2005, p. 82), which begins with an unforgettable quote:

You taught me language and my profit on't
Is, I know how to curse
— *The Tempest*, William Shakespeare

Over the past few years, I have dangerously skirted many potential conflicts of interest. When I have criticized public organizations and government departments, their response has been benignly indifferent. Having recently accepted the position of Director of the Indian Institute of Science, I am even more conscious of this issue. Handling conflicts of interest is not easy; any misjudgement can lead to public embarrassment. It is this issue, together with the tiredness that creeps in when a hobby turns into a chore, that prompts me to take a break. The column will, of course, continue as fresh contributors fill the breach.

In my years as a writer I have never publicly acknowledged those who have helped me. M. S. Venugopal and Chandrika Ramesh have accepted handwritten scripts, delivered at the last possible minute, and, in a feat of alchemy, have transformed them into perfect page proofs in a few hours. Errors have always been mine. In the early years Ayan Guha, a Ph D student in physics at IISc drew cartoons to accompany the text. When we met, invariably at the coffee shop, we would discuss a possible topic and then part ways; he to draw a cartoon, while I penned a column. When he left, as all our students seem to do, for the United States, this journal lost a rare talent. When I began, most of my background work was done in the library; *Google* had not yet intruded. Age and competing demands have taken their toll as I am able to do little aimless browsing amongst the stacks. In the last few years I have had the luxury of a young colleague Riki Krishnan, who by scouring libraries and the Internet produces more material than I can possibly read. He has urged me on, constantly suggesting topics, with an enthusiasm that has been truly infectious. Finally, a word of thanks to those who make writing a pleasure, the many responsive readers of this journal.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Researching the Past

Book lovers held up in airports, by the customary delays in flights, face the hazard of being seduced by the attractive displays of new arrivals in airport bookshops. On a recent excursion I was compelled to browse, ending up inevitably with a book, that I would almost never finish reading in a reasonable length of time. I am now the proud possessor of a hardbound copy of William Dalrymple's *The Last Moghul*. So far I have read reviews of the book, some criticism by the more scholarly and traditional historians and noted with satisfaction that a recent year-end feature placed it on a list of books that the Prime Minister found worthy of mention. I was also drawn to the book because one of its major themes was a description of Delhi in 1857. Some years ago I had been castigated by a perceptive and critical reader of *Current Science* for alluding, incorrectly, to the events of 1857 as the 'Sepoy Mutiny', a phrase picked up from books by British historians. The critic pointed out that a more acceptable description would be a 'War of Independence'. Dalrymple's book seemed to present a new view of those momentous days. I must confess that I have not gone past the Introduction, although I have read a few pages at random. Finding time to read has become difficult. But, there is enough in the first few pages of Dalrymple's book to provide a basis for reflection. Can yet another book be written on the events of 1857? The answer is undoubtedly, yes, especially if new documentary material is uncovered. This is precisely what Dalrymple has done, discovering hitherto unread material in repositories from Lahore to Rangoon and, of course, most importantly in the National Archives in Delhi. Dalrymple notes that in 1923 Vincent Smith complained 'that the story has been chronicled from one side only'. He then adds: 'Yet all this time in the National Archives there existed as detailed a documentation of the four months of the Uprising in Delhi as can exist for any Indian city at any period of history – great unwieldy mountains of chits, pleas, orders, complaints, receipts, rolls of attendance and lists of casualties, predictions of victory and promises of loyalty, notes from spies of dubious reliability and letters from eloping lovers – and all neatly bound in string and boxed up in the cool, hushed air-conditioned vaults of the Indian National Archives'.

Why have I begun this column with Dalrymple and his book, surely a topic as far removed as can be from the concerns of readers of this journal? The answer lies in the nature of the two tasks that I have been confronted with in recent months; tasks that would be well accomplished if I had the benefit of a carefully catalogued written record. The first is the job of writing about the early years of this journal, which is now in its 75th year of publication. The second is the formidable job of building a permanent archive at the Indian Institute of Science, which is rapidly approaching its Centenary Year. In both tasks I have been astounded by the absence of catalogued records in the organizations themselves; a clear sign that an historical record is not a matter of grave importance.

The careful maintenance of a written record and the building up of archives and repositories of documents is, undoubtedly, a Western practice. Oral history is more popular in India, with every story embellished in the retelling. Organizations which retain every file in dusty and disorganized disarray, usually discard them by the truckload in periodic cleaning operations. There is no resident archivist, who sifts through the piles of paper looking for the bits that may help a future chronicler to piece together an authentic and interesting story. I felt the absence of a written record, files of correspondence and photographs, most acutely when confronted with the self-imposed task of writing on the early years of *Current Science*. It seemed appropriate to mark the anniversary by tracing the roots of the journal and discovering the personal stories of its founders. To the hard-nosed reader of a scientific journal, pragmatic and focused, history will undoubtedly be of little more than passing interest. But, as the journal's present custodian, it did seem important to look back at the extraordinary achievements of my predecessors, especially those who midwifed *Current Science* into existence.

Starting the journal must have been a formidable task; sustaining it in the early years even more demanding. The need for a journal, interdisciplinary in content and modelled on the great scientific periodicals of the time, *Nature*, *Science* and *Die Naturwissenschaften* would have been easy to articulate. Indeed, the available record shows that an Indian Science journal was conceived in discussions at the Science Congress and its birth hastened by the positive responses received to a questionnaire, circulated in 1931 by Martin Forster, then Director of the Indian Institute of Science (IISc), Bangalore. I would have loved to see this questionnaire and read some of the responses, for it is really these little bits of correspondence that provide a true glimpse of the times. Unfortunately, no records are traceable. The daunting task of producing, delivering and nurturing the journal was left to a very small band of intrepid editors operating out of Central College and IISc in Bangalore. Seventy five years later I can only imagine the difficulties they would have encountered. Resources would undoubtedly have been meagre and the logistics of producing a monthly, devoted to science, forbidding. But, I suspect in the heady days of the early 1930s science in India was riding the crest of the wave. Raman had

just received his Nobel prize. The renaissance was in full bloom in Calcutta. From Dhaka to Lahore, science seemed to be emerging as a force that would envelop the Universities across the country. In Bangalore, IISc was entering its third decade, clearly developing into a major centre of research. Living in these times, authors and supporters would have been enthusiastic and energetic in promoting the new venture. The journal counted amongst its strongest supporters many young and dynamic men of science, who in later years would build many of the institutions which are so important in India today. Among them were J. C. Ghosh, Birbal Sahni, S. S. Bhatnagar in addition to Saha, Bose and Raman. However, as anyone who has started or run a journal must know, the hard work is done by a few footsoldiers, who fight the daily battles of regular production.

So my search for the journal's roots began by trying to piece together a profile of the first editor, C. R. Narayan Rao (1882–1960). There are no records of the files he must have created at the journal office. Even the precise location of *Current Science*'s first home is not precisely identifiable. It must have been on the campus of the IISc which, in a touch of delicious irony, I happen to head today. Unlike Dalrymple, I did not stumble on any archival treasure. Searching for long lost files, within institutions disinterested in history, seemed an unproductive approach. Instead it seemed more pragmatic to search for the families and students of my predecessors, in the hope that some long forgotten documents would be found, surviving over the decades in the dark and untouched recesses of a family cupboard. The task of tracking down Narayan Rao's descendants required the energy, enthusiasm and unbridled optimism of youth; qualities that I lost a long time ago. Fortunately, I was able to recruit a young colleague at the journal, Riki Krishnan, to the undoubtedly stimulating task of unearthing papers which would provide us an insight into the early days of *Current Science*. We were successful in locating Narayan Rao's son, C. R. Ramachandra Rao, now in his eighties, living in the middle class locality of Jayanagar in Bangalore. Even as we moved across the throbbing heart of the city to talk to him, I could only speculate on how peaceful Bangalore must have been in the 1930s. Central College's historic campus, where Narayan Rao worked and where Raman first lectured on his effect, must have been a far cry from its gridlocked present. We were greeted with the utmost graciousness and old-world charm by Narayan Rao's son, who told us of his father, of *Current Science* and the formidable, and at times difficult, figure of Raman. Fathers can leave an indelible impression on their sons. As we sat, silent listeners sipping the traditional coffee, I could see that the old gentleman in front of me was a child again, recalling events that must have been long buried in the recesses of the mind. I have heard many lectures, seminars and talks, but rarely have I been as captivated by an account of a personal history. He spoke of his father as a writer and scholar, a man who befriended *Nature*'s editor, Richard Gregory. As one who has read almost all of the early editorials in *Current Science*, written in the 1930s, it became clear to me that even though we were only recording oral history, we had indeed discovered the author of many of those extraordinarily perceptive unsigned columns. We were given an unexpected and wonderful gift; well-preserved letters received by Narayan Rao from a host of correspondents in the 1930s. It was in this treasure that we found correspondence with Richard Gregory, spanning a period of over two decades. We came away with the feeling that one task had been reasonably well accomplished; we now knew something about the man who laid the foundations, so firmly, for this journal. Narayan Rao's remarkable scholarship and commitment was evident in all that we heard and read.

The starting of an interdisciplinary science journal in the 1930s must have contributed substantially towards the creation of a community spirit amongst scientists in India, at a time when the stirrings of modern science were just beginning to be felt in this country. Ironically, a vision for an 'Indian Academy of Science' articulated elegantly in an editorial in this journal in 1933, quickly became a focus for controversy. Unification and fragmentation have been recurring themes in our history; scientific activity has not been an exception. Nevertheless, in the 1930s and 40s this journal was the major vehicle of communication between scientists in far-flung parts of India. The early band of editors, Narayan Rao in particular, struggled with finances. It is a tribute to the community of the times that the journal survived the most difficult periods of its history, without a break in production.

In attempting to research this journal's past, I was struck by how much we have disregarded the task of maintaining archival records. Even where they are available, their existence is little known and shrouded by an enveloping disinterest. Bangalore's historic institutions of science will soon mark important anniversaries. The IISc will reach a century in 2009 and coincidentally the Indian Academy of Sciences will celebrate its 75th year. These institutions and *Current Science* have had an historic association. In the 1930s the linkages would have been very close. Little remains of this record. In searching, admittedly in a cursory fashion, for archival material, I have been forcefully reminded that matters of the moment are more important; digging into the past is hardly a productive activity.

In the many years that I have heard accounts of the past, the early years of science in Bangalore, the dominant figure that emerged was always C. V. Raman. Larger than life, the lustre of his achievement undimmed over the years, Raman seemed to be the force behind this journal from its inception. His long, formal innings as President of the Working Committee of the Current Science Association from 1948 to 1970, ensured that the journal bore his stamp over the decades. In attempting to return to our origins, in marking 75 years of this journal's existence, we came upon the work of the first editor, C. R. Narayan Rao. It is to him and his successor, M. Sreenivasaya that this journal owes a great deal for their selfless stewardship in the difficult early years.

In attempting to conclude this essay I turned again to Dalrymple, asking the question: 'How does a historian come to the end of a long story'. In ending his tale of the turbulent events of 1857 and the last years of Bahadur Shah Zafar, Dalrymple takes shelter in a familiar quote, attributed at times to Edmund Burke and at other times to George Santayana: '... those who fail to learn from history are always destined to repeat it'. This ending seemed inappropriate for the minor research into the past that I have described. But then I was reminded of an oft forgotten dictum that must worry historians: '... time blots out small merit, while fattening big glory'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Probing Misconduct: Treading a Dangerous Path

There is nothing more debilitating in a journal editor's life than to be involved in a discussion of a case of scientific misconduct. The issues involved are often contentious, unpleasant and difficult to understand, with the ever-present danger of being accused of bias and malice by all the parties involved. Discussions of the ethics of practising science often transmute into debates on the ethics of journal editors, who make the decisions on publishing or refusing material pertaining to a specific case. Often, and this journal is no exception, the editor is also an active researcher raising the bogey of motivated judgement and conflicts of interest. It is therefore with a considerable degree of misgiving, that I chose to write this column, introducing to readers the case of alleged misconduct at the National Centre for Cell Science (NCCS), Pune, which is considered at some length in this issue. The discerning reader will note that I have used the qualifier 'alleged', because both law and common sense dictate that anyone accused of an offence is innocent until proven guilty. In cases of data manipulation and fabrication, guilt is sometimes not easy to establish. Even more importantly, in the increasingly complex world of modern science, in multi-author papers the individual perceptions on responsibility for specific pieces of data can differ.

The NCCS case, like most such problems, begins with an anonymous e-mail to the head of the institution. One of the charges is that a Figure published in a paper in 2005 (Rangaswami *et al.*, *J. Biol. Chem.*, **280**, 19381) is a reproduction of a Figure published in 2004 (Rangaswami *et al.*, *J. Biol. Chem.*, **279**, 38921), with only a change of labelling. Simply put, this is alleged to be an example of fabrication of non-existent data. The Figures represent 'Western blots', a favourite of cell biologists studying signal transduction, an area mired in biochemical complexity. With the mounting pressure on journals to look attractive, gel photographs (of all varieties) are cleaned and dressed up in many ways, using many different versions of 'image enhancement' software. To an outsider to the field, one blot looks very much like another, with only the legends to figures permitting ready identification. Mislabelling, both intentional and unintentional, can happen. Modern digital technology which permits such facile image storage and manipulation, even by beginning students, also provides the tools to detect 'photo forgeries'. It is precisely such analyses which have been used to address the issue of whether the figures in the two papers from NCCS are identical or not. Matching signatures or fingerprints, in more primitive times, required experts who had learnt to recognize subtle clues in the data placed before them. In the case of the NCCS Western blots, it is a computerized analysis of images that constitutes the basis on which to conclude whether or not an inappropriate act has been committed. At first glance, the problem appears simple. Feed in the images, let the analysis software loose, examine the results and pronounce judgement.

Unfortunately, in the NCCS case there are two conflicting analyses, both of which are described in this issue. The first, conducted by an officially appointed committee chaired by G. Padmanaban, including several active researchers drawn from across the country, comes to the conclusion that the Figures are different and that there is no basis for the allegation of misconduct. The second, initiated by Sohan Modak, was conducted by an independent body, the Society for Scientific Values (SSV), based in Delhi. The SSV, which projects itself as a watchdog of scientific integrity, comes to an unambiguous conclusion that the Figures are deliberately manipulated. Both groups employ image analysis techniques; the former arguing that their conclusions are also based on access to original data, notebooks and interviews with all authors. The waters are further muddied by an independent investigation by the *Journal of Biological Chemistry*, which then proceeded to unilaterally withdraw the 2005 paper. In this case the details of data analysis are unavailable. Finally, there is the complicating factor of an 'internal review' which established a prime facie case, resulting in an attempt by the corresponding author to withdraw the paper under duress. In deciding to publish all the views on this affair, this journal has followed a course that was taken some years ago (*Curr. Sci.*, 2001, **81**, 1389), in which all parties have been given an opportunity to be heard. The authors have been gracious enough to permit a degree of editorial moderation, although it has been difficult to temper the language in all cases. For accusers, there is a great tendency to adopt a strident and judgemental tone; clothed, as they are, in the impregnable armour of self-righteousness. Whistleblowers in India are usually anonymous; their anonymity, presumably, a defence against vindictive institutions and managements. In the NCCS case the charges were publicized, investigated and 'guilty' judgements pronounced by a private body, the SSV. With both libel and privacy laws being largely non-functional in

India, the SSV has been able to take the questionable step of circulating by e-mail and advertising on its website the contents of their findings to large groups of scientists. On the other side, for the defenders there is the tempting option of tarnishing the image of the accusers; malicious intent to destroy institutional and individual reputations is easy to allege, and is sometimes true. In the heat and dust of accusation and counter-accusation, the original problem recedes into the background and a new charge of institutional complicity in a cover-up emerges. In the NCCS case the focus has shifted; the accusers, represented in the published correspondence by Modak, challenging the competence and at times, by implication, the intentions of the Padmanaban committee.

A feature of most discussions on misconduct in India is the pervasive view that there is a malignant 'Indian scientocracy', which seeks to influence all investigations of fraud. ('Scientocracy' is a curious word which could arise by a fusion of 'scientist' with 'aristocracy' or alternatively, with 'bureaucracy'. The former conjures up a vision of a decadent upper class with deteriorating moral values, while the latter invokes an image of a stonewall, defending wrongdoers). The SSV and its proponents therefore argue that an empowered, privately constituted group of 'vigilantes' would be the best way to raise the ethical standards of scientific practice in India. Here, I am reminded of Lewis Carroll's famous line: ' "I'll be judge, I'll be jury", said cunning old Fury'. There is also the oft-stated assumption that the treatment of alleged misconduct cases is carried out more efficiently in other parts of the world. Although the Office of Research Integrity (ORI) was set up over twenty years ago in the USA, the number of cases resolved is only the tip of the iceberg. Institutions struggle with their internal investigations and the fate of whistleblowers remains a matter of concern. A sad and disturbing case at the University of Wisconsin, which hinges curiously enough on manipulated Western blots, ended last year with the resignation of a professor, leaving questions about the veracity of data in three published papers in *Nature Structural and Molecular Biology*, *Developmental Biology* and *Molecular Cell* (Couzin, J., *Science*, 2006, **313**, 1222). Over nine months after this report, none of these papers has been withdrawn, with one journal reportedly waiting for the results of an ORI investigation. The reluctance of journals to publicly state a position on these papers is in sharp contrast to the treatment of the NCCS paper by the *Journal of Biological Chemistry*. It is difficult to avoid the suspicion of bias; I raise this even at the risk of being described as a 'scientocrat' who 'resorts to calling it India bashing' with the intention of whitewashing 'the misdeeds exposed by *JBC* and SSV' (Modak, S., *Curr. Sci.*, 2007, **92**, 1469).

In order to dispel any impression that it is only Western blots and cell biology that throw up cases worth investigating, I must cite the example of Purdue University and the 'bubble fusion' controversy. Here the University has struggled to resolve an issue, which surfaced following publication of a dramatic result over four years ago (Taleyarkhan, R. P., *Science*, 2002, **295**, 1868). A third investigation has now been launched, even though two earlier probes did not definitively establish fabrication of a result (*Nature*, 2007, **447**, 238). In such situations, resolution of a case can be a long drawn-out affair. Indian institutions must learn from many of these experiences in order to address the problem of setting up fair and credible investigations. The job of probing misconduct can be arduous, if approached with a completely open mind. In small institutions (and many of our high profile laboratories are miniscule in size), it will be very difficult to set up impartial internal reviews. Including members from other disciplines can bring a much needed freshness to an investigation. Bodies that arrogate to themselves the power to pass judgements, with little regard for individual rights, need to understand that their quarrels with the scientific establishment cannot be settled at the expense of ordinary researchers, who must have the right to defend themselves, when accused of wrongdoing. In the NCCS case the SSV does not seem to have taken the trouble to ensure that the first author of the *JBC* 2005 paper had a chance to review and respond to the charges, although it may be argued that they have no *locus standi* to ask for a response. It is finally, the student who collected and organized the data, who stands firmly accused of fabrication. Supervisors can be charged in the worst case with complicity, or in the best case, with poor supervisory practice.

What then is the final resolution? If the verdict is 'not guilty' the authors can go back to work, undoubtedly scarred by the stresses and strains of a long drawn-out public controversy. Life may never be the same again. If the verdict is 'guilty', what is the punishment? This is a most difficult problem for institutions to address. Punishments must fit the crime. In the age of scientometrics the behaviour of scientists is conditioned by the tyranny of the journal impact factor. The pressures to publish in the most sought-after journals are impossibly high for those with overwhelming personal ambition. Stepping over boundaries between right and wrong is not uncommon. Indeed a recent study appears to provide a correlation between high retraction rates and high impact factors (Cokol, M. *et al.*, *EMBO Rep.*, 2007, **5**, 422); Butler, D. and Hogan, J., *Nature*, 2007, **447**, 236). Major errors of judgement are often committed under the intense pressures for quick success in brutally competitive, high-profile institutions. Do these merit the harshest treatment of dismissal and denial of degrees, or is there room for reprimand, punishment and rehabilitation? In science the greatest punishment is the silent censure of peers and the uphill task of attempting to piece together a shattered career. In India there is also the fear, and SSV articulates this concern well, that the 'guilty', if powerfully placed, will remain untouched and at times, be further strengthened by recognition and elevation. Investigations of alleged scientific misconduct must tread a dangerous path. Overzealousness can give the impression of a witch-hunt, while inadequate attention invites the charge of a cover-up.

P. Balaram

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EDITORIAL

The Birth of the Indian Institute of Science

A weakness for history and the temptation to retreat into the past, in order to escape the pressures of the present, has drawn my attention to two books which have appeared over the last year or so. Ramachandra Guha's compelling account of our history in the post-Independence era (*India after Gandhi: The History of the World's Largest Democracy*, Picador, 2007) and Rajmohan Gandhi's uniquely personal view of Mahatma Gandhi (*Mohandas: A True Story of a Man, his People and an Empire*, Penguin/Viking, 2006) relive much that has happened in India over the 20th century. Both books, formidably sized and extensively researched, are a testimony to the ability of talented authors to bring the past alive, permitting ordinary readers to be informed, educated and, at times, inspired. A key element in writing history is the passion to hunt for long forgotten records in libraries and archives. It is this fondness for things past that has nudged me into thinking about the birth of the Indian Institute of Science (IISc), which will soon enter the hundredth year of its existence. The history of IISc is intimately linked with the story of the evolution of higher education, research and science and technology in India, over the course of the turbulent years of the 20th century. It is a story that begins in the high noon of the British Empire and spans the entire period of the nationalist movement that culminated in Independence. It is also a story of the birth and growth of the science and technology enterprise over the last half a century. It is a story that begins with an act of philanthropy, unprecedented for its vision and unmatched for its generosity in the years that have followed. This journal, like many other institutions which would appear in later years, was conceived and midwifed into existence on the IISc campus in the early 1930s. This column, therefore, seems to be an appropriate place to remember the past.

IISc was the second scientific research institution to be set up in India. The distinction as the country's first research centre, in the modern era, must be accorded to the Indian Association for the Cultivation of Science (IACS), which was born in Calcutta (now Kolkata) in 1876, the brainchild of Mahendralal Sircar, ably supported by Father Lafont. IISc was founded somewhat later, in 1909, after a long and difficult period of gestation, but developed on a pattern entirely different from IACS over the course of the century. Indeed, a comparative study of the growth and development of these two institutions may prove educational for those who seek to build new institutions today. In trying to piece together an authentic historical record of the institution, where I have worked for so long, and in attempting to create a permanent Archives for the future, I have realized, with some dismay, that history is not a subject of any significance within the precincts of a research institute. But, in many ways, there is much to be learnt from the events of the early years of IISc.

To what sources must we turn in order to recapture the key events in the genesis of what is, arguably, India's most important scientific research institution? There are two biographies of Jamsetji Tata: the first by Frank Harris which appeared half a century ago (*Jamsetj Nusserwanji Tata: A Chronicle of His Life*, Blackie, 1958) and the second, a smaller and more recent account, by R. M. Lala (*For the Love of India: The Life and Times of Jamsetji Tata*, Penguin/Viking, 2004), whose publication coincided with the Tata Centenaries. There is one account of the birth and development of the IISc, authored by B. V. Subbarayappa that appeared in 1992 (*In Pursuit of Excellence*, Tata McGraw Hill). All three sources detail the events that followed J. N. Tata's initial proposal to pledge a substantial part of his wealth towards creating a research institute or university. The Tata scheme was the product of a penetrating vision that could see very far into the future. The idea of a postgraduate research institution must have seemed far fetched in the 1890s, at a time when university education had an extremely limited reach. J. N. Tata backed his vision with an unprecedented act of philanthropy and most remarkably did not want his name to be associated with the new institution, thereby paving the way for support from all quarters. For the scheme to materialize two conditions had to be met. First, assured annual support from the government of India, whose powers were vested in the British Viceroy in Delhi, was essential. Second, identification of a location and a land grant was crucial to the implementation of the scheme. The British government's objections were overcome by 1905 and the grant of land from the Maharaja of Mysore was realized in 1907, culminating in the issual of a formal vesting order in May 1909. J. N. Tata died in 1904, unaware that his idea would indeed bear fruit. The tradition of philanthropy was firmly established in the House of Tatas when his sons, Dorab and Ratan, committed themselves to the vision of establishing a research institute. The story of the long struggle to ensure that the IISc did indeed come into existence and its difficult years after birth are not well known.

There are many elements in the saga of the Institute's birth. J. N. Tata's letter to Swami Vivekananda is now a part of the Institute's folklore: '...It seems to me that no better use can be made of the ascetic spirit than the establishment of monasteries or residential halls for men dominated by this

spirit, where they should live with ordinary decency and devote their lives to the cultivation of sciences – natural and humanistic. I am of the opinion that if such a crusade in favour of an asceticism of this kind were undertaken by a competent leader, it would greatly help asceticism, science and the good name of our common country; and I know not who would make a more fitting general of such a campaign than Vivekananda...'. The discussions on the import of J. N. Tata's letter have been elaborate (Basu, S. P., *Prabuddha Bharata*, 1978, pp. 413–420; 448–458), although Harris' original biography confines this episode to a footnote. Both men, undoubtedly, saw with remarkable clarity the need for India to build its own centres for research and technological advancement. Sadly, both died several years before the founding of the Institute, Tata in 1904 and Vivekananda in 1902.

Today India is in the throes of a new round of institution building. It is clear that many schemes can be conceived in committee rooms; the real challenge lies in defining and realizing a vision. Can anything be learnt from the past? How was the plan for creating IISc drawn up and how successfully was it implemented in the early years? How did the bureaucracy of British India respond to an initiative that had no precedent? The answers to these questions are necessarily long and buried in hundreds (indeed thousands) of pages of documents (some disintegrating) lying in the National Archives in Delhi. A few are to be found in the more recently created Tata Archives in Pune and, of course, in the libraries in London, which maintain much of the written record of nearly two centuries of British presence in India. As the institutional archives begins the slow process of collecting and cataloguing records that are more than a century old, I have realized that the story of the early history of IISc really centres around one man, Burjorji Padshah (1864–1941), and his complex and, at times, difficult relationships with two Englishmen, George Nathaniel Curzon (1859–1925), the Viceroy of India and Morris Travers (1872–1961), who was the first Director of the Institute.

By all accounts, Padshah was a remarkable man. Intensely loyal to the vision of J. N. Tata, he worked unceasingly to bring the projects of the steel plant, hydroelectric company, and the research institute to fruition. Padshah, a ward of J. N. Tata, came under the spell of Gopal Krishna Gokhale, but moved on to work for the establishment of the Institute. He toured the world between 1896 and 1898 to learn from Western experience. He drafted the early documents of which the report entitled 'Institute of Scientific Research for India' (1898) must really mark the starting point for the long and protracted negotiations with the British government. The idea of using an American Institution like Johns Hopkins as a model, rather than British or European universities, was due to Padshah. Even a cursory glance at available archival material between 1898 and 1910, establishes Padshah as a central figure in realizing Tata's vision. Padshah's skills at negotiations, his prodigious intellectual abilities and his complete detachment from material pleasures seem to have been key elements in his successful pursuit of the goals set by J. N. Tata. His personal idiosyncracies were such that Mahatma Gandhi (with whom he later corresponded) had this to say: '...I had never met him (Padshah), but friends said that he was eccentric. Out of pity for horses he would ride in tramcars, he refused to take degrees in spite of a prodigious memory, he had an independent spirit, and he was a vegetarian, though a Parsi' (*Sands of Time* (Tata Archives), April 2005, p. 4).

I am no historian, but was fascinated by an essay authored by Kim Sebaly in *History of Education* (1985, **14**, 117–136) entitled 'The Tatas and University Reform in India, 1898–1914', which a young and alert colleague stumbled upon, while surfing the Internet. Sebaly details the persistent efforts of Padshah to promote the Tata scheme in the face of Government reservations. The low point in the struggle was reached when Padshah publicly proclaimed that the new Viceroy (Curzon) was 'in sympathy' with the scheme. This drew a blunt response from the Home Secretary: '...desist from quoting Lord Curzon's name or views'.

Almost the very first issue that Curzon faced when he landed in Bombay as the new Viceroy in December 1898 was the proposal to set up the Institute. Indeed, a deputation including J. N. Tata and Padshah met him on 31 December 1898. Curzon was a brilliant and complex man. The veteran journalist Durga Das provides an assessment, a generous one: 'In a real sense, nevertheless, Curzon was the midwife of India's emergence on the world scene... What Curzon set in motion was decades later to find consummation at the hands of Jawaharlal Nehru' (*India: From Curzon to Nehru*, Rupa & Co, 1981). A quotation from Curzon, used by Durga Das, highlights an imperial ambition: 'India is the pivot of Empire, by which I mean that outside the British Isles we could, I believe, lose any portion of the Dominions of the Queen and yet survive as an Empire; while if we lost India, I maintain that our sun would sink to its setting'. Durga Das has a tempered view of Curzon's efforts in university education: 'The measures Curzon introduced to reform university education and promote technical training bear the stamp of a courageous vision, although they confirmed his anti-Indian bias by excluding Indian intellectuals from membership of the commissions on university education.'

In piecing together a documentary record of an institution's early days I have had tantalizing glimpses of individuals and events. From the Tata Archives there are letters, hard to decipher at times, from Padshah to Gokhale. From the archives at the University of Strathclyde comes a letter from Sister Nivedita to the Scottish 'thinker and planner' Patrick Geddes (28 January 1903) which says: '...The last time I saw Tata's Secretary he was quarrelling with (William) Ramsay in order to have yourself named as the Principal of the Institute. Personally, I think nothing will come of this scheme'. At the University College in London there is still a treasure to be seen, an unpublished manuscript of Morris Travers' autobiography. Why study the history of IISc? Kim Sebaly notes that his first visit to India in 1965–66 was to do 'research on the establishment of the IITs'. He notes that he then discovered what he regards 'as the source of the social and intellectual capital that led to their establishment after Independence: the IISc, Bangalore'. He adds: 'However important foreign technical assistance was to the establishment of the IITs, I thought (and still think) the story could be more accurately told through a better understanding of the struggle to establish the Institute'. The full story of the IISc and the men who built it is yet to be written. If the right scribe is found, it should be a tale worth reading.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Universities: Restructuring and Reform

January is the month for conferences and overseas visitors. The weather over India is at its best, attracting both tourists and others on more serious business. The annual Science Congress is the most visible event, with its centrepiece being the inaugural session, where the Prime Minister publicly articulates new programmes and policies, related to science and scientists. This year the Congress was held in the coastal town of Vishakapatnam, on Andhra University's large and magnificently located campus. Having spent some of the best years of my youth, in what was once a small provincial town, I returned to Vishakapatnam, hoping to revive memories of my long forgotten period of adolescence. In keeping with the times, the city had transformed beyond recognition, but the campus of the University seemed still recognizable; magnificent stone buildings, atop a hill overlooking the Bay of Bengal.

In the brief period that I was there, I could sense that much had changed. The University had probably seen better times. Even as the city around it had evolved under the selective pressures of a liberalized economy, the intellectual environment of the University had probably declined; the institution I had in my mind's eye once boasted of some of the best departments in the country, spanning a wide range of disciplines. On my way back, I was consoled, somewhat ironically, by the thought that this process of academic decline was not specific to an institution, which I had once admired from afar, but a phenomenon that appears to have permeated the entire University system in India. The decline of intellectual discourse, the absence of a vibrant academic culture and difficulties of attracting the best of faculty and students in our Universities have of course been the subject of much discussion. Can anything more be said? Even more importantly, 'Can anything be done?'.

Returning to Bangalore in a pensive mood, I was confronted with the task of speaking at a course on 'An Integrated Approach to Knowledge and Information' intended for university and college teachers. The organizers were clear in their instructions. I was asked to speak on 'Restructuring Indian Universities: Renewed Focus on Research' or 'Reforming Indian Universities: Triad of Universities, Research Institutes and Industries'. As a pliant and submissive invitee, I worked hard to put together a presentation, which would, in some measure, address the concerns of the course organizers. Two words in the suggested titles made a deep impression on me, *restructuring* and *reform*. They immediately brought to mind the two words in Russian that were immortalized by Mikhail Gorbachev in the mid-1980s; *perestroika* and *glasnost*. The former is translated as *restructuring*, while the latter means *transparency* or *openness*. The events that followed in the Soviet Union and Eastern Europe, largely as a consequence of the effects of *perestroika* and *glasnost*, are now part of history. A new order appears to have evolved out of the turmoil. Restructuring, especially when accompanied by a desire to be transparent, can be a powerful weapon of change. Is India's university system now at the cross-roads? Is this the opportune moment for a major intervention by governments, state and central, and academic bodies, which might catalyse a transformation?

In thinking about universities, assistance appeared from an unexpected quarter. January brought to India and Bangalore a distinguished academician and administrator, Alison Richard, the Vice-Chancellor of the University of Cambridge; an institution with an unmatched history and tradition as a centre of scholarship. Over a period of almost eight hundred years, Cambridge has grown to be a model for universities worldwide. Two books, both published by Cambridge University Press, provide a glimpse of the institution's influence in shaping the modern world; *Cambridge Minds* (ed. Mason, R., 1994) and *Cambridge Scientific Minds* (eds Harman, P. and Mitton, S., 2002). The trail of Cambridge's contribution to modern science begins with William Gilbert (1544–1603) who discovered the Earth's magnetism. It ends in the present with Stephen Hawking. Along the way are many iconic figures: Isaac Newton, Charles Darwin, Charles Babbage, James Maxwell, Paul Dirac and Alan Turing. Some are paired with others or their laboratories: Stokes and Kelvin, Thomson, Rutherford and the Cavendish, Hopkins and biochemistry, Sherrington, Adrian, Dale and the Physiology Laboratory, Russell and Whitehead, Hardy and Littlewood, Crick and Watson. The list is remarkable also for its omissions, Francis Bacon and Henry Cavendish, among others. I have not touched upon the humani-

ties, social sciences or disciplines that border science; *Cambridge Minds* provides some profiles including John Maynard Keynes and Ludwig Wittgenstein. But, Cambridge today is indeed challenged by the large American universities in a manner that could hardly have been anticipated at the beginning of the 20th century, when the scientific revolution was well and truly launched from England and Germany.

Thus, the Cambridge Vice-Chancellor seemed to be uniquely positioned to answer the question, 'What makes a world class university?' (Alison Richard, *The Economic Times*, 7 January 2008). Having often been challenged to make the Indian Institute of Science truly world class, I was encouraged by her somewhat cautious start: 'Many people talk about "world class" universities, but what the term means is often left hovering in the air, undefined'. She goes on to identify four factors that are critical in reaching 'world class'. First, there must be 'a commitment to breadth and excellence in all fields of human inquiry, not simply in a particular niche'. Her second point is one that may not find wide approval in our research institutions: 'World class universities engage in cutting edge research whilst at the same time teaching the next generation, their students. Teaching and research are intrinsically bound together, with top researchers inspiring and mentoring their students. In turn, students themselves inspire and challenge their teachers'. Her third point is one that will leave both faculty and administrators, in our midst, bemused: 'Great universities must allow their researchers the freedom to experiment, succeed and sometimes fail. They must be able to make grand mistakes as well as grand discoveries'. Her last point is a recognition of the rapidly changing face of science: 'World class universities must have permeable boundaries. This means encouraging interdisciplinary research and teaching; it means working with the private sector for example, fostering and encouraging partnerships with industry; and it means encouraging international collaboration'. While Richard has reiterated much of what is recognized as the mandate of a modern university, her conclusions are important: '... universities must never forget that they are very much embedded in their countries and their regions... We are all very much part of where we live and, as we look to the future, managing the balance between the international and the local is one of the main challenges universities confront'.

In thinking about Universities my attention was drawn to a somewhat unusual source, the Silver Jubilee Volume of the *Journal of the Annamalai University*, February 1955. Here, in an essay entitled 'The New Idea of a University', M. Ruthnaswamy discusses the transformation of Cardinal Newman's original mandate of a university, '...diffusion of knowledge and extension of knowledge rather than the advancement', into the modern research driven institution. In India, for a brief period before the growth of the specialized research institutions, universities did indeed fulfil, in large measure, this dual duty of both research and teaching. Sadly, the emphasis on research has diminished over the years in the universities. The fragmentation of these centres of learning into specialized technical universities has hastened the decay. In many states there are medical, engineering and law universities (agriculture's separation seems to have happened even at the start) leaving the original university to function with only the sciences and humanities as areas of study. The separation of undergraduate and postgraduate teaching is almost complete, the former largely the province of affiliated colleges. Autonomy of colleges with postgraduate courses, but little or no research activity, ensures a complete separation of teaching and research. The institutions that are intended as research centres do little or no teaching, contributing in negligible fashion to either undergraduate or graduate education. The rise of the 'directed universities' intended to produce students trained to fulfil a special need is a new phenomenon. The strategic sectors of space, defence research and atomic energy have all turned into educators, creating a new definition of the term 'university'. Ruthnaswamy's 1955 essay anticipates the problem: 'A University teaching one special subject is a contradiction in terms. It is opposed to the basic idea of a university which is a corporation of teachers and students engaged in a variety of studies. Corporate social life and a liberal education are its differentia'.

The importance of higher education has been recognized by the impressive allocation of resources projected in the 11th Five Year Plan. The promises of a dramatic increase in the number of new universities must, of course, be viewed in the context of the quagmire in which our present university system finds itself. The National Knowledge Commission's Report (2007) is candid: 'We recognize that a meaningful reform of the higher education system with a long-term perspective is both complex and difficult. Yet it is imperative'. The report offers suggestions, but implementation of any agenda for reform has never been easy. Indeed, if reform and restructuring must happen, the movement for change must come from within the universities. The faculty and administrators at our universities must be active participants in defining the reform process. We must also reassess the policy of starting small and specialized institutions, which by a regulatory sleight of hand, transform themselves into deemed universities. In the modern world, universities are key elements in driving economic development and in facing the challenges of global competition. Restructuring and reform of our existing system must accompany the process of expansion.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Judging Academics

Evaluating the work of researchers and teachers is not an easy task. It is probably easier to assess the performance of investment bankers and marketing managers, although I suspect there are many other professions where evaluation may be a difficult exercise. Academic institutions, universities and research laboratories need to periodically assess performance of individuals and departments, if improvements are to be effected. In these days of 'world rankings', Indian institutions cannot afford to be totally left out or appear to slide down the charts in the face of global competition. Assessments, evaluations and the rate of success of new recruits, both faculty and students, in establishing themselves in productive niches, are therefore legitimate concerns of those responsible for administering academic institutions. I was therefore very attentive when a young, ambitious and articulate colleague argued that the emphasis on research publications, as an index of academic success, was misplaced. He advanced the view that it was important to also consider the contributions that individuals (and, by implication, institutions) make to 'human resource development (HRD)'. My interrogator asked me pointedly: 'Do you consider the creation of human resource as an important parameter in the academic assessment of faculty?' I have never been comfortable with the abbreviation HRD or its expansion; this is a phrase that has replaced a simple word, that was common in simpler times – 'education'. However, both government and the corporate world believe that it is an apt descriptor for activities, which train people to be usefully and productively employed. HR (human resource) managers seem to be important functionaries in every major corporation, but are unknown in academia. In thinking about my colleague's poser, I came to the conclusion that he was referring to teaching in all its forms; in the conventional classroom, as well as in the laboratories, where students learn the practice of research. A few days later, I was in the audience when C. N. R. Rao, one of our most illustrious scientists, reflected on a long and unmatched career. In concluding a forceful and exuberant presentation, he exhorted the young and aspiring researchers, who had assembled in large numbers, to be 'unafraid of publishing'. His argument was compelling; quality and quantity of academic output must go hand in hand if Indian research was to keep pace with the increasingly competitive world of global science. This was a message which seemed to be clear and unambiguous; one which I could confidently endorse, both as a scientist and as an administrator. But, listening, as I have been to divergent views, two questions seemed to loom large. First, what does it take to be 'successful' in science? Second, how can institutions judge the performance of researchers and teachers?

A career in academia can be most enjoyable and fulfilling, low salaries notwithstanding. Academic scientists choose their own problems and follow their own inclinations, jealously guarding their freedom and independence. But academic careers can follow a treacherous path, especially in the early years. A Ph D is the passport to a life in research. Choosing disciplines and mentors ('guides', a term which somehow always reminds me of R. K. Narayan's famous novel) can be decisive in shaping careers. The craft of research is best learnt by apprenticeship. The ability, to make judgements about scientific problems and approaches to studying them, is effectively acquired by closely watching the more experienced and successful practitioners of the craft. The postdoctoral period allows a glimpse of the increasingly common world of team effort; it is a time when the skills of collaboration are often learnt. The hardest period is undoubtedly at the start of an independent career, when problems have to be chosen and the difficult lessons of mentoring have to be mastered. In most fields, new faculty members at any of India's major institutions are able to quickly attract students willing to register for research degrees. The immediate task then is to choose the right research problems to address. This is often difficult for young researchers returning to India after long stints in large groups in Western laboratories, which function like well-oiled research machines. Imported problems rarely take deep root in local soil. The absence of technical support to operate and maintain sophisticated equipment is a sudden impediment that confronts researchers, who are used to efficiently run 'core facilities'. In fields like biology and chemistry, the need to routinely use expensive and complex equipment is acutely felt; technical innocence can be a crippling handicap. For the 'inbred' researcher, in our institutions (and inbreeding is a common practice) there is a regrettable tendency to continue with problems and areas investigated for a doctoral thesis. Succumbing to this temptation is clearly an invitation to bury a research career even before it has begun. Choosing a problem that is topical and addressable is a challenge that must be faced. Reading the literature, listening (and listening carefully) to seminars

and taking an interest in problems being tackled in the surroundings is a pragmatic way of feeling one's way through new areas of research. Chasing after ever changing fashions in research can turn out to be an unrewarding exercise, in the Indian environment. Peter Medawar in *Advice to a Young Scientist* (Harper and Row, New York, 1979) offers a cautionary note: 'It is one thing to fall into step with a great concerted movement of thought such as molecular genetics or cellular immunology, but quite another to fall in with prevailing fashion, for say, some new histochemical procedure or technical gimmick' (p. 15).

How long must research problems be pursued if no results of note are forthcoming? This is a question which is difficult to answer in a precise fashion. The history of science is replete with examples where great success has been achieved after a long period of drought. Relentless pursuit of a problem by an obsessed and determined researcher can yield a solution after considerable periods of time. But, in today's world there is little sympathy for long, unproductive periods in a researcher's life. Results, publishable and visible, are important. It is therefore critical for researchers, especially in the early phases of their careers, to judge how long they will plough an unyielding field. Most often, it is intellectually satisfying to pursue what is sometimes called 'hypothesis-driven research', as opposed to the simple minded approach to science, which Ernest Rutherford famously labelled as 'stamp collecting'. It is not uncommon to see researchers driving their students to find experimental support for a pet hypothesis. When this is not forthcoming a very difficult situation can develop; many mentor–student relationships have floundered when pet hypotheses resist experimental verification. Medawar proffers sage counsel: 'I cannot give any scientist of any age better advice than this: the intensity of conviction that a hypothesis is true has no bearing on whether it is true or not' (p. 39). The irrational desire to provide an experimental test of a flawed hypothesis can be dangerous; overly optimistic interpretations of results can lead to researchers deceiving themselves. Medawar has a sombre thought: 'A scientist who habitually deceives himself is well on the way toward deceiving others' (p. 39).

For researchers at the start of an independent career, there is a sudden transformation in role; the apprentice is expected to blossom into a teacher. The twin responsibilities of classroom teaching and mentoring students in laboratories can seem formidable in the early years. Teachers acquire a degree of competence in classroom lecturing, with practice. Building a viable research group is a key ingredient for success in most areas of science. There are very few research problems today that can be successfully tackled by lone researcher. The difficulties of group activities quickly become apparent, as individuals vary greatly in their personality traits, their intellectual and experimental abilities and in their commitment to work. Students (and senior researchers) can be energetic and aggressive, passive and introverted, ambitious and cunning, enthusiastic and childish. The selection procedures at the best of our institutions are intended to ensure a basal level of academic ability. They rarely test motivation and commitment. The task before institutions and mentors is to mould the new entrants into competent and productive researchers. It is this activity that my colleague seemed to be referring to when he used the term 'development of human resources'. Most young members of the faculty develop passable skills at mentoring, but few will disagree that mentoring can, at times, be a difficult task. The pressures of research and the drive to be successful can often make senior researchers view students as a 'resource', with which their personal career goals are advanced. In these situations the student can acquire a number of technical skills, but may be slow to mature into an independent scientist, with a broad view of the discipline. Mentoring, like parenting, can be both pleasurable and stressful. Patience and good humour are characteristics of successful mentors. The best teachers are those who like to have students around them always; age is never a barrier in this relationship. Teachers can be intellectually refreshed as each new generation of students stimulates a renewal of the mentor's commitment to scientific research. The inability to act as a mentor can destroy scientific careers, as research productivity, most often, depends on the work of students. There is one important difference between conventional teaching and mentoring in a research environment. The damage that can be done by a poor lecturer in the classroom is limited. Far greater damage can be inflicted in a research laboratory.

I must return to my original concerns. Are publications the only parameter for assessing faculty performance in our research institutions? I cannot really think of a simpler and more reliable metric of measuring scientific output. Assessments of what constitutes acceptable quantity and quality necessarily vary across fields. Comparisons are always more reliable when restricted to well defined areas. We must also remember that Ph D research that does not eventually get published, makes most serious academics uncomfortable. For work from India to have significant impact and to be noticed, both quality and quantity are essential. Mentoring is another activity that merits credit. The primary purpose of research institutions is to provide a launching pad for academic research and to produce successive generations of trained researchers. In emphasizing the role of 'creative mentoring', the journal *Nature* has introduced a new award, 'created on the premise that the mentorship of young researchers – although fully deserving of recognition – is perhaps the least remarked on of all the activities that take place in the lab' (Lee, A., Dennis, C. and Campbell, P., *Nature*, 2007, **447**, 791). After a survey of the attributes of good mentors, the authors conclude: 'Having a good mentor early in one's career can mean the difference between success and failure in any career'. Even more importantly, they note: 'Those who are good mentors get incalculably more out of it than they put into it'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Homi Bhabha and Reflections on the Administration of Science

1966 was an unhappy year for India. The 'green revolution' was still to materialize and food shortages were a difficult reality. A war with Pakistan had just ended. The Prime Minister Lal Bahadur Shastri died suddenly at Tashkent, on January 11, a few hours after signing a declaration that was to herald an uneasy peace on India's borders. A few days later, on January 24, Homi Bhabha, the head of India's Atomic Energy program died, when an Air India flight crashed on Mont Blanc. I was reminded of that turbulent year when a senior colleague drew my attention to a lecture delivered by Bhabha at a meeting of the International Council of Scientific Unions (ICSU), held at the Tata Institute of Fundamental Research (TIFR) in Bombay. The lecture entitled 'Science and the Problems of Development' was presented on January 7, Bhabha's last major appearance before his tragic end. The copy I received was a version, presumably distributed on that occasion, printed at the Onlooker Press in Bombay. But, in these days of the Internet, it was not long before I discovered that the entire text had indeed been published in *Science*, almost immediately in the February 4 issue (1966, **151**, 541–548), with a footnote by Frederick Seitz, the then President of the US National Academy of Sciences. Seitz noted that Bhabha 'addressed the assembly on the role which science can and should play in a developing country such as India. To those of us who knew Homi Bhabha and his dynamic nature it was apparent that he was undertaking the task with even more than his normal vigor. During the address he spoke with a species of intensity that was unusual even for him'. Reading Bhabha's lecture over four decades later, I am struck by how much of it is relevant even today; although the world has transformed in a manner that could hardly have been anticipated in the 1960s, at the height of the Cold War, which was a major driver in the growth of science and technology in the West.

Homi Bhabha viewed science in India from a unique perspective. By 1966 he was clearly the most remarkable builder of scientific institutions in India. The TIFR was formally conceived by him in 1943 and quickly came into existence in 1945, supported by the Sir Dorabji Tata Trust and the Government of Bombay. He was quick to persuade Jawaharlal Nehru and the government of newly independent India to set up the Atomic Energy program in 1948; an initiative that came in a surprisingly short time, after the military applications of atomic energy were demonstrated in horrific fashion at Hiroshima and Nagasaki. Between 1940 and 1945, Bhabha was at the Indian Institute of Science (IISc), Bangalore. In his biographical memoir, Lord Penney quotes M. G. K. Menon as describing this time as 'the period when he [Bhabha] found his mission in life; when he became aware of the role he would play in the development of India' (*Biographical Memoirs of Fellows of the Royal Society*, 1967, **13**, 78–97). One of the most readable accounts of Bhabha's role as an institution builder is by R. S. Anderson, which describes the contrasting approaches of Bhabha and Meghnad Saha (*Building Scientific Institutions in India: Saha and Bhabha*, McGill University, 1975). Anderson's analysis read over three decades after its publication is still valuable in providing insights into the evolution of TIFR and the Saha Institute of Nuclear Physics over the years. In a thoughtful concluding section, Anderson addresses the problem of 'dual loyalties in a divided landscape'. Here he refers to the competing demands of 'the necessities of organizing scientific institutions to do research of international standards' and 'the political needs of the people and their government to the development of the nation' (p. 98).

Bhabha's 1966 address to the ICSU General Assembly is particularly relevant today as we worry about the growing bureaucratic stranglehold on scientific institutions and the problems of dramatically expanding the science and technology infrastructure in the country. Even as we contemplate the problems of creating dozens of new institutions and raising significantly the output of scientific research, Bhabha's presentation, made over 40 years ago, merits re-reading. He describes the different paths taken by the Department of Atomic Energy (DAE) and the Council of Scientific and Industrial Research (CSIR). Bhabha notes that the device of setting up CSIR as a 'Society with a Governing Body' freed it 'from recruiting its staff through the Union Public Service Commission, whose methods are both time consuming and inappropri-

ate for the selection of scientific and technical staff'. He however notes, with what I must assume as a tinge of regret: 'The opportunity of framing an administrative structure and rules and procedures appropriate for a scientific organization, which this set up was intended to confer on the Council of Scientific and Industrial Research has, however, been lost to a large extent by the omnibus adoption of Government rules and regulations'. Four decades later there are many institutions set up as 'societies' apparently autonomous, which are completely in the grip of procedures intended for institutions with completely different purposes. Today, recruitments, assessments, promotions and even appointments of heads of organizations are entangled in a maze of centralised procedures, a situation that Bhabha seems to have sensed even in the 1960s. Even then he appeared critical of the practice of filling national laboratories by drawing away senior faculty of university departments. Bhabha appears caustic when he says: 'The standard method of planning laboratories and filling posts is often forced on many by the administrative and financial requirements of the Government'. He quotes P. M. S. Blackett: 'We must endow ability whenever it is found, and we must guard against subsidising mediocrity'. To every observer of scientific institutions it must be apparent that this sound piece of advice has been rarely heeded. Bhabha noted: '...it is in Government's interest to study and devise *de novo* the best administrative and financial procedures for scientific institutions and for getting the maximum return on the money spent. To apply existing administrative and financial procedures, devised for an entirely different purpose, to scientific institutions is largely to defeat the purpose which the Government has in view by letting the tail wag the dog'. Over the decades since Bhabha raised these concerns, procedures at scientific institutions have remained frozen in time, limiting the ability of organizations to respond to changing environments. The apparent success of the Atomic Energy Commission, and later the Space Commission, in maintaining a substantial degree of administrative and financial autonomy has led to the formation of other Commissions, in the optimistic hope that this device will lead to administrative efficiency. But, by and large, academic and research institutions in India struggle to cope with the growing pace of scientific and technological change, burdened as they are with cumbersome procedures. There is a growing appreciation of the need for academic institution to build up a corpus of funds that can be quickly deployed for development. Unfortunately, government often adopts an ambivalent and unpredictable attitude towards these resources. Financial rules are usually designed in a manner that legitimizes inefficiency and delays execution of projects. Decision making is invariably a slow and deliberate process, in which files move at a glacial pace up and down a rigidly hierarchical organizational set-up. The interface between the agencies which disburse funds and the institutions that receive them is usually limited. The processes of raising money and spending it are very slow in government institutions. This administrative ambience does little to facilitate competitive research.

Bhabha, in the concluding section of his address, reflected on the role of administration in science. He was forthright: 'It is thought by many that we are reasonably advanced in administration but backward in science and technology. This statement is misleading. We have fortunately inherited extremely competent administrative services capable of dealing with all the types of administration ... in what was intended to be a static and underdeveloped economy. ... The type of administration required for the growth of science and technology is quite different from the type of administration required for the operation of industrial enterprises, and both of these are again quite different from the type of administration required for such matters as the preservation of law and order, administration of justice, finance and so on. It is my personal view ... that the general absence of the proper administrative set-up for science is a bigger obstacle to the rapid growth of science and technology than the paucity of scientists and technologists, because a majority of the scientists and technologists we have are made less effective through the lack of the right type of administrative support.' Reading Bhabha's last speech, so many years later, I cannot help noting that little has changed in the administration of science. Indeed, in places it has changed for the worse. Even as we plan for a major expansion of the system for higher education and research in science and technology, we might do well to revisit the issues raised so eloquently by Homi Bhabha. Was Bhabha successful in his time in building scientific institutions that provided an exciting ambience for the pursuit of science? The answer is given emphatically in the Acknowledgements section of G. Venkataraman's book *Bhabha and his Magnificent Obsessions* (Universities Press, 1994). Here the author notes that his book 'is about a man who directly enabled me (and hundreds like me) to pursue a satisfying scientific career in this country'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Scientists, Teachers and Writers: Remembering a Remarkable Trio

California has a vibrant set of public and private universities, which act as a magnet for scientists from across the world. Science took root on the Berkeley campus of the University of California less than a century ago. Stanford and the California Institute of Technology (Caltech) are younger, but their rise to the pre-eminent position they occupy today has been spectacularly rapid. Two of the major scientific and technological revolutions of our times, now widely termed as information technology and biotechnology, erupted from the uniquely Californian ambience. The 'Silicon Valley' model is one that governments everywhere would like to emulate; a seamless fusion of academia and industry in an environment where academic idealism and corporate pragmatism coexist. California's institutions have been major drivers in the tumultuous progress that science has made over the past fifty years. They have provided an environment which has fostered creativity and scholarship, which must make institutions elsewhere, envious. California also boasts of the greatest concentration of alumni from the best of Indian institutions. Last summer, the North American gatherings of IIT and IISc alumni, were both held in Silicon Valley. I thought of California, while reflecting on three extraordinary scientists who died in the last few months; Joshua Lederberg (1925–2008), Arthur Kornberg (1918–2007) and Daniel Koshland (1920–2007). All three were major influences on their disciplines. Lederberg and Kornberg moved to Stanford in the late 1950s to chair the newly formed departments of genetics and biochemistry, while Koshland joined the Berkeley faculty in the 1960s. In the years that followed all three men would leave an indelible imprint as scientists, teachers and writers.

Lederberg was precocious, receiving a Ph D at Yale when he was only 22. He began his research career at an exciting time; Oswald Avery had just discovered that DNA was 'the genetic material', at the Rockefeller Institute of Medical Research (later to become a university), not too far from Columbia University, where the young Lederberg was a pre-medical student. Years later, he wrote: 'When biologists of that era used terms like protein, nucleic acid or nucleoprotein it can hardly be assumed that the words had today's crisp connotations of defined chemical structure. Sleepwalking, we were all groping to discover just what was important about the chemical basis of biological specificity. It was clear to the circle I frequented at Columbia that Avery's work was the most exciting key to that insight' (*The Rockefeller University Research Profiles*, Spring 1990). Encouraged by a far seeing mentor, Francis Ryan, Lederberg moved to Edward Tatum's laboratory at Yale, to do the initial experiments establishing transfer of genetic information in bacteria, that would eventually pave the path to his Nobel Prize in 1958. Lederberg reached the pinnacle of scientific success at the young age of 33, the greater part of his famous work being done at the University of Wisconsin, where he began his teaching career. Lederberg's ability to chart new courses and articulate new directions is illustrated by the words and phrases attributed to him in the Oxford dictionary; plasmid, transduction, prototroph and exobiology. It is at Stanford that Lederberg began to influence research and policy in disciplines far removed from microbial genetics. Lederberg saw very early the enormous potential of computers, in problem solving in chemistry and biology. The attempt at Stanford, with the computer scientist Edward Feigenbaum and the chemist Carl Djerassi to teach computers to handle chemical structures and interpret mass spectra, anticipated much of the later developments in the area of artificial intelligence. My own introduction to Lederberg's writings came through the many essays that he penned for the column in Eugene Garfield's *Current Contents* in the 1970s and 1980s. Lederberg's support and encouragement led Garfield to work on the *Genetics Citation Index*, the first attempt to begin the practical realization of the concept of citation indexes for science (Garfield, E., *Science*, 1955, **122**, 108). Lederberg was unwavering in his support for endeavours that allowed scientists to wade through the rapidly exploding scientific literature of recent years. His association with the *Annual Reviews Series* and the Institute of Scientific Information was evidence of a deep interest in organizing scientific information. The prefatory, often biographical, chapters in *Annual Reviews* provide a, albeit brief, glimpse into the lives of scientists, who are recognized by their peers as 'high achievers'. In in-

troducing a compendium of these chapters, *The Excitement and Fascination of Science* (Annual Reviews Inc., 1990), Lederberg reflects: 'Enmeshed in society, scientists may also find themselves with extra-scientific responsibilities and roles, though each of these is grounded in the fundamental one of discovering and telling the truth. . . .

. . . Our chapters abound in examples of the researcher doubling as teacher and publicist; organizer and manager; inventor, agent of technology transfer, and developer of useful applications; adviser to government or industry; prophet; and paragon. Caveat: perhaps those who write are a socially and self-selected sample; others may insist on staying at the bench to the exclusion of all else'.

Arthur Kornberg was an inspirational figure in biochemistry (and what later became popularly known as molecular biology). His discovery of the enzyme DNA polymerase led to the first test tube synthesis of DNA in the 1950s, resulting in the award of the Nobel Prize in 1959. Kornberg's enzymatic synthesis was to be followed by Har Gobind Khorana's *tour de force*; the painstaking chemical synthesis of oligonucleotides, which acted as templates for the Kornberg enzyme, paving the way for the studies that led to the deciphering of the genetic code. Khorana's Nobel lecture (1968) acknowledges the Kornberg influence: 'Some months later I visited Kornberg's laboratory (this was one of the many pilgrimages that I have made to this great laboratory) and started a few experiments with DNA polymerase and here, again, very short synthetic oligonucleotides containing alternating A and T units induced the enzyme to bring about extensive synthesis of the previously characterized high molecular weight dAT polymer'. Kornberg's influence on biochemistry was wide ranging. His autobiography was aptly titled: *For the love of enzymes: The Odyssey of a biochemist* (Harvard University Press, 1989). His prefatory statement that 'in my theater the nucleic acids write the script but the enzymes do the acting' is one that will strike a chord amongst a disappearing generation of biochemists. Kornberg was eloquent when he viewed the broad interdisciplinary interface of chemistry and biology. In a 1987 lecture, Kornberg borrowed C. P. Snow's famous phrase, 'The Two Cultures' to describe the rift between chemistry and biology (*Biochemistry*, 1987, **26**, 6888). In a lecture, whose message is valid even today, Kornberg noted with regret: 'Despite its enormous success. . .biochemistry has nevertheless failed to fill the gulf between chemistry and biology. . . . biochemistry itself is being pulled apart by the separate drifts of the two cultures from which it was assembled'. In his wonderfully worded essay he lamented: 'As long as this inattention to enzyme chemistry and basic biochemistry persists, the fundamental issues of cell growth and development will not be resolved and their application to degenerative diseases and aging will be delayed. . . . Molecular biology appears to have broken into the bank of cellular chemistry but for lack of chemical tools and training, it is still fumbling to unlock the major vaults.'

Daniel Koshland introduced the idea of 'induced fit' in explaining the specificity of molecular interactions involving proteins; an idea that replaced Emil Fischer's 'lock and key' theory of enzyme reactions. He anticipated the ideas of plasticity and dynamic mobility of biomolecular structures, which are so commonly accepted today. Koshland's influence as editor of *Science* for a decade (1985–95) marked a period of unprecedented transformation in one of the most influential journals in science. Koshland's humour and love for science are readily evident in his writings. His editorials which appeared as conversations with the pompous and egotistic Dr Noitall (undoubtedly a man who knew it all) were a delight to read. His 'farewell' editorial in *Science* (1995, **268**, 479) is a tribute to a man who knew how to laugh at himself, an endearing quality that is all too rare. Koshland's writings reveal a man who enjoyed science, teaching and communicating. He begins his biographical essay in *Annual Review of Biochemistry* (1996, **65**, 1–13): 'When writing a prefatory chapter for Annual Reviews a scientist is confronted with the question of what in his or her life might be interesting to others. In my case I was appalled at the absence of material that generates good novels: no broken homes, no misunderstood childhood, no criminal youth gangs, no disastrous liaisons. A landscape of boredom from sea to shining sea. If there is one overlying theme it is that I got paid for doing what I enjoyed all my life'. Koshland's enthusiasm is apparent when he writes: 'The pleasures of doing research and teaching made it difficult to pretend I was working instead of merely having a good time. Scientists are basically puzzle solvers and they get hired to solve puzzles. So someone else is providing the capital for them to satisfy a lifelong desire.'

At times, Lederberg and Kornberg were magisterial in their writings. Koshland's self deprecatory style was captivating. Why have I chosen to reflect on men whom I have never met? It is because over the years I have spent many hours reading what they wrote, and I never failed to come away inspired, challenged and enthused about science. In remembering them, I am, in small measure, paying a tribute to a generation of scientists who are slowly fading, whose passion and commitment act as a beacon for the generations that follow.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Darwin, Medicine and the Rise of Antibiotic Resistance

This year and the next mark two Darwin anniversaries. The first public unveiling of the ideas of natural selection and biological evolution happened at a meeting of the Linnean Society in London in 1858. It was at this meeting that Charles Lyell and Joseph Hooker presented to the Society the papers of Charles Darwin and Alfred Wallace, which had been produced independently. They noted that the papers 'relate to the same subject, viz. the Laws which affect the Production of Varieties, Races and Species'. The second and more celebrated event is the publication of Darwin's, *The Origin of Species*, which first appeared in November 1859. Coincidentally, 2009 also marks the 200th anniversary of Darwin's birth. Darwin's evolutionary synthesis has grown to be one of the three pillars on which modern biology rests; Gregor Mendel's work on inheritance and the recognition by Oswald Avery, James Watson and Francis Crick of the chemical basis of heredity are, arguably, the other two. In the century that has passed since the appearance of *Origins*, Darwin has attained scientific immortality; Wallace is much less known amongst those interested in science, although he occupies an important position in the pantheon of heroes in the field of evolutionary biology (Sarkar, S., *J. Biosci.*, 1998, **23**, 3; Smith, C. H., *Nature*, 2006, **443**, 33). A review of a Wallace biography published in this journal begins on a note of regret: 'In the folklore of evolution, Alfred Russel Wallace probably is destined to be the Other Man' (Nanjundiah, V., *Curr. Sci.*, 2002, **83**, 1151; reprinted in *Resonance*, 2008, **13**, 277). The impending Darwin anniversaries have triggered the writing of many scholarly retrospectives on his influence in biology and our understanding of life and its origins on earth. The final lines of *The Origin of Species* convey a sense of awe that Darwin must have felt and generations of readers must undoubtedly feel: 'Thus from the war of nature, from famine and death the most exalted object which we are capable of conceiving, namely the production of the higher animals directly follows. There is grandeur in this view of life, with its several powers having been breathed by the Creator into a few forms or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being, evolved'. In the century and a half since Darwin wrote these lines, the chemistry of life has been unravelled to reveal a complexity that is most intimidating. There remains a great deal of work to be done on establishing the connections between chemical and biological evolution. Complexity and diversity of form and function appear to be attributes of both molecules and organisms.

A recent essay on Darwin's enduring legacy begins emphatically: 'Perhaps no individual has had such a sweeping influence on so many facets of social and intellectual life as Charles Darwin. . . ' (Pandian, K., *Nature*, 2008, **451**, 632). The author notes eloquently that Darwin, a 'child of the Enlightenment was well aware of more ancient world views, and humbled by what the new investigations of the cosmos revealed. Humans are animals, one species of many on the planet bound by common ancestry to all other species, part of an ages-old dance of reproduction, accommodation, survival and alteration. It is for this vision, one that liberates humans from the conceit of special creation', that we honour Darwin two centuries after his birth. Since this is also examination time, I wondered how much are our children taught about Darwin. I took a clandestine look at a X standard biology textbook and found a picture of Gregor Mendel, but no mention of Darwin. There were sections on cell structure, genetics, respiration, nervous and reproductive systems, population and health, but surprisingly not even a passing mention of the origins of biological diversity. On enquiry, the owner of the textbook was dismissive: 'Only you and the BBC are interested in Darwin'. I was left wondering about the curious fact that while no physics textbook can escape mentioning Newton, Darwin whose impact on science is surely no less, does not appear to merit the same importance.

In this frame of mind, I was instantly alert when the lead speaker at a seminar, while describing the problem of antimicrobial resistance, displayed a slide with a picture of Charles Darwin, unlegended but clearly recognizable. It was indeed reassuring to see that practitioners in the area of public health and those interested in pathogenic microorganisms and antibiotics were ready to acknowledge an intellectual debt to Darwin. A little over two years ago the journal *Science* recognized 'Evolution in action' as the 'Breakthrough of the Year' (Culotta, E. and Pennisi, E., *Science*, 2005, **310**, 1878). In the commentary justifying the choice, evolution was described 'as the foundation of biology, so basic and all pervasive

that scientists sometimes take its importance for granted'. Here the authors seem to be echoing Theodosius Dobzhansky who many years ago stated simply that 'nothing in biology makes sense, except in the light of evolution'. Considering the accepted importance of evolutionary concepts in biology, the cavalier treatment meted out to Darwin in the high school textbook puzzled me. But, I quickly discovered that 'evolution' is a word that is avoided elsewhere too. An essay in the journal *PLoS Biology* (Antonovics, J. *et al.*, 2007, **5**, e30), entitled 'Evolution by Any Other Name: Antibiotic Resistance and Avoidance of the E-word', points out that authors studying the rise of antibiotic resistant microbes generally eschew the word 'evolution' in their papers. They appear to prefer the words 'emerge', 'arise' and 'spread' rather than 'evolve'. The major biomedical journals, *The Lancet*, *The New England Journal of Medicine* and *The Journal of Antimicrobial Chemotherapy* overwhelmingly prefer the word 'emergence' to 'evolution', when describing the appearance of microbial strains resistant to antibiotics. In their opening paragraph Antonovics *et al.* note that: 'The increase in resistance of human pathogens to antimicrobial agents is one of the best-documented examples of evolution in action at the present time, and because it has direct life-and-death consequences, it provides the strongest rationale for teaching evolutionary biology as a rigorous science in high school biology curricula, universities and medical schools'.

The connection between microorganisms and infectious disease was established by the heroic work of Louis Pasteur and Robert Koch in the 19th century, not long after Darwin's insights. The control of infection by chemotherapy made a dramatic advance following the introduction of penicillin, which marks the beginning of the antibiotic era. Penicillin's impact was spectacular, coming as it did towards the end of the Second World War. The presentation speech in December 1945 on the occasion of the Nobel award to Fleming, Florey and Chain recognize the double-edged nature of inventive genius: 'In a time when annihilation and destruction through the inventions of man have been greater than ever before in history, the introduction of penicillin is a brilliant demonstration that human genius is just as well able to save life and combat disease'. Alexander Fleming was prescient in his Nobel lecture, an instinctive Darwinian, when he sounded a note of warning: 'Penicillin is to all intents and purposes non-poisonous so there is no need to worry about giving an overdose and poisoning the patient. There may be a danger though in underdosage. . . . The time may come when penicillin can be bought by anyone in the shops. Then there is the danger that the ignorant man may easily underdose himself and by exposing his microbes to non-lethal quantities of the drug make them resistant'. Fleming concluded with a moral: 'If you use penicillin, use enough'.

For over a decade now the spectre of microbial resistance has begun to look increasingly more threatening. Public health authorities in the advanced countries have begun to react with concern to every case of infection by resistant microbes. Common pathogens like *Staphylococcus aureus* and *Mycobacterium tuberculosis* are now emerging (evolving in a Darwinian sense under the selective pressures of antibiotic use) as drug-resistant strains. Hospital environments are a fertile ground for accelerating the evolution of resilient bacteria, with the result that hospitalization can always entail a risk of acquiring a troublesome infection. An overview of the antibiotic resistance problem published a few years ago noted that 'bacteria are wily warriors but even so, we have given them – and continue to give them – exactly what they need for their stunning success. By misusing and overusing antibiotics we have encouraged super-races of bacteria to evolve. We don't finish a course of antibiotics. Or we use them for viral and other inappropriate infections – in fact, researchers estimate that one-third to one half of all antibiotic prescriptions are unnecessary' (Nicolaou, K. C. and Boddy, C. N. C., *Sci. Am.*, 2001, pp. 56–61). The microbial world is so adaptable that it is not only bacteria, but viruses, fungi and parasites which have all learnt the tricks of mutation and survival, despite attack from the enormous weaponry of the pharmaceutical industry. Faced with an environmental challenge the power of Darwinian 'natural selection' is best illustrated by the rise and spread of resistant microbes.

Since biology textbooks in school may pass lightly over Darwin and evolution, we might well ask if medical education fills this gap at a later stage. An editorial in *Science* notes that evolution's 'full potential for use in medicine has yet to be realized. Some insights have immediate clinical application, but most are fundamental, as is the case in other basic sciences. . . . Although anatomy, physiology, biochemistry and embryology are recognized as basic sciences for medicine, evolutionary biology is not (Nesse, R. M. *et al.*, *Science*, 2006, **311**, 1071)'. The authors go on to add that while 'the evolution of antibiotic resistance is widely recognized' very few in the biomedical community appreciate that 'competition among bacteria has shaped chemical weapons and resistance factors in an arms race that has been going on for hundreds of millions of years. The incorrect idea that selection reliably shapes a happy coexistence of hosts and pathogens persists, despite evidence for the evolution of increased virulence when disease transmission occurs through vector such as insects, needles or clinicians' hands'. The spread of resistance to drugs is a problem that transcends the area of bacterial resilience to antibiotics. The evolution of ever more virulent strains of pathogens is a problem that will remain with us; an inevitable consequence of the evolutionary forces that shape biology. In marking the 150th anniversary of the Darwin–Wallace papers, we recognize profoundly powerful insights that have transformed the modern view of biology.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science Journals: Issues of Access

For academic institutions in India, a library well stocked with science journals can be an exceedingly expensive proposition. The scientific literature is exploding and publishing is an increasingly profitable enterprise. The frenzy to judge scientific output in terms of numbers of publication is now exploited by publishers, who are rapidly expanding their stable of journals. *Nature*, which was for a long time a flagship of the science publishing enterprise, is now surrounded by an armada of journals bearing the *Nature* brand. The competition in the publishing world is fierce, with even old scientific societies turning to commercial publishers to bring a degree of professionalism to the job of producing and marketing their journals. Organizations like the American Chemical Society have a management structure and an outlook that is decidedly corporate. As journals multiply and costs increase, libraries (and institutions) must worry about budgets and coverage. The problems of libraries in India are compounded by the growing costs of sometimes maintaining both print and on-line subscriptions and issues related to 'perpetual' electronic access to backfiles.

Over the last few years a new wind has begun to blow over the turbulent world of science publishing. The Open Access movement, which appears to be most vigorously championed by activists, who have created a world-wide network, challenges the conventional view of paying for what you read. Their argument is compelling; science is most often paid for by public funds and therefore the results of research must be freely available to anyone who wishes to read. This will presumably enhance the worldwide reach of science. In effect, the internet revolution can be used to provide free electronic access to all articles. This is indeed a philosophy that must appeal to all except journal publishers, who will find few buyers if their products were freely available on the worldwide web. Journals add considerable value to manuscripts by the processes of selection and peer review; not surprisingly scientists yearn to publish in 'high impact' journals, which are most likely to reject their manuscripts. The costs of journal production clearly dictate that 'Open Access' journals will be financially unviable, unless the costs are met by another mechanism. This has given rise to a new model, the 'author pays' model. In this approach, scientists pay for publishing their results, while no cost is borne by the reader. This is a wonderful model from the point of view of the cash strapped librarian (or individual subscriber), but one in which publication costs must be met from research grants. The 'pay for publishing' model has been adopted by high profile journals that have been started by open access advocates, of which the journals belonging to the Public Library of Science (PLoS) stable are a prominent example. The costs to authors for publishing in the high impact, open access journals can be substantial; at times a figure as high as $6000 (a formidable Rs 2.5 lakhs) per paper has been estimated. For authors in the developed countries of the West, this figure may seem significant, but one that can be met from research grants. For researchers in India, with the exception of a very small minority of exclusive institutions, this figure is unaffordable. Authors in the developing world, understandably do not submit too many manuscripts to the most visible open access journals.

Mandating open access for all publicly funded research publications is easy to do by legislation. It is also a requirement that can be insisted upon by philanthropic private funding bodies like the Wellcome Trust and the Howard Hughes Medical Institute, which can underwrite the costs of publication. Many mainstream journals today follow a dual policy; if authors want their papers to be freely available, immediately after publication, they must pay; otherwise the papers may be read online only by paid subscribers. This is a model that appears to allow commercial publishers to profit from both the 'author pays' and 'reader pays' situations. The US National Institutes of Health, which has been a key promoter of open access, has advanced proposals which require grantees to post research papers on the PubMedCentral archive, within six months of publication. A recent report highlights a resolution passed by the Harvard University faculty that 'authorizes Harvard to place a faculty member's work in a repository that will be available to all at no cost'. Interestingly, this move by the arts and sciences faculty does not seem to affect the medical and public health schools. Faculty may opt out, but a dean's waiver appears necessary. The response from publishers has been predictably negative, with the president of the Asso-

ciation of American Publishers (AAP) noting that 'publishers may not be quite as excited to take articles from Harvard' (Lawler, A., *Science*, 2008, **319**, 102). The battle between open access advocates and publishers in the United States may still be far from a decisive stage; a report in *Nature* (Giles, J., 2007, **445**, 347) notes that the AAP appears to have hired an aggressive ('pit bull') public relations consultant to mount a media campaign against open access movements.

While the question of who will pay for 'open access' journals remains unresolved, an alternative strategy of promoting an 'open archives' movement seems practicable. Institutional repositories that maintain electronic files of all publications in a freely accessible form appear to be an attractive option for making research results widely available. Copyright issues remain, but many publishers seem comfortable with posting of publications on individual or institutional websites. Deposition of the 'prefinal' version of manuscripts has sometimes been suggested as a device to avoid controversies with difficult publishers. The idea of open, institutional archives is one that must be vigorously promoted in India. The introduction of legislation that vests copyright with institutions, in the case of publicly funded research, may also provide the necessary legal framework to avoid any contentious issues. The maintenance of open archives in institutions in India requires a considerable degree of education of researchers and administrators on the value of maintaining easily accessible repositories. With internet search engines becoming ever more powerful, with every passing day, open archives may indeed lead to greater visibility and higher citation rates for publications; an outcome that is most desirable for both individual scientists and their institutions. As libraries, worldwide, prune their subscriptions in the face of mounting costs, institutional archives are a good way of providing readers free access.

What is the future of libraries? In a 'reader pays' model, libraries remain the centrepiece of institutions. In an 'author pays' model, libraries will no longer be the conduit for payment to publishers. As fewer and fewer readers enter libraries, physically, we might envision our present structures transforming into monuments that may become objects of curiosity for a new generation. In a world of 'open access', authors will pay for publishing and institutions will pay for internet access. Faced with a monotonically increasing library budget an interesting exercise was conducted at the Indian Institute of Science, which houses what is probably India's largest holding of science and engineering journals. This was in response to the question: 'Can we prune the library's subscriptions?'. A young (and in such exercises, youth and competence seem inextricably linked) and enthusiastic colleague quickly generated a list of journals that might be dropped. The criteria were simple. Journals, in which the institutional faculty did not publish for a period of a few years, which were not cited by institutional authors and also did not cite any papers emanating from the institution, were listed. A quick scrutiny revealed almost no high profile journals, but did yield relatively new, rising journals. A little manual intervention then resulted in a list of journals that could be discontinued, resulting in a savings of about Rs 1 crore (about $ 250,000 in the globalized world). The hope was that the money saved could then be deployed to buy some of the new and attractive journals that are now beginning to appear in fast moving fields. Surprisingly, when the list was circulated there were strong pleas for retention of journals in which no one seemed to publish and there seemed little evidence for readership. The relationships between readers, writers, editors, publishers and buyers are extremely complex in the world of academia; sometimes bordering on the incestuous. Large publishing houses like Elsevier Science and Springer-Verlag are beginning to monopolize the world of science journals; a situation that promotes a seller's market. Efforts to contain bloating library budgets meet with resistance at every step; open access, with an 'author pays' model may eventually alter the dynamics of libraries. The cost burden will now be spread across individual researchers in an institution; a situation that will lead to a new set of problems for administrators to confront. The traditional librarian who worried about keeping his readers well informed in their areas of interest has given way to a new breed; administrators who respond to pressures from publishers and those who hold the purse strings. The traditional library is also disappearing; a place where one browsed in leisurely fashion through books and journals, attracted by bright covers and endlessly distracted by new and wonderful things to read. Internet surfing may be a poor substitute. The disappearance of the printed journal may be hastened by the wars over who pays for publishing science. Fortunately, new generations of researchers may never miss the library of yore, having grown up in the electronic world.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science, Invention and Pasteur's Quadrant

New programs, initiatives, products and even books are customarily launched at public functions, providing photo opportunities and a chance for acknowledging the contributions made by many well wishers of the venture. As a minor (and available) functionary, I found myself roped into speaking at a gathering organized to unveil an 'electronic classroom', in a school for underprivileged children. A magical white board, with a touchscreen, brought the world of *Google* and *National Geographic* into a small and crowded classroom, filled with expectant children. The wonders of the communication age permitted the proceedings to be beamed live to two other schools, several kilometres away. In a very real sense, the children had a wide open window through which the world was suddenly visible. My brief was simple. I was to answer questions asked by the children, including those who viewed me on a screen at a distant location. I am still a novice at videoconferencing and feel most comfortable when a questioner confronts me in flesh and blood. Even as I wondered what the children might say, the first question was flung at me 'Have you invented anything?', asked a bright little boy, his voice eerily distorted by technology. My instant and instinctive response was to say 'No, I have not invented anything'; honesty is sometimes not only the best policy but also a default option in a stressful situation. On the long journey back to my laboratory, through Bangalore's crowded and chaotic streets, I came to the conclusion that there is much to learn from children. The image of the scientist in the public eye is a simple one; scientists are inventors who create wonderfully useful devices. They are puzzled about what most scientists do, since inventions (especially those that are widely applicable) come along very infrequently. In thinking about inventors, I could quickly conjure up images of Benjamin Franklin, Thomas Edison, Alexander Bell and Nikolau Tesla. Michael Faraday seemed more of a 'scientist', in the mold that most professional scientists look up to, although the induction motor may be as practical and applicable a device as any. J. C. Bose was another name that seemed to straddle two distinct worlds; one, a practical and inventive arena, while the other was a more academic and esoteric playing field. The little boy's penetratingly innocent query raised another question in my mind. Was 'invention' a restrictive word that excluded scientific 'discovery'? Was the hugely popular word 'innovation' a modern term that expanded and linked the more traditional words 'invention' and 'discovery'? And, in the background loomed that perennial issue: are 'pure' and 'applied' science as distinctly separated as Rudyard Kipling's East and West?

Basic research up to the middle of the 20th century was largely driven by the desire to understand the physical and natural world. Inventions were the result of creative genius, some of them spectacularly altering the human condition. Organized, government-supported research, involving large numbers of scientists, is really a post-World War II phenomenon. Vannevar Bush signalled a new era in research policy when he produced his much cited 1945 report *Science: The Endless Frontier*, which set the stage for large scale public funding of basic research in the United States. Bush was clear in his view that applied ('applicable' may be a more appropriate term) research can grow only out of basic research, creating a powerful argument for supporting 'pure' science. More than half a century after Bush argued persuasively for public funding of science, the world has changed beyond recognition. The debate on the utility of basic research has grown in intensity, as have the expectations on the practical outcomes of scientific research. It is in this climate that Donald Stokes advanced a new approach to viewing the interconnected world of pure and applied science. Stokes, a professor at Princeton died in 1997, the year in which his influential book *Pasteur's Quadrant: Basic Science and Technological Innovation* was published (Brookings Institution Press, Washington DC).

Stokes examined the legacy of the Vannevar Bush report in the context of 'the debates over science and technology policy in our own time – the increasingly troubled dialogue between science and government today'. He notes that: 'admiring as we all can be of the success of the paradigm view set out in *Science: The Endless Frontier* and its ushering in of the Golden Age of American science, the incompleteness of this view of the nature of basic sciences and its relationship to technological innovation has become increasingly clear'. Stokes discards the 'linear' view of a progression of science from a 'pure'

zone at one end of the spectrum to an 'applied' region at the other. Stokes introduces a two dimensional conceptual plane in which the vertical dimension represents 'the degree to which a given body of research is motivated by the quest of fundamental understanding and the horizontal dimension the extent to which it is motivated by considerations of use'. Editorial columns are traditionally devoid of figures, but the Stokes Diagram is easy to visualize. Four distinct quadrants (varying dimensions may be useful) are marked on a square. The top left is the Bohr quadrant, capturing the space of basic research at its best, an unalloyed search for understanding. The bottom right quadrant honours Edison, symbolizing applied research and inventive ability of the highest order. The top right zone is Pasteur's quadrant, representing, in Stokes' words, 'use-inspired basic research'. Why did a professor of political science and public policy chose Louis Pasteur as the icon to capture the fusion of basic and applied research at the highest level? Stokes advances an interesting argument: 'There is no doubt that Pasteur wanted to understand the process of disease at the most fundamental level as well as the other microbiological processes that he discovered, but he wanted that to deal with silk worms, anthrax in sheep and cattle, cholera in chickens, spoilage in milk, wine and vinegar, and rabies in people. The melding of those motives in the work of the mature Pasteur is so complete that you could not understand his science without knowing the extent to which he had considerations of use in mind. *The mature Pasteur – not the crystallographer at the dawn of his career, the man who took on the enigma of racemic* (tartaric) *acid at the Ecole Normale – embarked on a pure voyage of discovery* (emphasis mine). But the mature Pasteur never did a study that was not applied while he laid out a whole fresh branch of science'. I was struck by Stokes' assessment as I might have placed the young Pasteur's resolution of tartaric acid in the mid-19th century, the starting point of all of organic stereochemistry and structural biology, right at the top of the Bohr quadrant. By Stokes' reasoning Pasteur would have been an anomaly in a one-dimensional view of research, appearing discontinuously at two ends of a continuous spectrum. The Stokes Diagram resolves this, with Pasteur exemplifying the highest ideals of science, while simultaneously powering the most breathtakingly practical innovations. The Edison quadrant is, undoubtedly, the region of Stokes' space that is easiest to appreciate for the lay public. In considering the race towards a major invention, Stokes notes that 'Edison never allowed himself or those working with him . . . five minutes to consider the underlying side of the significance of what they were discovering in their headlong rush to commercial illumination. Edison himself one night heated up a filament in a vacuum and observed what is now known in American physics as Edison's effect because he wrote it down in his notebook. . . if he had tried to consider its more fundamental implications, he might have shared the Nobel prize with J. J. Thompson for discovering the electron, but he went right on' (Stokes, D., Completing the Bush Model: Pasteur's Quadrant. Talk at a conference 'Science: The Endless Frontier' 1945–1995, 9 December 1994; http://cspo.org/products/conferences/bush/stokes.pdf).

Where do some recent advances fall in the Stokes Diagram? The work leading to the transistor or magnetic resonance imaging must surely belong in Pasteur's quadrant. So too must the work of Boyer and Cohen, which ushered in the recombinant DNA revolution; clearly an outstanding example of 'use-inspired' basic research, despite the absence of recognition by Nobel committees. The discovery of the polymerase chain reaction (PCR), which allows DNA to be copied with spectacular efficiency, has revolutionized biotechnology and might rightfully appear in Edison's quadrant. The Stokes diagram allows scientists and analysts to reflect on the position of their favourite advances in science.

What of the unnamed quadrant which lies to bottom left of the Stokes diagram, diagonally related to Pasteur's quadrant. This is probably the most densely populated region of Stokes' 'two-dimensional' conceptual plane. This is where I believe most of us work, attempting to move away from the origin in all directions. A little reflection suggests that the quadrants in the Stokes' plane are not separated by insurmountable barriers; indeed a seamless transition from basic to applicable research is readily achievable. The limiting factors will, of course, be the imagination and creativity of individuals and their passion and commitment to their chosen areas of work. I am sure if my young interrogator had been allowed to frame a supplementary question he might have asked: 'Why have you not invented anything useful?'. This is a question that must often appear in the minds of policy makers, charged with the task of reconciling financial inputs with scientific and technological outcomes. The Stokes analysis was stated in somewhat different terms more recently by John Marburger, the Director of the US Office of Science and Technology Policy. He recalled J. Robert Oppenheimer's observation that society supports science for two 'disturbingly different' reasons and added that: 'Science is useful, and the pursuit of science is ennobling. Perhaps, those qualities are complementary in the sense of quantum physics – narrowing the focus on one makes the other more uncertain. If so, funding the state in which both are optimized is a worthy goal of science policy' (*Physics Today*, June 2006, pp. 38–42).

P. Balaram

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EDITORIAL

Morris Travers: Remembering an Institution Builder

Anniversaries at institutions are a time to look back at events long past and to remember with gratitude those who have built the structures we exploit today. Memories can fade with time and the documented historical record is often meagre. Unsurprisingly, the architects of institutions are sometimes relegated to the obscure recesses of collective institutional memory. As the Indian Institute of Science (IISc) is poised to enter its hundredth year, I have wondered about its first Director and attempted to imagine what it must have been like to embark on the unprecedented experiment of building a research institution in India, at the dawn of the twentieth century. Bangalore, a hundred years ago would have been a far cry from today's bustling metropolis. Research was not a common or well-understood activity in India. The turbulence and turmoil of the first half of the twentieth century still lay ahead. The Victorian era had just drawn to a close and Gandhi was yet to appear on the Indian scene. It is in this setting that the first Director, Morris Travers, began the task of building an institution that has weathered the tumultuous century that followed. Travers is not a well-known name in India. Even at the IISc, a lecture in his memory, marking his research interests in chemistry, was established only in 1990, over eight decades after he began the work of building an institution. Travers worked in Bangalore between 1906 and 1914, resigning in the midst of a growing controversy between him and members of the governing council of the Institute. His years in India must have been difficult, but the foundations he laid have held well against the ravages of time. The story of the genesis of IISc in the form of a magnificent endowment by Jamsetji Tata is well known. Tata's vision for India's future was unparalleled in its foresight. But visions need to be translated into reality; a task that often proves impossible. Burjorji Padshah, a devoted executor of the Tata vision, spent years in securing the acceptance of the proposal by Curzon's government, leading ultimately to the visit of William Ramsay towards the end of 1900. Several more hurdles were to be crossed before the Institute became a reality and Travers arrived in Bangalore, as 1906 drew to a close. The written records available at the Institute are pitifully small. Formal minutes of meetings, however well preserved, tell little of the story. In attempting to piece together a narrative that would capture a sense of the times, I was delighted to be handed a bundle of poorly kept letters, written by Travers to one of his successors, S. Bhagavantam in 1958/1959, when IISc marked the completion of fifty years. By this time, Travers was past eighty and nearing the end. His recollections of the events of the first years of the Institute are vivid and poignant.

Morris Williams Travers (1872–1961) was a chemist, a student of William Ramsay at the University College London. He was Ramsay's assistant in the famous experiments, which led to the isolation of the inert gases neon, krypton and xenon. These studies were carried out immediately after the Ramsay–Rayleigh discovery of argon and Ramsay's work on helium. The experimental skills of Morris Travers must have been formidable; the fractionation of liquid air yielded three new elements in a few weeks of work. The first British scientists to win Nobel Prizes were Rayleigh (Physics, 1904) and Ramsay (Chemistry, 1904). Travers appears to have been a meticulous documenter of events and experiments and an author of uncommon talent. In 1901 he published an *Experimental Study of Gases* (Macmillan, London); an original edition sits by my side as I write. This is a book that is remarkable for its experimental detail and I believe it is testimony to the care and devotion that Travers brought to his work. At 29, Travers had completed a major piece of work and published a scholarly treatise. Five years later, at a remarkably young age, he would begin building an institution, working in an environment that would bear no resemblance to his London laboratory. Travers authored a second book, *A Life of William Ramsay* (Edward Arnold, London, 1955), written between his 'eighty-first and eighty-third' birthdays. The Ramsay biography is a remarkable book; extraordinary in its detail and meticulous in its documentation, providing an insight into the author's character. Travers' account of Ramsay's life and work must stand as one of the classics in the genre of biographies of men of science. The author's attention to detail and his ability to make Ramsay come alive, speak of Travers' talents as a chronicler. It is this rare talent that one must bear in mind when reading his account of his days in India, written half a century after he began the work of building the Indian Institute of Science.

Travers did not complete an autobiography, but left behind at the University College London's Archives a typescript, notes and diaries. His biography of Ramsay contains a chapter entitled 'India'. It begins on an intriguing note: 'A reason for wishing to finish the work on the rare gases in July 1900, was that Ramsay had arranged to visit India in the autumn. As Travers sat at the balance on Monday, July 7th making weighings in determining the first density of pure neon, he gave him an outline of the proposal. They had seen little of one another the previous six weeks. A wealthy Parsee, Mr Jamsetjee N. Tata of Bombay, proposed to establish in India what he called an *Institute* or *University of Research*, said to be on the lines of the Johns Hopkins University of Baltimore. Ramsay asked Travers if, in the event of the scheme materializing he would consider going out to India as the first Principal. Travers replied at once that he would not; though he was very ill at the moment, he was confident that at no distant date he would obtain a chair in a British university institution, and he did not relish the idea of exile, though the salary offered was large' (p. 194). Travers presents an account of Ramsay's visit to India quoting his mentor, who said that it was 'like reading the index of a book, without time to read the book itself'. The young Travers had visions of a chair of chemistry in a British University. But circumstances were to force him to accept the task of building IISc as its first Director, undoubtedly attracted by the annual salary of £1800 and a promised pension.

Travers arrived in India towards the end of 1906. A notice in the journal *Science* (1906, **24**, 710) notes that he 'left Marseilles, on November 2, in the mail steamer *Victoria*, for Bangalore to take up his work as first director of the Indian Institute of Science'. He was then only 34 years old. In letters to his mother he describes vividly the sights, sounds and smells of India, a century ago. The notes available at the University College London Archives present a fascinating picture of an era long gone. Travers' first sight of the land destined to be the IISc campus follows a ride on horseback from the West End hotel in Bangalore. He recounts a tale of buying a horse 'from a native' for the then princely sum of Rs 250. In a letter written in 1959 to S. Bhagavantam he says: 'During the period 1906–1914, I gave myself to India and the Institute. I left England in November 1906 knowing little of Indian Education. The India Office had given me a copy of the Report of the Royal Commission on the Universities of India of 1902. I read it thinking it just rubbish; but when I got to India I learned that when Lord Curzon received the Report he summoned the Commissioners, told them what he thought of their Report, tore it across, stamped on it and dismissed them'. Travers worked against many odds in getting the Institute to a stage where students were admitted in 1911. In a letter addressed to the Registrar of IISc, A. G. Pai, in July 1954, Travers writes: 'When I first went to India one of the first things I did was to look into the question of housing and feeding students drawn from all over India. I came to the conclusion that I should have to have several messes. Then I was right, for when we opened I found that we had to have five. Then a Muslim student turned up, and as no mess would take him in, I had to make a one-man mess for him. I still wonder how fish-eating Bengalis get on with meat-eating Punjabis.'

Travers fought an unceasing battle with Padshah and Dorab Tata on the academic directions of the Institute. Padshah favoured starting subjects like archaeology and the humanities, while Dorab Tata had visions of bacteriology and a tropical diseases institute. Travers was sharply focused, realizing that resources, both human and material, would permit only a more limited approach. In many ways he set the agenda for the century that followed. His troubles with the Tatas were compounded by problems with the task of erecting the magnificent structure which symbolizes the Institute today. He fell afoul of the governing council and eventually resigned in 1914, forced out by opponents who converged from different directions. In his letter of 1959 he reflects on the end: 'My last visitor in June 1914 at Bangalore was Lord Willingdon. After going round the place he said to me: "I had no idea that there was anything like this in India". I said: "There is nothing like it in India; and nothing better in Great Britain"'. Travers involved himself in research, despite great odds, setting an example for his successors. He reflects: 'A particularly happy memory at Bangalore is the work in my laboratory during my last three years. Ramsay always said that our discoveries are our students'. Travers' side of the story is compelling, but his opponents prevailed. In 1954, he notes, with some regret: 'Spite exceeded wisdom'. He adds: 'I had achieved, so far as was possible, what I undertook when I left for India. But I had to accept as true "that it is in the nature and essential constitution of things, calumny and abuse are essential parts of triumph" (Edmund Burke)'.

An apt way to pay a tribute to Morris Travers would be to borrow the words used by the President of the Royal Swedish Academy in presenting William Ramsay in 1904. Travers triumphs with the noble gases and the establishment of the Institute could 'not have been acquired without great toil, being not merely a combination of fortunate circumstances but the result of a well planned persevering and tiresome work'.

P. Balaram

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EDITORIAL

Higher Education: Globalization and Expansion

'Globalization' is a word that has come to be used with increasing frequency over the last few years. Words and phrases have a way of dominating our thinking, despite conveying different meanings to different people, at various times. Winston Churchill, while visiting the United States in the aftermath of his postwar electoral defeat, famously declared that '. . . from Sttetin on the Baltic to Trieste on the Adriatic an Iron Curtain has descended across the continent'. Churchillian rhetoric has a resonant ring and soon thereafter the 'iron curtain' became the common descriptor for the divide between the distinct political and economic ideologies of the erstwhile Soviet Union and its allies on the one hand and the Western powers led by the United States on the other. The Berlin Wall came to symbolize in stark, physical terms the political divide. Up to the late 1980s ideological barriers seemed to obstruct collaboration across national boundaries. Churchill's metaphorical 'iron curtain' vanished rather suddenly by the early 1990s; the spontaneous demolition of the wall in Berlin was a dramatic image of the times. The breaching of political barriers led seamlessly to an integration of economic activity; trade restrictions were suddenly undesirable. Open markets and free trade became the pillars of globalization. As a cloistered academic scientist, my first brushes with the new world order came when the number of foreign delegations visiting the campus of the Indian Institute of Science began to rise in dramatic fashion. Visiting groups included politicians, administrators and academics, all of whom appeared to seek collaboration and expansion of their activities on a global scale. Delegations come bearing gifts and I have been inundated with ties that I do not wear. But, one group appeared instinctively to sense that I was bemused by their stated objective of 'exploiting global markets in higher education'. They presented me a copy of Thomas Friedman's bestseller *The World is Flat* (Farrar, Straus and Giroux, 2005), which is provocatively subtitled 'A brief history of the twenty-first century'. To me the 21st century seems too young to have a history. This is not a book that I would normally have read. It is a book that I would have glanced through, idly, while waiting for a delayed flight. However, there is something seductive about a hardbound book that is received as a gift; it compels the recipient to browse. I turned instinctively to the index to look for the word 'globalization' and to my surprise it was missing. Only then did I realize that for Friedman globalization was an all pervasive term permeating (and indeed flattening) the world. The word had entered everyday usage and was no longer indexable. My ignorance lay further exposed when I turned to the 'Acknowledgements' and realized that Friedman's earlier book, *The Lexus and the Olive Tree* (1999) was indeed on globalization. I did find several definitions of the word, but they all seemed context dependent; none seemed to be completely applicable to the rapidly changing face of academia. I like words that have a certain ring to them; in Friedman's 2005 book I found one that I disliked immediately. 'Glocalize' is a word that Friedman advances to describe the ability of cultures to absorb 'foreign ideas and best practices' easily melding them with local traditions. I hope this word, which really describes a pragmatic approach to progress, dies a quiet death.

Even as I have struggled with the unfamiliar terrain of Friedman's 'flat world', two more books arrived on my desk. The first, an edited volume entitled *The Globalization of Higher Education* (Weber, L. E. and Duderstadt, J. J., Economica, London, 2008) is the result of a colloquium that 'brought together university leaders from around the world in Glion above Montreux, Switzerland to consider the challenges, opportunities and responsibilities presented to higher education by the emerging global knowledge-driven economy' (p. xi). I have always wondered about phrases like the 'knowledge economy'. Is it not true that economic progress and knowledge and its application have always been linked, right through history? The 21st century will, of course, be no exception. Yet, there is a comforting feeling that future progress is in some way more organically linked to the creation of knowledge than was true in the past. For university leaders, this reiteration of the importance of research and innovation must surely be heartening. The second volume that I received was slimmer, glossier and closer to home. This was a report produced by the consulting firm of Ernst and Young, entitled *Globalizing Higher Education in India* (March 2008), produced for an event, EDGE-2008, held in Delhi; the acronymn expands to 'Emerging

Directions in Global Education'. I have always been struck by the ability of 'consultants' to produce wonderfully attractive reports on complex themes. In scientific research, producing a critical review of the literature even in one's own area of expertise can be a formidable task, requiring a knowledge of the field that takes years to acquire. I suspect that the products of management schools, who are found in profusion within consulting firms, are a cleverer and bolder breed. They are able to quickly capture the essence of an area and have the ability to weave statistics into a skillfully produced presentation. Armed with these two books I could now begin to see the flat and globalized world a little less hazily.

The Glion colloquium begins with an extremely readable assessment of the meaning of globalization in the world of higher education. The economist Deepak Nayyar, a former Vice-Chancellor of Delhi University, is emphatic: 'The spread of markets and the momentum of globalization, during the past two decades, have transformed the world of higher education almost beyond recognition. Market forces, driven by the threat of competition or the lure of profit have led to the emergence of higher education as business'. It is this transformation of education into a commercial activity that has had the greatest impact on the academic scene in India. Foreign universities regularly send teams to assess the potential for off-shore ventures. India's vast and untapped reservoir of youth presents a most desirable market for institutions, that can now 'sell' higher education. Until recently, the sector of higher education was largely publicly supported and subsidized. A transformation is imminent and has already taken place in areas like management. The inroads into the fields of medicine and engineering at undergraduate level by private institutions has been happening for some time. Postgraduate and research degrees will undoubtedly be areas where foreign institutions will have definite interest. Government regulations can impede the entry of foreign institutions, but it is debatable as to whether this is a sustainable policy. Nayyar reflects on the dangers we face: 'What can we lose? First, markets should not decide on academic curricula or research agenda. . . Teaching and research cannot be simply about use-value and exchange-value. Second, management methods of business are not appropriate for universities. The objectives cannot be efficiency or profits'. He goes on to worry that 'markets and commercialization' can erode the 'values and ethics in the university community'. To some extent Nayyar's fears are already relevant in research institutions, where commercial interests can direct research goals and the distinctions between publicly funded facilities and private interests begin to blur.

Even as the shadow of globalization clouds the higher education scene in India, we have the prospect of an unprecedented expansion of universities and institutions for science and engineering. In the last sixty years the number of universities has increased by a factor of nearly 20, going from exactly 20 in 1947–48 to about 370 last year. The new institutions will add to this number and the fact that they are centrally supported may provide them a more robust platform for rapid expansion. The major limiting factor in the growth of the sector of research and higher education is the extreme shortage of well trained faculty to man these institutions. The growth of private institutions will pose severe problems for the new public universities, as both sectors compete to hire from a limited faculty pool. At some time in the future, the entry of foreign institutions will pose a major challenge for our public universities. The expansion of the higher education system in India is both necessary and imminent. Managing this expansion in a globalizing environment may require new strategies, within the framework of coherent and well-thought out public policy.

P. Balaram

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EDITORIAL

Writers and Readers

Writing about science and scientists requires two distinct skills; knowledge and understanding of science and an ability to communicate in a language that readers appreciate. India boasts of very few professional science writers, capable of popularizing and communicating the happenings in the world of science. Some years ago the Ramaseshan fellowships were instituted at *Current Science*, in the hope that support provided for aspiring science writers would result in contributions, that would improve the readability of the News sections of the journal. While the response to the advertisement of the fellowships was encouraging, my colleagues and I quickly discovered that the task of finding science students, with writing skills and a desire for a career in science journalism, was going to be very unrewarding. In attempting to foster a new breed of budding science writers, I have often wondered which attribute is primary; knowledge of science or skills in writing. Most applicants for the fellowships appeared too specialized, rarely able to muster interest in any topic outside their narrow areas of expertise. Many seemed to have little idea of the rich history of the fields they were studying. If a broad interest in science seemed rare in the group of aspiring science writers whom we met, writing skills seemed even rarer. In interviewing candidate after candidate I realized that reading may have become a lost art. While books were almost never 'read' by the group of students we met, surprisingly even popular journals and magazines did not seem to be worthy of even a cursory glance.

Writers must, of necessity, be readers first. In principle a course on science writing or communication must teach budding writers what to read. The attention span of most readers is severely limited and any reading course would be best advised to prescribe material that is restricted in length. In thinking about a suggested reading list I came to the unsurprising conclusion that short essays on science may be the best place to begin. Quite often the most interesting essays are camouflaged as book reviews. For avid readers, who are hard pressed for time, a review may help in deciding whether a book is worth pursuing. The difficulties (and expense) of getting one's hands on some titles in India cannot be underestimated, making well-written reviews invaluable. Reviewers who are knowledgeable about the subject and its history can sometimes provide insights that are lacking in the book itself. I am addicted to book reviews and it is to these pages that I turn in journals like *Nature* and *Science*, after a quick glance at the contents page reassures me that no spectacular breakthrough has been reported in my areas of interest. A review that is really an essay in disguise, provides the author with a wonderful opportunity to reflect on a field, unconstrained by the precise details of the book being reviewed. Book reviews fall into many categories. There are dull cataloguers who list the chapters and always recommend the volumes to libraries and undiscerning buyers. Then there are the professional critics, who take their jobs rather seriously, working on the assumption that reviewers must find fault with everything they read. Finally, there are the essayists, writers waiting for an opportunity to expand and digress; the book at hand only providing a catalyst for creative expression.

In this genre of reviewers, writing about science and scientists, few can match the rare talent of Max Perutz. An Austrian by birth and an Englishman in every other way, Perutz reviewed a remarkable range of books in his later years. He was one of the dominant figures in the revolution in structural and molecular biology that took place in the 1950s and 1960s. His work paved the way for our present understanding of protein structure and function. He spent the most fruitful years of his career at Cambridge, working till the very end. He was uniquely positioned to view and describe science and scientists. He was a prodigious reviewer of books, sharing his insights and experiences with readers in simple, yet elegant prose. His collection of essays *I Wish I'd Made You Angry Earlier* (Oxford University Press, 1998) must be required reading for every budding science writer. This collection of essays contains several book reviews that featured in *The New York Review of Books*, *London Review of Books* and *Nature*. The book derives its title from an essay in *New Scientist* (23 February 1987), which describes his experimental confirmation of the alpha-helical structure of proteins, proposed by Pauling and Corey in 1951. A few months earlier Lawrence Bragg, John Kendrew and Max Perutz had published a paper in the *Proceedings of the Royal Society, London* (1950, **A203**, 321) on the structure of polypeptides. Bragg had received the Nobel Prize in 1915 and Perutz and Kendrew would receive it in 1962. Hindsight would show that every structure in this paper was probably wrong. Perutz writes that confirma-

tion of the Pauling–Corey insight required one simple but, crucial experiment, which he performed immediately. The result proved Pauling right. When Perutz 'stormed into Bragg's office' to show him his X-ray diffraction picture, Bragg asks him, 'what made you think of this experiment?'. Perutz replies 'that the idea was sparked off by my fury over having missed building that beautiful structure myself'. Bragg's prompt response, 'I wish I had made you angry earlier!' is used decades later as the title of Perutz's fascinating collection of essays.

Perutz ranges far and wide. In reviewing two books on Fritz Haber and his wife Clara Immerwahr, he highlights the dilemma of science, valuable and destructive at the same time. Haber's work on the synthesis of ammonia and the consequent growth of the fertilizer industry has sometimes been hailed as the harbinger of the first agricultural revolution of the 20th century. But Haber's work also signalled the beginning of chemical warfare research in a modern context, field tested in the trenches of the First World War and resulting, almost inevitably, in the use of Zyklon B for mass murder in Hitler's Germany. Perutz has a somber assessment of Haber: 'By a terrible irony of fate it was his apparently most beneficent invention, the synthesis of ammonia, which has also harmed the world immeasurably'. Perutz can be scathing in a measured way, as in his review of a book that attempts to 'deconstruct' Louis Pasteur (Geison, G. L., *The Private Science of Louis Pasteur*, Princeton University Press, 1995). He admonishes the author: 'Toppling great men from their pedestals, sometimes on the slenderest of evidence, has become a fashionable and lucrative industry, and a safe one, since they cannot sue because they are dead. Geison is in good company, but he, rather than Pasteur seems to me guilty of unethical and unsavoury conduct when he burrows through Pasteur's notebooks for scraps of supposed wrongdoing and then inflates these out of all proportion, in order to drag Pasteur down'. Perutz proceeds to reach a diametrically opposite conclusion, reflecting the fairness that he always displayed in his writing: 'Pasteur may have been domineering, intolerant, pugnacious, and, in his later years, a hypochondriac who searched every slice of bread for bacteria before eating it; but he was courageous, compassionate, and honest and his scientific achievements, which have much reduced human suffering, make him one of the greatest benefactors of mankind' (p. 129). Perutz is often at his best when he reflects on many of science's best known names. He ends his review of the autobiography of Hans Krebs agreeing with Krebs' view of research, which echoes 'Noel Coward's saying that work is fun, there is no fun like work'. He saw the double helical structure of DNA emerge, even as he struggled with haemoglobin. He ends an essay entitled 'How the Secret of Life was Discovered' by noting that 'like Leonardo, Crick and Watson often achieved the most when they seemed to be working the least. They did an immense amount of hard work, studying while hidden away, often at night, but when you saw them they were most likely engaged in argument and apparently idle. This was their way of attacking a problem that could be solved only by a tremendous leap of the imagination, supported by profound knowledge. Imagination comes first in both artistic and scientific creations. But in science nature always looks over your shoulder.' Perutz then paraphrases Churchill in concluding, 'In science you don't need to be polite, you only have to be right'. In his writings Perutz turns out to be a man who was both polite and right. There are many gems strewn throughout his essays. In concluding his reflections on Rita Levi-Montalcini's autobiography *In Praise of Imperfection*, Perutz recalls Paul Ehrlich's dictum that 'success in research needs four G's: Glück, Geduld, Geschick and Geld (luck, patience, skill and money)'. He notes wryly that Levi-Montalcini 'had the first three in good measure and needed little of the fourth' (p. 303).

In introducing the Spring Books Supplement of *Nature* many years ago (1981, **290**, 633), John Maddox asks 'What are book reviews for?'. He goes on to conclude that 'the scientific community, any intellectual community, has reason to be grateful to reviewers'. The requirements of a book review, according to Maddox, is that they 'should be essays in sympathetic understanding, marked sometimes with sorrow, rarely with anger'. Book reviews are not always benign and sympathetic and Maddox draws attention to an interesting exception. This is Dorothy Hodgkin's review of Maurice Goldsmith's biography of J. D. Bernal, arguably one of the most influential and interesting figures of 20th century science. Hodgkin, a student of Bernal, who in Perutz's words was 'a great chemist; a saintly, gentle and tolerant lover of people. . .', is devastating in her critique of the Bernal biography (Goldsmith, M., *Sage: A Life of J. D. Bernal*, Hutchinson, 1980) (*Nature*, 1981, **289**, 99). Maddox defends Goldsmith noting 'that there are some occasions on which even bad books are better than no books. . . . And books – or good books – can survive from one decade to another'. I did not find a review of Goldsmith's book by Pertuz, who also learnt the tools of the trade from Bernal. But one might guess that he would have skirted the book itself, but dwelt on the man, whom he said turned the 'dingy quarters' of the Crystallographic Laboratory at Cambridge 'into a fairy castle' by his brilliance.

In the preface to his collection of essays Perutz says disarmingly, 'when I read books, I jot down any wise sayings which appeal to me. I keep them in my Commonplace Book, a name that goes back to antiquity. . .' He reproduces his collection at the end of the book and I cannot resist using one attributed to Groucho Marx: 'I was so long writing my review that I never got around to reading the book'. In admiring Perutz for his craftsmanship, I came to the inescapable conclusion that writers must read.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Resilience and Imagination

Producing a fortnightly editorial column is a task that can be both liberating and stifling. It is liberating when the author can freely express an opinion, without fear of objections from referees and editors. It is stifling when tight deadlines cast iron constraints on word limits and inviolable production schedules sharply define the time available to draft an essay. Critics abound, who are quick to spot repetition, pounce on factual errors and gleefully highlight mistakes of every kind. The task is uplifting when one encounters the sympathetic reader, who draws attention to related writings. The most formidable part of this task is to decide on a subject to write about. Columns must be relevant and topical, informative and useful, criteria that cannot always be met. Once printed, even the most hastily written composition is exposed to analysis and scrutiny. In searching for subjects, help often arrives from readers who are kind enough to send material that appears interesting. Almost always, these are topics that might otherwise have escaped attention. In struggling to decide on a topic for this column, help arrived unexpectedly. Two colleagues, independently, sent me (electronically, of course) the text of J. K. Rowling's Commencement Address at Harvard, delivered a few weeks ago. The title was compelling: 'The Fringe Benefits of Failure, and the Importance of Imagination'. The speaker appeared to have been placed in an interesting setting; the creator of Harry Potter asked to provide sage counsel to the graduating class of, arguably, the world's most prestigious university. If the past is any guide, one might conclude that many members of Harvard's graduating class would in future influence their surroundings in a very significant way. My interest was piqued by the fact that decades ago I had unsuccessfully attempted to enter Harvard for a doctoral program. I did however, stand on the fringes of a Harvard Commencement, as an onlooker thirty five years ago, listening to an address that I have now forgotten. The Commencement Address is what we term as a Convocation Address; an occasion for a well-known speaker to reflect on an important subject. Harvard's most famous and influential Commencement speech is undoubtedly the one delivered by George Marshall in 1947, when he announced the Plan, that now bears his name, for the reconstruction of the economy of post-War Europe. Marshall's speech in 1947 begins with words that must have found some resonance in India in that remarkable year: '. . . I need not tell you that the world situation is very serious . . . I think one difficulty is that the problem is one of such enormous complexity that the very mass of facts presented to the public by the press and the radio make it exceedingly difficult for the man on the street to reach a clear appraisement of the situation'.

Heads of state and government, Nobel laureates and some of the world's best known names in the corporate world have addressed the Harvard gatherings. A cursory search on the Internet produced addresses by figures as diverse as Bill Gates and Alexander Solzhenitsyn. Rowling seemed an unusual choice, but on reflection, an extraordinarily imaginative one. Children, who have experienced the Potter magic would surely endorse the choice, many of them on the verge of entering college and some probably present in the audience that heard Rowling. The Potter series has captivated an entire generation of young readers, although I must confess that as an unimaginative adult I have never really read a Rowling book in its entirety. The Rowling story is now the stuff of legend. The rise of a poor, single mother in her thirties to the position of the most successful author of her time, in the short span of a few years is an inspirational story. When Rowling reflects on her experiences, before a Harvard audience, she eloquently touches on issues that must strike a chord in most of us: 'Half a lifetime ago, I was striking an uneasy balance between the ambition I had for myself and what those closest to me expected of me'. She notes that her parents hoped she would study for a 'vocational degree'. Instead she 'scuttled off down the Classics corridor'. Her parents, Rowling reflects, 'would have been hard put to name' a subject 'less useful than Greek mythology when it came to securing the keys to an executive bathroom'. Rowling's parents wished for her a future that millions upon millions of poor parents across the world wish for their children, a lifetime free of poverty. In Rowling's words: 'They had been poor themselves, and I have since been poor and I quite agree with them that it is not an ennobling experience. Poverty entails fear, and stress, and sometimes depression . . . poverty itself is romanticised only by fools'. But, in an intriguing insight that seemed especially appropriate for the elite graduating class, Rowling notes that what she feared most

'was not poverty but failure'. Rowling is at her most eloquent when she says that 'talent and intelligence never yet inoculated anyone against the caprice of the Fates', reminding her Harvard audience that they might well 'be driven by a fear of failure quite as much as a desire for success'. She turns to her main theme in declaring that 'failure gave me an inner security that I had never attained by passing examinations. Failure taught me things about myself that I could have learned no other way'. Many successful individuals would echo Rowling when she says: 'The knowledge that you have emerged wiser and stronger from setbacks means that you are, ever after, secure in your ability to survive'. Rowling is more qualified than most to extol the virtues of imagination: 'Imagination is not only the uniquely human capacity to envision that which is not, and therefore the fount of all invention and innovation. In its arguably most transformative and revelatory capacity, it is the power that enables us to empathise with humans whose experiences we have never shared'. Rowling describes her period of work at an Amnesty International office as 'one of the greatest transformative experiences'. In reading Rowling, I was struck by her characterization of imagination as the bedrock upon which all of invention and innovation rest. In discussions of science, technology and economic development, the two words most often used are 'invention' and 'innovation'. It may be useful to reflect on the importance of 'imagination'. The Rowling story of a rise from rags to riches and fame is likely to be retold many times in future. What is the quality that really makes such transformations possible? Undoubtedly, it is an inner toughness, a unique resilience that learns quickly from failure, turning defeat into victory. Failure is a common phenomenon in research, but this is a condition that is generally not recognized. In refusing to acknowledge failure we do not learn from it; borrowing Rowling's phrase, we do not reap the 'fringe benefits of failure'.

In advancing 'resilience' as a key element of success in difficult environments, the story of Mario Capecchi merits retelling. Capecchi shared the 2007 Nobel Prize in Physiology or Medicine for his work on gene targeting, which has paved the way for disrupting genes in animals, resulting in the powerful technology of 'knockouts', that permits biologists to probe gene function in living organisms. Indeed the field has advanced to a stage where 'approximately a half of all genes in the mouse genome (about 22,400 genes)' have been mutated (Deng, C. D., *Int. J. Biol. Sci.*, 2007, **3**, 417). Capecchi's scientific achievement has been formidable; the impact of his work is now felt across a broad front in biology and biomedical research. But for the general reader, Capecchi's early life vividly demonstrates the role of resilience in shaping futures. In a lecture delivered in 1996, while accepting the Kyoto Prize, Capecchi details his turbulent early years. The title of his lecture 'The Making of a Scientist' (available on the Howard Hughes Medical Institute's website) should attract scientists, young and old, aspiring and established. In a language that appears, at first glance, completely different, Capecchi conveys a message that finds its echoes in Rowling's Harvard address: 'I have no formula for generating creative scientists. To the contrary, my thesis will be that such a formula may not exist. My skepticism comes from a deep-rooted prejudice that creativity in science, or in any other discipline, may require the abrasive juxtaposition of unique sets of life experiences that are too complex to preorchestrate. It is in this spirit that I will share with you my own experiences as a tribute to such stochastic, chaotic influences. The only general interest of this story is that it exemplifies the antithesis of a nurturing environment, which all of us deeply want to believe is a conducive prerequisite for fostering thoughtful, creative human beings'. Capecchi's narrative is compelling. Born in 1937 in Italy, at a time when Fascism and Communism were beginning to be dominant, to parents as diverse as can be, he was soon (like Rowling) the child of a single mother. She was a poet working, with a group opposed to the Nazis, resulting in her arrest and internment in Dachau in early 1941. For a while the child was looked after by a peasant family who were given the proceeds of the sale of Capecchi's mother's meagre possessions. But by 1942 when the 'money ran out' Capecchi, as a child of 4½, set off on his own: 'I headed south, sometimes living in the streets, sometimes joining gangs of other homeless children, sometimes living in orphanages and most of the time being hungry. My recollections of those four years are vivid but not continuous, rather like a series of snapshots. Some of them are brutal beyond description, others more palatable'. The Capecchi story has a happy ending, with his mother surviving until the liberation of Dachau in 1945 and miraculously finding him on his ninth birthday. Emigration to America was the turning point. In Capecchi's words: 'I was expecting to see roads paved with gold in America. I found much more: an opportunity'. In looking back at his early years Capecchi says: 'I marvel at the resilience of the child'. He concludes that the 'genetic and environmental factors that contribute to such talents as creativity are too complex for us to predict. In the absence of such wisdom, our only course is to provide all of our children with ample opportunity to pursue their passions and their dreams'.

The Rowling and Capecchi stories are inspirational. They emphasise the importance of concentration and determination; resilience and imagination.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Ending an Innings

Retirement is not always an attractive prospect. There are some who approach it with equanimity, while others struggle to come to terms with the inevitable. Sportsmen face this choice at a relatively early age; youth and physical fitness are essential prerequisites for success. Indian cricket has witnessed some dramatic upheavals over the last year or so and the pressures on the big three, Rahul Dravid, Sachin Tendulkar and Saurav Ganguly to 'voluntarily' retire have been evident. A graceful exist draws applause; a forced retirement can leave scars that never heal. For sportsmen, retirement from the major calling comes early in life and alternate careers must be carefully considered, even when performances are at their peak. For most others, who work in a less frenzied environment retirement looms at a specific point in time; politics is, of course, a profession that is an exception, clearly because it does not provide even minimal security during the evolution of a career. Unlike many other fields of work, scientists appear to struggle to a significantly greater extent with the notion of retirement. Having spent an entire career working on subjects they like and enjoy, retirement seems to be a rude and disagreeable way of ending a deeply personal relationship with their places of work. Unlike writers or artists, scientists can almost never work from home. They need laboratories, equipment, expensive journals, co-workers, and most importantly, a community to interact with, in a highly structured environment. In the best of institutions, many researchers and academics enjoy a remarkable degree of freedom to pursue their interests; always getting paid and rewarded for doing what they like best. There can be no better career than one where a pet hobby can be relentlessly pursued, even while a living is earned. Successful scientists, who have earned a measure of recognition from their peers, are always loathe to retire; unhappy that age suddenly debars them from activities they enjoy. Some countries, notably the United States, have legislated against 'age discrimination', thereby eliminating mandatory retirement in academia. This is indeed possible in a system where every facility extended to faculty in an academic institution can be based on a hard-headed internal assessment of performance in teaching and research. Laboratory space and salaries are the two most important instruments of administrative coercion available to managers of institutions. Attractive retirement benefits can also serve as an inducement for early departure.

The demographics of the developed countries, the West and Japan, have been changing rapidly. The aging of societies in these countries is evident. Countries like India and China, which boast of reservoirs of youth as an intrinsic strength, have begun to realize that enhanced life expectancy and lowered rates of population growth will begin to tilt the balance later in this country. Aging cannot be arrested in individuals. Neither can collective aging in modern societies be ignored. Some European countries already face the spectre of a large proportion of the population living off state pensions, after retiring from the rolls of productive employment. In these situations it would seem important, indeed imperative, to ensure that people work as long as they can. Nevertheless, it is in Europe that mandatory retirement is enforced and voluntary retirement appears attractive. For many years now, European (a term that must include the British) scientists have migrated westwards, as they approach the age of retirement, which varies between 65 and 68 years. North American universities have greatly benefited from this 'mature' brain drain; the senior expatriates often adding substantially to the academic profile of the institutions that provide them shelter. There have also been some celebrated cases of Indian scientists, who have accepted lifelines thrown to them from American institutions, at late stages of their careers. The interesting differences between European and American approaches to retirement in academia are highlighted by Peter Lawrence in a recent Commentary in *Nature* (2008, **453**, 588).

Lawrence, a developmental biologist at Cambridge, titles his essay 'Retiring retirement' and opens with the quote 'The evening's the best part of the day' (Kazuo Ishiguro). He begins his attack on mandatory retirement citing the examples of famous British biologists who moved to America; Oliver Smithies, winner of last year's Nobel Prize for physiology or medicine, and the now well-known examples of Francis Crick and Sidney Brenner. Lawrence bolsters his argument quoting, approvingly, the German President Horst Kohler: 'We could achieve much more if we allowed the curiosity and impulsiveness of youth to be tempered by the wisdom and the inner calm of the old'. Critics of the Lawrence thesis

will undoubtedly be quick to point out that 'wisdom and inner calm' are not necessarily attributes that appear with age. Lawrence notes that 'thanks to pressure from the European Union' the situation is slowly changing in the UK. Academics can now apply for extensions and he cites the example of Cambridge where 50% of the staff ask to be continued. To Lawrence this reflects a 'high level of job satisfaction'. The scientists whom Lawrence quotes and holds up as examples of highly productive individuals, even at an advanced age, are some of the most illustrious names in their field. Who can argue with Sydney Brenner when he says that 'I know many 35-year old scientists who should be retired and some 70-year olds who are the best postdocs you will ever find'. There is an oft advanced counter to Lawrence's plea to retire retirement. Keeping people in employment indefinitely will inevitably limit the opportunities for the young. While this may be less of a concern in aging societies, in most countries the abolition of 'age discrimination' can only be approached with great caution. Lawrence realizes that his views run counter to accepted wisdom and takes the opposition head on by quoting Anton Chekhov (*Uncle Vanya*): 'For twenty-five years he's been lecturing and writing about things that any intelligent person already knows and no stupid person cares to know . . . which means that for twenty-five years he's been keeping somebody else out of a job'. To Lawrence the tendency to 'stereotype individuals' and to 'assess people as groups' lies at the heart of the problem. Clearly, the principle that all men (and women) are equal cannot apply when performance is judged; by the time mandatory retirement age approaches the differences between individuals can be exceptionally large. The cycle of retirements and recruitments permits academic institutions to maintain an upward slope of performance, if the bar is constantly raised with every succeeding generation. If high performers are abruptly retired, institutions may temporarily witness a drop in research productivity, which can be disconcerting. For the most highly motivated individuals relocation to countries where retirement is not mandatory appears an uncomfortable, but viable, option.

In India the age of retirement in central academic institutions has been recently raised to 65, a jump of three years. In some institutions 'reemployment' can be considered for a further five years. This move came in the wake of the expansion of the higher education system, ostensibly to address the shortage of teaching faculty. Inevitably, the increase in retirement age will spread to research institutions, although the state universities may not follow suit. The absence of an 'earned tenure' system in India means that most newly appointed faculty at our institutions are 'permanent' members. Formal contracts are rarely enforced and there is no real connection between performance and continuation. As a consequence, when retirement approaches there are no accepted mechanisms for discrimination between performers and nonperformances. In many publicly funded institutions the most valuable perk of a 'government job' is housing. Increases in retirement ages immediately affect the ability to accommodate new individuals. Despite the many attempts to promote off campus housing, the situation in our major cities acts as an inhibitory factor. Laboratory and office space is another difficult issue. Paradoxically, it is easier to continue members who do little and ask for little. In an environment where intellectual attainments are not valued and administrative influence is prized, extensions and facilities are often not extended to the most valuable members of an academic community. To some extent the inability of universities to sometimes retain the most academic of their faculty has been responsible for the gradual erosion in intellectual values. India may not be able to 'retire retirement' but we might do well to reflect on Lawrence's essay and ask: 'How can we exploit the talents and abilities of the most senior and able members of our community'. Several schemes introduced by agencies and academies now permit formal association of retired researchers with their parent institutions. In most cases the association is a limited administrative arrangement; there is little or no academic involvement with the surroundings. It is clear that only extraordinary and exceptional individuals can maintain a high degree of intellectual productivity, with advancing age. For the rest, a substantial majority, retirement can provide a release from the constraints of a formal commitment to an institution. If our institutions demand more from their academic faculty, retirement may indeed provide an avenue to shed the shackles of regular employment. Mandatory retirement is unlikely to disappear from the academic scene in India. Our institutions may even benefit by making voluntary exit more attractive. This would permit individuals to make informed decisions and explore alternate choices of careers. An editorial comment that precedes the *Naturejobs* section in the issue of the journal that carries Lawrence's commentary notes: 'But in the end, perhaps the only way is to let each individual decide; should I continue to trek to the lab or should I be content to let the next generation take up the mantle' (p. 693). Ending an innings is not easy, especially if the decision is voluntary.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Images and Icons: Chemistry, Physics and the Garden of Mendeleev

Scientific journals are no longer as staid and conservative as they used to be. Brightly illustrated covers, colourful figures and cartoons can be found on their pages. One feature which has always interested me is the practice of having photographs of authors and short, sometimes irreverent biographical sketches. Authors suddenly seem to be made of flesh and blood; a pleasing change for editors, for whom authors are sometimes disembodied and argumentative correspondents. While reading a recent article entitled 'What Einstein meant when he said God does not play dice. . .' (Natarajan, V., *Resonance*, 2008, **13**, 655), I was struck by a sentence in the biographical sketch of the author, which stated that 'a picture of Einstein hangs above his chair'. The author, a young colleague of mine, was clearly proclaiming his commitment to physics and one of the subject's most enduring icons. In the article, the use of the word 'God' is analysed in the context of the author's characterization of Einstein as an atheist. As I read the article it seemed to me that the assessment that Einstein's great contributions stemmed 'from his belief in precise mathematical laws that govern the natural world' summarized in every way the philosophy of physics. Einstein, in some sense, was the prophet of this new religion. For physicists and indeed for almost every educated person, the portrait of Einstein that adorns the walls of many scientific institutions is instantly recognizable; an enduring symbol of the power of physics. Do chemistry and biology have any iconic images that can be proudly flaunted, in order to announce an allegiance to a discipline. Even as I idly thought about chemistry's icons, a book arrived on my table. My task was to find a reviewer, but a quick glance through the book told me that my search for chemistry's iconic image was at an end. The book *The Periodic Table: Its Story and Its Significance* (Scerri, E. R., Oxford University Press, 2007) appeared on the 100th anniversary of the death of Dmitri Mendeleev (1834–1907). In the introduction to the book I found the sentences that I was looking for: 'The periodic table of the elements is one of the most powerful icons in science: A single document that captures the essence of chemistry in an elegant pattern. Indeed, nothing quite like it exists in biology or physics, or any other branch of science, for that matter. One sees periodic tables everywhere: in industrial labs, workshops, academic labs and of course, lecture halls' (p. xiii). Glancing through Scerri's book, I was reminded of the large, colourful and sometimes faded periodic tables that adorned the college classrooms, where I first learnt chemistry many decades ago. They must still be there, hanging disregarded and forlorn, in many chemistry lecture halls across the world. Scerri opens his book with a quotation from *The Elements* by J. Emsley: 'As long as chemistry is studied there will be a periodic table. And even if someday we communicate with another part of the universe, we can be sure that one thing that both cultures will have in common is an ordered system of the elements that will be instantly recognizable by both intelligent life forms'. Scerri's book is scholarly and extremely well documented and, in large measure, fulfills its stated objective of establishing 'that one of the best ways to explore the relationship between chemistry and modern physics is to consider the status of the periodic system' (p. xxii).

The early history of chemistry is set in 18th and 19th century Europe, a continent famous for its wars and the cradle for modern science. Two characters are pre-eminent in the construction of the periodic table, Julius Lothar Meyer and Dmitri Ivanovich Mendeleev. As often happens, both men converged on the same result; that the known elements could be so arranged as to reveal a periodicity of properties. The controversies for priority were eventually resolved, with the general acceptance of Mendeleev as the main architect of the Periodic Table, as we know it today. Reading Scerri's account of this controversy reminded me that great insights sometimes occur simultaneously; Darwin and Wallace in biology and Schrödinger and Heisenberg in quantum physics. Much of the credit for tirelessly championing the periodic table must go to Mendeleev, who not only defended his creation but also continually worked for its elaboration. His work began around 1865 as an 'attempt to systematize inorganic chemistry'. It reached its climax in 1869 when he produced the first table. Cannizzaro's atomic weights provided his first parameter for classifications, leading to the arrangement of elements in horizontal rows. Scerri notes that February 17, 1869 is 'the date of the famous first table he produced'. Classification and organization were recurrent themes of 18th and 19th century science; biology's most famous names Linnaeus and Darwin were great observers and organizers of the objects they studied. Mendeleev was born in Siberia and did his famous work

at the University in St. Petersburg, a city renamed as Leningrad after the success of the Bolshevik Revolution, but restored its original name in more recent times. Pictures of Mendeleev reveal a bearded visage that could only have stepped out from the pages of Tolstoy or Dostoevsky. There is a certain character that is uniquely Russian, fashioned by harsh winters and harsh regimes. Scerri's assessment of Mendeleev is one that must gladden the hearts of chemists: 'His name is invariably and justifiably connected with the periodic system, to the same extent perhaps as Darwin's name is synonymous with the theory of evolution and Einstein's with the theory of relativity'.

Mendeleev's ideas of periodicity acquired widespread, albeit slow, acceptance in the years following his original proposal. In 1889, Mendeleev reviewed the status of the Periodic Table, while delivering the Faraday Lecture at the Royal Institution. By then the believers outnumbered the critics. His words introducing his own work are an eloquent statement of the importance of incontrovertible facts and generalizations in advancing science. Indeed two of the most celebrated advances of the 19th century, Darwin's evolutionary synthesis and Mendeleev's periodic table symbolize the critical importance of organizing a vast body of known facts, as an essential prerequisite for a major conceptual advance. Mendeleev uses a biological metaphor: 'But as the shade of the leaves and roots of living plants, together with the relics of a decayed vegetation, favour the growth of the seedling and serve to promote its luxurious development, in like manner sound generalizations – together with the relics of those which have proved to be untenable – promote scientific productivity, and ensure the luxurious growth of science. . . (*J. Chem. Soc.*, 1889, **55**, 634). He recognizes that critics abound but provides a stout defence: 'We still may hear the voices of its opponents; they enjoy perfect freedom, but vainly will their voices rise so long as they do not use the language of demonstrated facts'. Over a century after Mendeleev's death, chemistry and indeed all of science has grown in a manner that the very vastness of available facts intimidates new entrants to any field, who may have a talent for empirical generalizations.

In thinking about the virtues of empiricism, I was pleasantly surprised to receive an article entitled 'The End of Theory: The Data Deluge Makes the Scientific Method Obsolete' (Anderson, C., *The Wired Magazine*, 23 June 2008). This brief editorial essay begins in a provocative fashion with a quote: 'All models are wrong, but some are useful'; tempting readers to go further. The summary of the computer revolution is compelling: 'Sixty years ago, digital computers made information readable. Twenty years ago, the Internet made it reachable. Ten years ago, the first search engine crawlers made it a single database. Now Google and like-minded companies are sifting through the most measured age in history, treating this massive corpus as a laboratory of the human condition. They are the children of the Petabyte Age'. Anderson's conclusion merits some reflections: 'Correlation supersedes causation and science can advance even without coherent models, unified theories, or really any mechanistic explanation at all'. Confronted with the extraordinary power of modern approaches to data gathering, organization, retrieval and interpretation, I was left with the uneasy feeling that the Age of Darwin and Mendeleev has long passed into the pages of history, never to return again to science as practiced in the brave new world.

Mendeleev died in 1907, by which time six Nobel prizes had been awarded. He was twice considered seriously by the award committee but passed over, ostensibly on the ground that his seminal work had been done decades earlier. Mendeleev and Oswald Avery, who established DNA as the genetic material, are always cited as the most famous examples of Nobel omissions. The closest that Mendeleev came to getting the prize was in 1906, when Henri Moissan the discoverer of fluorine, an element that ironically appeared inevitable as a result of the periodic principle, was preferred (Hargittai, B. and Hargittai, I., *Struct. Chem.*, 2007, **18**, 253). A search of the Nobel website for references to Mendeleev yielded a dozen hits, including presentation speeches for Frederick Soddy (1921) and Edwin McMillan and Glenn Seaborg (1951). Curiously, there was no mention of Mendeleev in the presentation speeches for William Ramsay (1904) who discovered the rare gases or Moissan (1906).

In remembering Mendeleev we celebrate the diversity of chemistry as revealed by the elements and the materials they form. There is unity in this diversity. There is no paean to chemistry that is more enthusiastic than Oliver Sacks' wonderful book *Uncle Tungsten: Memories of a Chemical Boyhood* (Vintage Books, New York, 2001). Sacks' book will delight all those who relish chemistry and good prose. Sacks, a neurologist and writer of uncommon distinction, describes a photograph of Mendeleev: 'He looked like a cross between Fagin and Svengali, with a huge mass of hair and beard and piercing hypnotic eyes. A wild extravagant barbaric figure – but as romantic, in his way, as the Byronic Humphry Davy. He notes that he 'spent hours now, enchanted, totally absorbed, wandering, making discoveries, in the enchanted garden of Mendeleev'. Sacks also quotes C. P. Snow, whose first reaction to the Periodic Table was very similar: 'All the jumbles and recipes and the hotchpotch of the inorganic chemistry of my boyhood seemed to fit themselves into the scheme before my eyes – as though one were standing beside a jungle and it suddenly transformed itself into a Dutch garden. The periodicity that Mendeleev recognized is taught today as the inevitable consequence of atomic structure theory, one of the great constructs of 20th century physics. Is there more to be learnt from the Mendeleev classification? Spending some time in Mendeleev's garden may still be instructive and entertaining for a future generation of chemists.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Carbon Dioxide, Climate Change and Geoengineering

Carbon dioxide (CO_2) is a deceptively simple molecule. It is encountered in early chemistry courses, when molecular shapes are introduced for the first time. Teachers hold up CO_2 as an example of a molecule in which the three constituent atoms lie on a straight line; a 'linear' molecule as opposed to the 'bent' structure observed in the other common triatomic molecule, water (H_2O). In biochemistry, CO_2 appears as a product of animal respiration and a substrate for photosynthesis carried out by plants, a process in which the carbon in CO_2 is 'fixed' by conversion to glucose. There is a remarkable economy, a more appropriate word may be sustainability, in biology. Animals and plants coexist in a symbiotic relationship; a bond of cooperation and coexistence imposed by chemistry and biology. In geology, CO_2 provides an entry to geochemistry through the study of limestones and carbonates in water. To the spectroscopist, CO_2 appears at an early stage in discussions of normal modes of vibration, which absorb infrared radiation. I have encountered CO_2 in many discussions of molecular shape, spectroscopy and photosynthesis for much of my scientific career, but have scarcely thought of CO_2 as a molecule which would one day occupy the attention of the world's politicians; a molecule that today stands charged with, almost singlehandedly, being responsible for global warming and climate change. In retrospect, the problem seems obvious. Almost all human activity is powered by the burning of fossil fuels, a process which oxidizes organic carbon to CO_2, leading eventually to rising levels in the atmosphere. Diminishing forest cover, an inevitable consequence of development, reduces CO_2 absorption. As atmospheric CO_2 concentrations rise, heat radiated from the earth is trapped, leading to the phenomenon described by a phrase, which now rolls off the lips of every public figure with a familiar ring – 'global warming'. As the Earth's atmosphere warms up, glaciers and polar icecaps must melt, oceans and snow-fed rivers must rise, low lying areas must go under water and as always, the hardest hit will be the populations of the poorer countries. The rise of global temperatures must inevitably affect the dynamics of the atmosphere giving rise to 'climate change', another term that has entered the vocabulary of all educated persons. Hollywood movies have now replaced visions of a world rendered desolate after a nuclear war, by images of cities trapped in a new ice age. The area of climate change has now grown into an industry; international conferences abound, held most often in exotic locations; treaties and protocols are promoted by some countries, rejected by others; Nobel Peace Prizes are awarded to the most visible campaigners in the battle to control global warming; and the scientific literature is beginning to see the emergence of a new discipline termed 'geoengineering', based entirely on simulations. Global warming, climate change, geoengineering, carbon trading and carbon footprints are terms that are part of today's dictionary; their origins trace back to the chemistry and biology of CO_2.

There is a famous graph that is shown in almost every lecture on climate change, which summarizes the variation of atmospheric carbon dioxide as a function of time, beginning with measurements made in the late 1950s. Why was CO_2 in the atmosphere measured, with precision, at a time when the term 'nuclear winter' was becoming common and 'global warming' was not yet conceived? The first measurements were made by Charles Keeling, working as a postdoctoral fellow at Caltech in the mid-1950s. Keeling died in 2005. In an extremely readable prefatory chapter entitled 'Rewards and penalties of monitoring the earth' (*Annu. Rev. Energy Environ.*, 1998, **23**, 25), Keeling traces his interest in the problem to a suggestion by his mentor Harrison Brown on the power of applying chemical principles to geology. In Keeling's words: 'He suggested that the amount of carbonate in surface water and near-surface ground water might be estimated by assuming the water to be in chemical equilibrium with both limestone and atmospheric carbon dioxide'. Keeling had found a project that linked chemistry and geology, which was to turn into a lifetime's work. Keeling's description of his effort to build an apparatus to measure CO_2 concentrations in air evokes memories of an era where successful experimental design always had the stamp of simplicity. He used a gas manometer based on a 1916 design and 'a dozen 5-liter glass flasks. . . each closed off with a stopcock to hold a good vacuum'. Having struggled with failed vacuum and leaky stopcocks in my youth, I was riveted by Keeling's account. Keeling soon discovered that measurements in Pasadena were irreproducible, variations being a consequence of immediate

human activity. He travelled to Big Sur in California to sample 'the pristine air next to the Pacific Ocean', developing an elaborate sampling strategy. Keeling reflects, decades later, that his strategy was probably not required. Why then did he do it? His answer is engaging: 'The reason was simply that I was having fun. I liked designing and assembling equipment. I didn't feel under any pressure to produce a final result in a short time. It didn't occur to me that my activities and progress might soon have to be justified to the sponsoring Atomic Energy Commission'. Reading Keeling's account, I could not but marvel at the fact that enjoyment was the catalyst that triggered an experiment, whose results are the basis of all climate change discussions today. Keeling acknowledges that there are skeptics who do not believe there is a global warming problem; he adds that 'there are some who doubt that atmospheric CO_2 concentrations are rising'. There is little cause now to worry about the accuracy of CO_2 data; the work of Keeling and others has established these measurements on a firm footing. He notes that 'there is greater justification to doubt that air temperatures are rising' but argues that 'the atmospheric CO_2 record makes any other interpretation difficult'. The consensus today is that climate change is already upon us, requiring a serious strategy for limiting the use of fossil fuel in order to limit CO_2 emission. This is an area where the results of science have a direct and immediate impact on discussions of public policy.

The climate change debate has attracted authors and film makers. Michael Crichton's book *State of Fear*, a laboured work of fiction, is an attack on climate science research and its practitioners. Unlike *Jurassic Park*, it does not really stimulate the imagination. At the other end of the spectrum is the movie *An Inconvenient Truth*, that has the former US Vice-President Al Gore in a professorial role, lecturing on the need to arrest climate change. For the environmental activists, climate change is a new cause; an issue discussed provocatively by Freeman Dyson in the *New York Review of Books* (2008, **55**, June 12). Dyson's review focuses on two books that address issues of global policy and economics; discussions which can be disturbing and dismal when centred on 'carbon prices' and market rates for generating CO_2 emissions. For the common person these discussions appear to point towards a 'pay and pollute' policy. At the end of his review, Dyson notes that 'the main point is religious rather than scientific. There is a worldwide secular religion which we may call environmentalism. . . The ethics of environmentalism are being taught to children in kindergartens, schools and colleges all over the world. Environmentalism has replaced socialism as the leading secular religion. And the ethics of environmentalism are fundamentally sound. Environmentalism, as a religion of hope and respect for nature is here to stay. This is a religion that we all share, whether or not we believe that global warming is harmful'.

Is there a 'technological fix' to retard global warming and climate change? The emergence of a new term in the scientific literature, 'geoengineering' suggests that there will soon be a sizable group of scientists suggesting imaginative (and possibly dangerous) ways of 'engineering climate'. One of the approaches that appears to be gaining attention, largely thanks to an article by the Nobel laureate Paul Crutzen (*Climatic Change*, 2006, **77**, 211), is to develop strategies to reduce the amount of solar radiation reaching Earth. Crutzen suggested 'stratospheric sulfur injections', a process by which sulfur dioxide (SO_2) is let loose in the upper atmosphere, where a significant amount 'is converted to submicrometer size sulfate particles' which then contribute to 'backscattering to space of solar radiation'. Other proposals including 'orbiting sunshades' have appeared (Robock, A., *Science*, 2008, **320**, 1166). Geoengineering appears to be a fertile field for computer simulations (Matthews, H. D. and Caldeira, K., *Proc. Natl. Acad. Sci. USA*, 2007, **104**, 9949; Bala, G. *et al.*, *ibid*, 2008, **105**, 7664), driven by 'the desire to have the benefits of abundant fossil fuel energy without unfortunate consequences' (Brewer, P. G., *Proc. Natl. Acad. Sci. USA*, 2007, **104**, 9915). Is 'engineering' an appropriate descriptor for proposals to modify climate by geochemical and geophysical approaches? In an article highlighting the area of geoengineering entitled, 'Climate change: Is this what is takes to save the world?', there is an interesting thought: 'Humanity has shown a great capacity to make a mess, mostly as a side effect of just trying to make a living. But that is not engineering. Engineering involves intention' (Morton, O., *Nature*, 2007, **447**, 132). The Human Development Report 2007/2008 (UNDP) is subtitled 'Fighting climate change: Human solidarity in a divided world'. The report is exhaustive and well produced. Curiously, the chapter entitled 'Avoiding dangerous climate change: Strategies for mitigation' begins with a quote attributed to Mahatma Gandhi: 'Speed is irrelevant if you are going in the wrong direction'. In casually reading about climate change and some of the underlying science, I was left marvelling at how far we have travelled from Keeling's five liter flasks and precise measurements of CO_2 in the air to a world where economists and politicians can use carbon dioxide as a new currency in international relations.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Scientometrics: A Dismal Science

Measurements, accurately made and carefully documented, form the core of science. Gathering and organizing data, as a prelude to interpretation, is a routine activity in the daily life of scientists. But in a development that has rapidly engulfed science, scientists themselves have become objects of measurement, even as the field of scientometrics expands, fuelled by the technologies of the information age. The primary metric for measuring the performance of researchers in science is by studying their body of published work. This requires assessors who are experts in the area of research; 'peer reviewers' in every sense. Reading and understanding a colleague's scholarly output requires patience, commitment and a genuine interest in the subject being studied. However, there have been dramatic changes in the modes of assessing science and scientists over the last decade or so. The practice of using citation counts and journal impact factors to quickly gauge a scientist's performance has become commonplace. Databases are now widely available; on-line access and powerful software tools provide 'quantitative data' on research publications, at the 'click of a mouse'. In assessing scientists, counting the number of citations their papers have accumulated and inspecting (and sometimes 'averaging') the impact factors of the journals in which they publish has become a widespread practice. The 'average impact factor' seems particularly popular in India; its significance obscure to even its most ardent proponents. The motivation in all these attempts to quantitatively (and presumably, objectively) assess the scientific output of individuals, institutions and countries is to develop 'single number' criteria, that permit rapid comparison. Financial analysts seem comfortable with indices like the Dow Jones in the United States and the Sensex in India as an indicator of a stock market's health; should there not be a simple parameter to judge the productivity and impact of scientists and their institutions?

The world of science has never been the same since Eugene Garfield introduced the Science Citation Index and the idea of the Journal Impact Factor. Slow to be accepted at the beginning, these tools have now been sharpened by the information technology revolution; ensuring that scientists and administrators, editors and publishers self consciously examine performance indices at every step. The joy of science seems to have disappeared for many individuals, who are now consumed by impact factors and citation analysis. There is a more calculated and purposeful approach to science today; the refreshing naivete of the amateur has been replaced by the, sometimes, disturbing realism of the professional manager.

Almost exactly three years ago a posting appeared on the physics on-line archive entitled 'An index to quantify an individual's scientific research output' (Hirsch, J. E., arXiv: physics/0508024, August 3, 2005). The paper begins engagingly: 'For the few scientists who earn a Nobel prize the impact and relevance of their research is unquestionable. Among the rest of us, how does one quantitate cumulative impact and relevance of an individual's scientific research output?' I read this article almost immediately after it appeared online; struck by the simplicity of the '*h*-index', a single number that assesses an individual's consistency of performance. For good measure, the paper contained equations, graphs and integrals, that seemed to invest it with the respectability of the quantitative sciences. The Hirsch definition was simply stated: 'A scientist has index h if h of his or her N_p papers have at least h citations each and the other $(N_p - h)$ papers have $\leq h$ citations each'. The Hirsch paper was republished a few weeks later in the *Proceedings of the US National Academy of Sciences* (*PNAS*, 2005, **102**, 16569), clearly launching it on a career which will accumulate citations in great abundance. 'Scientometrists' (practitioners in the field of scientometrics) have been quick to adopt the *h*-index; the simplicity of the calculation ensuring that it can be applied to any aggregate of publications, permitting classification of individuals, institutions and journals. The *h*-index avoids some of the obvious pitfalls in assessing scientific output. Total citation counts may be skewed by very few highly cited papers, while journal impact factors can be misleading with most papers in high profile journals accumulating only a modest number of citations. Indeed, in an essay entitled 'Escape from the impact factor' the editor of *Nature*, Phillip Campbell notes that an analysis of citations received in 2004 to papers published in 2002 and 2003 revealed that '89% of our impact factor was generated by just 25% of our papers' (*Ethics Sci. Environ Polit.*, 2008, **8**, 5). Campbell views the impact factor from an enviable vantage point as

the editor of journal sought after by the most accomplished of authors. His conclusions are conservative: 'I am sure that citation statistics of large numbers of individual papers can reflect the impact of contributions at the institutional or national level. They can also provide useful input at the level of an individual. But for a sure assessment of an individual, there is truly no substitute for reading the papers themselves, regardless of the journals in which they appear'. Campbell's views appear in a theme section of the journal *Ethics in Science and Environmental Politics* (*ESEP*) entitled 'The use and misuse of bibliometric indices in evaluating scholarly performance' (*ESEP*, June 2008). The articles in this thematic section uniformly appear concerned over the growing misuse of impact indices and the attendant damage to the world of scholarly publishing. At times, the assault on the quantitative indices is entertaining with even Shakespeare (*Julius Caesar*) joining the battle:

> '*Are all thy conquests, glories, triumphs, spoils*
> *Shrunk to this little measure*'
>
> (Lawrence, P. A., *ESEP*, 2008, **8**, 9).

The 'little measure' that appears to have captivated both scientists and analysts is the *h*-index. A flood of papers in *Scientometrics* attempt to improve the *h*-index. The *g*-index is already being discussed (Egghe, L., *Scientometrics*, 2006, **69**, 131), and undoubtedly more indices will follow. In every case the attempts are directed towards balancing the contributions of unevenly distributed citations to publications by an author. Hirsch's original suggestion is striking in its simplicity and ease of computation, making it a dangerous weapon in the hands of naïve analysts. He has both a diagnosis and prescription, after examination of the *h*-index for a large number of celebrated (and some less well known) authors. He recommends that an *h*-index of 20 after 20 years of activity, 'characterizes a successful scientist'. Since longevity must hopefully improve a scientist's *h*-index, Hirsch adds that a value 'of 40 after 20 years . . . characterizes outstanding scientists, likely to be found only at the top universities or major research laboratories', while 'an *h*-index of 60 after 20 years or 90 after 30 years, characterizes truly unique individuals'. Hirsch then prescribes (possibly tongue-in-cheek) norms for career advancement and recognition. An *h*-index of ~12 should qualify for grant of academic tenure (a hurdle that does not exist in our research institutions), $h \sim 18$ is the suggested bar for a professorship and $h \sim 45$ appears a norm for election to the US National Academy. While *h*-indices vary significantly with fields Hirsch notes, somewhat unsurprisingly, 'that Nobel prize winners have substantial *h*-indices', with a median value of 35.

The Hirsch paper has catalysed a burst of scientometric activity. Interestingly, the preoccupation with scientometrics appears disproportionately high in countries with limited scientific activity. Three databases are used by analysts; most often their limitations are not explicitly stated. The oldest and most comprehensive is the ISI Science Citation Index (Web of Science), about which the Editor of *Nature* laments: 'Try as we might, my colleagues and I cannot reconcile our own counts of citable items in *Nature*, several other *Nature* journals and indeed *Science*, with those used by ISI' (Campbell, P., *ISEP*, 2008, **8**, 6)'. The *Scopus* (Elsevier) and *Google Scholar* databases have their own idiosyncracies, which will undoubtedly be highlighted by serious, professional analysts. Unfortunately, scientometrics is the province of a diverse group of analysts; many studies commissioned by agencies of Government of India do not seem to bear the stamp of rigour. The need for understanding the nuances of publication practices in different fields of science is usually ignored. The inability of many 'scientometrists' to appreciate the need to check their data and carry out 'control analyses' renders many studies carried out in India almost useless. Most disturbingly, policy and decision makers often use these poorly conceived and badly executed studies to pronounce judgements. There is a need to insist on a more scholarly approach to scientometrics, which will enable new entrants to the area to understand that rigour is an integral part of the discipline. Citations and *h*-indices are good measures to identify exceptional performers; they are not useful in discriminating between scientists of average attainment. To borrow a widely used term in science and engineering, the signal is submerged in the noise, in such cases. Counting papers and citations has become an inescapable activity in the field of science. Even as the number of parameters that define the quality and quantity of scientific output increase, a little of the joy of doing research appears to be vanishing. Economists are fond of measuring the cost to benefit ratio. Scientists may soon argue that a cost to impact ratio is a valuable parameter. Economics has been famously and unfairly characterized as a 'dismal science', a term that appears in Thomas Carlyle's extensive writings on slavery in the mid-19th century. It would appear that this description is most apt for scientometrics today.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Analytical Chemistry: Resurrection of a Discipline

Chemistry is a subject characterized by well-defined boundaries, that separate the major sub-disciplines of organic, physical and inorganic chemistry. More recently, theoreticians who have little taste for the sounds and smells of a chemistry laboratory, have carved out a kingdom of their own. The organic chemists focus their attention on a few elements; carbon occupying centrestage. The inorganic chemists range farther afield. The borders are well demarcated, although organometallics and materials chemistry have, at least in part, breached barriers that seemed as impregnable as the Berlin Wall, in the years before its dramatic dismantling. Physical chemists appear wedded to areas that have their roots in physics; sometimes labelling themselves as chemical physicists, an acknowledgement of their distance from the heart of chemistry. The fierce loyalties of the practitioners of these sub-disciplines resemble those observed in religious sub-sects. The other sciences seem less divided by such clear boundaries. Analysis of substances, pure and mixtures of several components, forms the core of chemistry. Curiously, analytical chemistry, which once seemed a respectable term, is now largely shunned; a Cinderella amongst the various sub-sects of chemistry. For most people of my generation, who have had chemistry in college, the term conjures up visions (most often, nightmares) of being asked to separate and identify the constituents of an unknown mixture in a practical examination. The task was often aided by the goodwill of laboratory attenders who knew the right answers. Analytical chemistry, as I learnt it decades ago, always seemed to teach you more about practical human skills than about the subtleties of identifying molecules. Flame tests, gravimetry, titrations, colour reactions, precipitates that must appear and later dissolve, mirrors that permanently scarred clean test tubes and coloured and colourless gases that miraculously appeared under gentle chemical coaxing are memories that remain.

Analysis and synthesis constitute the heart of chemistry. Chemical analysis is, of course, as old as chemistry itself. The roots of modern analysis can be traced to the early years of the 19th century. Chemistry's origins coincide with some of history's most dramatic moments. Lavoisier's book, *Elements of Chemistry*, appeared in French in 1789 and a year later in English, sparking off the chemical revolution. Ironically and tragically, Lavoisier was caught in the turmoil of the French Revolution and guillotined in 1794. In a book tracing the 'discovery of the molecular basis of life', Graeme Hunter quotes Joseph Lagrange: 'It took them only an instant to cut-off that head, and a hundred years may not produce another like it' (*Vital Forces*, Academic Press, New York, 2000, p. 15). In tracing the path from the 'animal chemistry' of Lavoisier to molecular biology, Hunter pauses to survey early 19th century chemistry. His assessment, that 'chemistry in the first half of the 19th century was dominated by the findings, teachings and personalities of three men: Jöns Jacob Berzelius, Justus Liebig and Friedrich Wöhler', will be shared by many students of the history of the discipline. In many ways, analytical chemistry was truly midwifed into existence in Berzelius' work. Hunter notes that 'Berzelius was above all a classifier, the Linnaeus of chemistry'. In looking back at two centuries of progress since Berzelius' *Textbook of Chemistry* appeared in 1808, one can immediately see that analysis pervades every corner of chemistry. The pace of advance in chemical analysis has been spectacular in the second half of the 20th century; a diverse portfolio of spectroscopies permit identification and quantitation of chemicals, with a degree of sensitivity that is simply astounding. Molecular size and complexity are not a bar to analysis; mixtures of the most complex nature are resolved and analysed with relative ease today. Chemical analysis is the essence of forensic science and medical diagnosis. Paradoxically, analytical chemistry has become a victim of its own spectacular success, slowly disappearing as a well-defined sub-discipline of chemistry.

I must confess that writing a column on analytical chemistry would not have occurred to me if a book had not appeared on my desk, with a little note from the journal office asking me to suggest a potential reviewer. This is a routine chore for an editor and I usually have only a passing glance at the book. Over the years I have learnt that titles and publishers are a useful guide in assessing whether a review is merited. This time I was intrigued by the title and a quick glance at the contents persuaded me to retain the volume, which was the *Annual Review of*

Analytical Chemistry (Volume 1, 2008). Two hundred years after Berzelius, Annual Reviews Inc had turned to *Analytical Chemistry*, a discipline that I mistakenly thought had been quietly laid to rest. The Annual Reviews is, of course, an enormously influential publishing enterprise. Beginning with the *Annual Review of Biochemistry* in 1932, volumes now appear, yearly, in 33 different fields, ranging from *Genomics and Human Genetics* to *Nuclear and Particle Science*. Indeed, there is even a series on *Law and Social Science*. Fifty years after the first appearance of the *Annual Reviews*, the founder of the series, J. Murray Luck, describes the genesis in Volume 50 of the *Annual Review of Biochemistry*. As a young faculty member at Stanford in the late 1920s he found himself 'knee deep in trouble' while attempting to 'give a course on current research in biochemistry to a group of ten to fifteen graduate students'. The idea of an 'annual review' has proved remarkably successful in highlighting rapidly developing fields in diverse areas of science. Writing a scholarly and readable review is no easy task. Remarkably, the *Annual Review* series has maintained an enviably high quality, over a period of 75 years, even as the range of subjects has expanded. Despite their obvious utility, I have learnt to be wary in choosing reviewers for the *Annual Reviews*. While most scientists are pleased to receive a review copy that they can retain, there are accomplished, busy and impatient colleagues who peremptorily dismiss any idea of reviewing a collection of reviews.

The volume on *Analytical Chemistry* signals the beginning of a new series. Its prefatory chapter sets a tone that places the collection of reviews, right in the centre of the most active and visible fields of science today – medicine, biology and biotechnology. Lee Hood's account of 'A personal voyage of discovery: Developing technology and changing biology' is a story of the development of methods and instruments that permit rapid chemical analysis and synthesis of proteins and nucleic acids, molecules central to cellular biology. Hood begins with a quote from Freeman Dyson that is most apt for modern biology: '*New directions in science are launched by new tools much more often than by new concepts. The effect of a tool driven revolution is to discover new things that have to be explained*' (*Imagined Worlds*, Harvard University Press, 1998). The automated protein and DNA sequencers and the peptide and DNA synthesizers came out of Hood's laboratory at Caltech in the 1980s; rapid commercialization brought the new tools into biochemistry and molecular biology laboratories across the world and fuelled the growth of the biotechnology industry. The genomics revolution and the explosion of biological information is a direct outcome of the new technologies; systems biology, an area highlighted in Hood's overview, appears to be emerging as an integrating discipline. The deep inroads that biology has made into medicine, over the last decade or so, have largely been a result of the rapidity with which detailed and accurate chemical analysis of biological molecules can be performed. The overwhelming majority of the 29 reviews in this volume deal with methods that are directly applicable to biological problems. Old fashioned chemists, longing for a simpler age, may find some satisfaction in the realization that biophysics and spectroscopy now advance under the banner of analytical chemistry. The analysis of biological fluids, most commonly blood and urine, but increasingly others like cerebrospinal fluid, lung aspirates and extracts of biopsy tissue by NMR and chromatography coupled mass spectrometry, is becoming commonplace. Infelicitous terms like metabolomics and metabonomics have entered the world of chemical analysis (J. C. Lindon and J. K. Nicholson, p. 45). Mass spectrometry, whose origins can be traced to the work of J. J. Thompson in the last decade of the 19th century, is now the most rapidly evolving analytical method, applicable to almost every problem of chemical analysis. The reviews in this collection describe some spectacular new frontiers. These include, rapid characterization of microorganisms (P. A. Demirev and C. Fenselau, p. 71) and the use of laser desorption mass spectrometry for imaging biological tissues (K. E. Burnum *et al.*, p. 689). There are distinct advantages to being able to image molecular distributions in biological tissues. Many reviews in this volume describe techniques that have remarkable sensitivity and high spatial resolution. The 'single cell' is now the target of chemical analysis; a tribute to the rapid pace of technological advance. Lasers, specifically ultrafast lasers, are a key to many of the techniques described in this inaugural issue of the *Annual Review of Analytical Chemistry*. As the volume and resolution of chemical analysis of heterogeneous systems increases, the pressure on handling and processing information will grow. Dyson's forecast that we will 'discover new things that have to be explained' will, inevitably, come true.

In reflecting on the resurrection of analytical chemistry, I was drawn to an account of the life and times of Jöns Jacob Berzelius (J. Wisniak, *Chemical Educator*, 2000, **5**, 343). I realized then that Berzelius had indeed even anticipated the need for *Annual Reviews*. Wisniak notes that Berzelius published *Annual Surveys of the Progress in the Sciences* from 1821 to 1848, the year he died. These volumes which totalled 'about 12,000 pages' were 'probably his most important publications'. The appearance of an *Annual Reviews* in analytical chemistry marks the rebirth of an old discipline; an acknowledgement of the triumphant advance of the area of chemical analysis in the two centuries since Berzelius.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Rutherford's Nucleus and the CERN Experiment

A hundred years ago Ernest Rutherford received the Nobel Prize. Ironically, the man widely regarded as the pre-eminent experimental physicist of his times was presented the Nobel prize for chemistry in 1908. The award recognized Rutherford's researches 'concerning the disintegration of elements and the chemistry of radioactive substances'. Soon after in 1911, the nuclear atom arrived; a result of Rutherford's experiments on alpha particle scattering. The world of science has been transformed in the years that have followed, by a spectacular succession of advances in physics which have revolutionized our understanding of matter, chemistry and biology. Bohr's original conception of the model of the atom, electrons revolving in discrete orbits around Rutherford's nucleus, is a picture with which most people are instinctively comfortable; the analogy with the planets and the sun seems to provide a bridge between the seen and the unseen. The atom has proved to be more complex in the century since Rutherford and Bohr. So too, has the nucleus. Some decades ago in a misguided bout of literary enthusiasm, I picked up a copy of James Joyce's *Finnegans Wake*. It is a book that is impossible to understand by simple-minded readers and I did not get far. Years later I realized that a word which launched a new revolution in particle physics was indeed a creation of Joyce. The term 'quark', which transformed the comfortably simple picture of Rutherford's nucleus, was borrowed from Joyce's famous, but largely unreadable, book. The realization that the nucleus was still to yield many secrets and held the key to understanding the forces between elementary particles has fuelled research in nuclear and particle physics over the last few decades. Unlike the early years, theory has outstripped experiment, with experimental tests of theory becoming extremely expensive and forbiddingly complex.

In the last few weeks there has been unprecedented attention in the popular press and television on the starting up of the Large Hadron Collider (LHC), at a site near Geneva. This is an experiment on a scale that might even have astounded Rutherford. When fully operational (and even as I write the first reports of the inevitable technical problems that always arise, have appeared) beams of protons, traversing an enormous circular path in opposite directions, at speeds close to that of light, must collide. These spectacular collisions must release debris, which will hopefully provide new insights not only into the nucleus, but into events that happened when the Universe was born, immediately after the 'Big Bang'. Paradoxically, for the uninitiated observer, probing the deepest recesses of the atom seems to provide clues to understanding not only the genesis of the universe, but also illuminates the nature of another current favourite amongst astrophysicists—dark matter. The CERN collider, by all accounts, is an engineering marvel and testimony to the power of international cooperation in 'big science'. It is also a European triumph, surviving successfully in the aftermath of the demise of the Superconducting Super Collider project in the United States. The real experiment will begin soon but cautious voices have already been raised, which suggest the search for the 'God particle', as the Higgs boson has been dubbed, may go on far a while. On the sidelines, public interest has been kept alive by a rekindled controversy involving Stephen Hawking and Peter Higgs, the theorist whose work predicts the existence of the as yet unobserved particle that bears his name. There is a pervasive feeling that these new high energy experiments may yield surprises, providing particle physics a new lease of life, in an era increasingly dominated by other sciences, seemingly more deeply rooted in the problems of everyday existence. High energy physics is an esoteric area, where common words like 'colour' and 'string' acquire connotations so far removed from everyday experience, that even James Joyce might have been left bemused. The LHC experiment has suddenly catapulted high energy physics to the position of the 'poster boy of science', in the press. An experiment that promises to unveil fundamental constituents of matter, address events at the creation of the universe, provide insights into dark matter and even involve that perennial favourite, the black hole, cannot fail to excite the imaginative mind. Curiously, all discussions of the LHC experiment are dominated by theoretical physicists; the experimenters seem to be faceless. Modern day big science requires such large teams, with groups having diverse skills, that the individual retreats into anonymity. Conflict and competition, which sometimes seem to play so much of a part

in the history of science, appear to have little place in mammoth projects, where cooperation and organization are the key to success. Particle physics has indeed come a long way from the days of Rutherford.

The Rutherford era belongs to another day and another age. It is hard to imagine the Cambridge settings of the first three decades of the 20th century when physics, and indeed science, was transformed ('transmuted' might be an appropriate alternative). There are biographies of Rutherford which are valuable but do not, at least for me, capture the essence of the man and his times (*Rutherford: Scientist Supreme*, John Campbell, AAS Publications, 1999). My favourite account of this subject is in a volume penned by C. P. Snow in the mid-1960s, in which the author provides personal impressions of men he knew (*Variety of Men*, Macmillan, 1967; Penguin, 1969). The list is remarkable, beginning with Rutherford and includes G. H. Hardy, H. G. Wells, Einstein, Churchill and Stalin. Snow, a physicist turned administrator and writer, has an uncanny ability to bring his subjects alive. Masterful prose and a keen eye for the quirks of human nature are characteristic features of Snow's writing. Rutherford comes across in Snow's description as a larger than life figure who, more than anyone else, made Cambridge 'the metropolis of physics for the entire word' in the 1920s and 1930s. In Snow's words: 'He was a great man by any standards which we can apply. He was not subtle: but he was clever as well as creatively gifted, magnanimous (within the human limits) as well as hearty. He was also superbly and magnificently vain as well as wise—the combination is commoner than we think when we are young. He enjoyed a life of miraculous success. On the whole he enjoyed his own personality. But I am sure that, even quite late in his life, he felt stabs of sickening insecurity'. Snow recounts a discussion by 'half a dozen men all of whom had international reputations' on Rutherford's place in science: 'Was Rutherford the greatest experimental scientist since Michael Faraday? Without any doubt. Greater than Faraday? Possibly so. And then it is interesting, as it shows the anonymous Tolstoyan nature of organized science – how many years' difference would it have made if he had never lived? How much longer before the nucleus would have been understood as we now understand it? Perhaps ten years. More likely only five.' Rutherford's work set the stage for modern nuclear and particle physics, as Snow states so simply: '. . . he broke up a nucleus of nitrogen by a direct hit from an alpha particle. That is man could get inside the atomic nucleus and play with it if he could find the right projectiles. These projectiles could either be provided by radioactive atoms or by ordinary atoms speeded up by electrical machines'. Snow's 'electrical machines' have evolved over the decades into the Light Hadron Collider, which is now poised to break further into the heart of the nucleus. Snow reflects: 'Rutherford himself never built the great machines which have dominated modern particle physics, though some of his pupils, notably Cockcroft started them. Rutherford worked with bizarrely simple apparatus; but in fact he carried the use of such apparatus as far as it would go. His researches remain the last single-handed achievement in fundamental physics. No one else can ever work there again – in the old Cavendish phrase – with sealing wax and string'. Snow recalls that Rutherford proclaimed: 'I could do research at the North Pole', a sentiment that would have been shared by C. V. Raman.

The days of sealing wax have long disappeared into the pages of history. The run-up to the Large Hadron Collider is a tale of 'politics and high finance' compellingly told by a former Director-General of CERN, Chris Llewellyn Smith (*Nature*, 2007, **448**, 281). Research and development 'on the very demanding LHC magnets' started as early as 1988. Raising finance for a project of this magnitude (current estimates in the press quote figures as high as $8–10 billion) and shepherding the venture through years of uncertainty and changing perceptions of politicians and scientists is an achievement of which the international particle physics community can be justly proud. *Nature* begins a new series of essays on '*Meetings that Changed the World*' with a retrospective on a meeting, that led to the birth of CERN, held in December 1951. Francois de Rose, who chaired that UNESCO meeting, notes that he could conclude the event by saying: 'if it would be difficult to find scientists among diplomats, it was obvious that there were many diplomats among scientists' (*Nature*, 2008, **455**, 174). In looking back at the gigantic LHC project, Llewellyn Smith recalls a statement in *The Times* (London) 'on the worlds great construction projects which asserted that "if those involved didn't lie about the cost, they never would be built" '. While he notes this did not happen in the LHC projections, it may sometimes be necessary 'to approve projects without contingency on the basis of optimistic assumptions . . . if . . . this is the only way to get them approved'.

What will physicists find when the LHC matures in its career as a particle smasher? Leon Lederman, who titled his book *The God Particle* (he claims that his 'editor explained with an eye on the sales figures, "no one has ever heard of Higgs" ') asserts that 'we are reaching what the medieval map maker would have denoted *terra incognita*' (*Nature*, 2007, **448**, 310). He provides 'a speculative laundry list' of LHC stimulated advances but suggests that the lesson of history teach us that 'a discovery will be made that was not anticipated by theorists'. That sentiment would have undoubtedly pleased Rutherford, who is reported to have said: 'Don't let me catch anyone talking about the universe in my department'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

A Recipe for Success in Science: The Hamming Prescription

In academic settings casual conservations often turn to the subject of how can the committed scientist do significant research. Is there a recipe for success in science? Will all of one's work, carefully done over a lifetime, disappear unremembered and unsung even by colleagues in the same discipline? What distinguishes those who achieve the highest peer recognition from their faceless compatriots in science? How does a researcher at the start of a career choose an area of work and find problems whose solutions are of some importance? Is it wise to set one's sights on the 'grand challenge' problems, to the exclusion of all else or is such overweaning ambition likely to lead a researcher into the quixotic exercise of tilting at windmills? In a recent conversation, with a deceptively laid back but thoughtful colleague, my attention was drawn to a talk given by Richard Hamming over two decades ago. The talk in its entirety, including the discussion that followed, is available on the worldwide web with an annotation by J. F. Kaiser. Entitled *You and Your Research*, Hamming's talk forms part of a Bell Communications Research Colloquium Series and was delivered on 7 March 1986. His prescriptions are listed as '*Ten Simple Rules for Doing Your Best Research, According to Hamming*' in a recent editorial in the journal, *PLoS Computational Biology* (Erren, T. C. *et al.*, 2007, **3**, e213). Richard Hamming (1915–1998) was a mathematician and computer scientist (at a time when the discipline was in its infancy), who did much of his work at Bell Labs in the period 1946–76. This was the time when the electronics revolution took place and the age of information was born. Hamming began his career at Los Alamos, where he had a ringside view of the Manhattan project. In his own words, he says that he was 'brought in to run the computing machines that other people had got going, so those scientists and physicists could get back to business. I saw I was a stooge. I saw that although physically I was the same, they were different. And to put the thing bluntly, I was envious . . . I saw Feynmann up close. I saw Fermi and Teller. I saw Oppenheimer. I saw Hans Bethe: he was my boss. I saw quite a few very capable people. I became very interested in the difference between those who do and those who might have done'. Hamming entered Bell Labs in 1946 to become an important contributor to the scientific revolution that would take shape in this unique institution.

Bell Laboratories has been in the news recently as the parent company Alcatel-Lucent has begun the process of winding down basic research, in an institution that was once the undisputed leader of fundamental work in an industrial environment. The Bell Labs, whose ambience Hamming describes, has passed into history. But for a time it must have been science's equivalent of Camelot. Claude Shannon (1961–2001) wrote the paper, which marks the birth of information theory in the in-house journal *Bell System Tech.* in 1948. William Shockley, John Bardeen and Walter Brattain did the work on the transistor in 1946–47 and by 1950, semiconductors were beginning to be understood. John Tukey (1915–2000) the statistician, information scientist and generalist, whom Shannon credited as the coiner of the term 'bit' (binary digit) was there. It was these men whom Hamming observed and whom he describes in his remarkable lecture. He shared an office with Shannon who worked at that time on information theory. Hamming notes that he did his own famous work on coding theory at the same time. How did this happen? As Hamming says: 'it was in the atmosphere'. An obituary of Shannon notes that he 'must rank near the top of the list of major figures of twentieth century science, although his name is relatively unknown to the general public. His influence on everyday life which is already tremendous can only increase with the passage of time' (Colderbank, R. and Sloane, N. J. A., *Nature*, 2001, **410**, 768). In Hamming's assessment 'Shannon's scientific career' (and here he refers to extraordinarily creative activity) ended 'when he left Bell Labs'. Hamming recognizes that there can be no ideal environment for doing science. Managements and systems can often appear to be obstructive and inhibitory. His advice is to learn to subvert the system to serve your own scientific goals. Many scientists in India would do well to heed his words: 'If you choose to assert your ego in any number of ways . . . you pay a small price throughout the whole of your professional career. And this over a whole lifetime adds up to an enormous amount of needless trouble . . . By realizing you have to use the system and studying how to get the system to do your

work, you learn how to adapt the system to your desires. . . . Many a second rate fellow gets caught up in some little twitting of the system and carries it through to warfare. He expends his energy in a foolish project. Now you are going to tell me that somebody has to change the system. I agree; somebody has to. Which do you want to be? The person who changes the system or the person who does first class science? . . . Be clear when you fight the system and struggle with it, what you are doing, how far to go out of amusement and how much to waste your effort fighting the system. My advice is to let somebody else do it and you get on with becoming a first class scientist. Very few . . . have the ability to both reform the system and become a first-class scientist'. Hamming's advice is especially relevant in surroundings where the environment and management structure are far from ideal for the practice of science. It is sometimes too easy to come up with alibis for non-performance, citing a long list of systemic hurdles. Hamming has sage counsel: 'Don't try and kid yourself. You can tell other people all the alibis you want. I don't mind. But to yourself try to be honest'.

Hamming's entertaining talk has been summarized into a set of ten rules by Erren *et al.* in 2007. Many are self evident, while a few are deserving of serious thought. Like others before him, Hamming endorses the 'prepared mind'; a phrase traced to Pasteur's famous saying that 'fortune favours the prepared mind'. Luck is an important element in success, but opportunities must be quickly recognized by an alert and searching mind. Like Edison before him, Hamming advocates hard and effective work. Genius, as the oft quoted adage goes, is after all 'ninety nine per cent perspiration and one per cent inspiration'. I have noticed that many aspiring researchers do not realize how much effort is expended by their more successful counterparts. Productivity requires very hard work and total committment. The image of casual geniuses who solve problems by sudden flights of imagination is romantic, but misleading. Deep insights occur only to those who think deeply and to those who are obsessed with the problems they are tackling. Hamming quotes Newton: 'If others would think as hard as I did they would get similar results'. He goes on to emphasizing the importance of hard work, in language that is characteristically his own: 'Knowledge and productivity are like compound interest. Given two people with exactly the same ability, the one person who manages day in and day out to get in one more hour of thinking will be tremendously more productive over a lifetime'. Hamming acknowledges that age is important, although youth is not an asset in many fields. He advises aspiring researchers to be self-confident ('Yes I would like to do something significant') and to be courageous. Self-confidence and courage in tandem can be a powerful combination. If hard work and intelligence are added the recipe for success is nearly complete.

There are two pieces of advice that Hamming gives, which merit special emphasis. He argues that blaming 'working conditions' for poor performance may be a common, but inappropriate excuse: 'What most people think are the best working conditions, are not. Very clearly they are not because people are often most productive when working conditions are bad. One of the better times of the Cambridge Physical Laboratories was when they had practically shacks – they did some of the best physics ever'. Hamming strikes a chord here, to which most older readers will respond. The constant refrain that we hear of 'poor conditions for research' maybe an excuse that is wearing thin. He is a strong advocate for transforming 'a defect into an asset' by arguing: 'It is a poor workman who blames his tools – the good man gets on with the job, given what he's got, and gets the best answer he can'. Of all Hamming's pieces of advice I liked his 'open door' the best: 'Another trait, it took me a while to notice. I noticed the following facts about people who work with the door open or the door closed. I notice that if you have the door to your office closed, you get more work done today and tomorrow, and you are more productive than most. But 10 years later somehow you don't quite know what problems are worth working on; all the hard work you do is sort of tangential in importance. He who works with the door open gets all kinds of interruptions, but occasionally gets clues as to what the world is and what might be important. Now I cannot prove the cause and effect sequence because you might say, "The closed door is symbolic of a closed mind". I don't know. But I can say there is a pretty good correlation between those who work with the doors open and those who ultimately do important things, although people who work with doors closed often work harder'. Hamming's 'open doors' principle seems to operate in some places at some times, with extraordinary science emerging as a consequence of 'group creativity'. The Bell Laboratories of the 1940s and 1950s and Cambridge for physics in the 1920s and molecular biology in the 1950s are often cited as examples (Erren, T. C., *Medical Hypotheses*, 2008, **70**, 473–477).

Hamming observed his subjects closely. He notes perceptively that 'great scientists tolerate ambiguity well. They believe the theory enough to go ahead; they doubt it enough to notice the errors and faults so they can step forward and create the new replacement theory. If you believe too much you'll never notice the flaws; if you doubt too much you won't get started. It requires a lovely balance'. But Hamming reserves some of his most penetrating insights for the immortals. 'When you are famous it is hard to work on small problems. This is what did Shannon in. After information theory what do you do for an encore?' He argues that 'when you get early recognition it seems to sterilize you'. In surroundings, where the bar for success is placed rather low, the phenomenon of early over-recognition may serve to inhibit rather than promote good science.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Missing Out on a Nobel Prize

Every year the month of October brings the announcement of the Nobel prizes in the sciences, originally specified in Alfred Nobel's will: physics, chemistry and physiology or medicine. For scientists in these disciplines, the prize remains the pinnacle of academic achievement. In major laboratories across the world there is expectation. When the week is over there is much to celebrate for the chosen few and generally, well hidden disappointment in many, who are passed over. As the number of scientists and subdisciplines has exploded in the last three decades of the 20th century, Nobel choices have become increasingly difficult. There are many specific discoveries to be recognized and many extraordinary lifetime achievements to be honoured. A balance between different areas of the chosen sciences needs to be maintained. When the winners are announced, the postmortems begin. The hopefuls retire into the shadows of their laboratories to await another year. In the immediate aftermath of the Nobel week, there are inevitable discussions on those who might have shared the prize in the chosen field. This year has been no exception. The award of the prize in physiology or medicine to the discoverers of the virus that causes acquired immunodeficiency syndrome (AIDS) to the French scientists Luc Montagnier and Francoise Barre-Sinousi appears to pass a definitive judgement on an old question: 'Who are the discoverers of the AIDS virus?'. Observers of this field will recall the major controversy that provided riveting drama for the biomedical research community, in the 1980s. The French group, now vindicated, was ranged against the American team led by Robert Gallo, in a dispute that involved patent rights and financial returns for an AIDS blood test, based upon the virus. There was a remarkable spectacle, about twenty years ago, when a resolution to the dispute was reached and a settlement announced by Jacques Chirac and Ronald Reagan; a rare example of conflict resolution in science by leaders of two major countries. Echos of this old battle pervade many of the media reports that describe this year's award. In many earlier awards, Nobel watchers have been quick to point out major omissions.

This year's award in chemistry leads us to a strange and somewhat disquieting story; an omission that can hardly be justified on scientific merits. The 2008 award recognizes 'the discovery and development of the green fluorescent protein, GFP'. The laureates named are Osamu Shimomura of the Marine Biological Laboratory, Woods Hole, Martin Chalfie of Columbia University and Roger Tsien of the University of California, San Diego. The GFP is a substance isolated from the jellyfish *Aequorea victoria*, which absorbs light at 400 nm and emits a green fluorescence at about 505 nm. GFP is a protein and hundreds, indeed thousands, of proteins have been studied and dissected by biochemists across the world. Why has the Nobel Committee singled out GFP's discovery for recognition? The answer is evident when one examines the vast and ever expanding literature of molecular and cellular biology. GFP, with its characteristic green glow after excitation, has become an immensely powerful tool for visualizing gene expression in cells and has catalysed an explosive growth of fluorescence imaging as a means by which biologists can directly monitor cellular processes, particularly the 'localization and fate' of cellular proteins. Freeman Dyson's reflective comment that 'science is often driven by new technology rather than by new concepts', is probably more true in the biological sciences than in many other disciplines. GFP has been labelled as 'a guiding star for biochemistry' in the Nobel press release. The scientific background to the Nobel award, produced by the Royal Swedish Academy, notes that 'neither the biochemical nor the genetics revolution provided the experimental tools that would allow for quantitative and experimentally well-defined monitoring at the molecular level of the spatio-temporal intra- and inter-cellular processes that define the dynamic behaviour of all living systems. To obtain such knowledge new experimental and conceptual tools were required. Now, at the beginning of the 21st century we are witnessing the rapid development of such tools based on the GFP from the jellyfish *Aequorea victoria*, its siblings from other organisms and engineered variants of members of the "GFP family" of proteins'.

GFP's history begins with Shimomura's discovery of the protein and its characteristic glow, under UV irradiation, in 1962. This work followed his earlier work on the chemiluminescent protein aequorin, involving the pains-

taking collection of thousands of bioluminescent jellyfish. Shimomura's papers which described the discovery of the protein and the characterization of the chromophore (the small portion of the molecule, which absorbs and emits light) appeared in the period between 1962 and 1979. In a major review published in 1998, one of this year's laureates begins by noting: 'In just three years, the green fluorescent protein has vaulted from obscurity to become one of the most widely studied and exploited proteins in biochemistry and cell biology' (Tsien, R., *Annu. Rev. Biochem.*, 1998, **67**, 509). Clearly, he refers to events that happened in the years 1995–97, when GFP was launched on its career as an indispensable tool in cell biology. The critical breakthrough came in 1992 when the gene that encodes GFP was cloned (Prasher, D. C., *Gene*, 1992, **111**, 229) and the primary sequence of amino acids in the polypeptide established. Cloning genes was still a far from routine task in the late 1980s when Douglas Prasher began to work on the GFP gene. There was no widespread interest in GFP at that time and Prasher must have ploughed a lonely furrow. One of this year's laureates, Martin Chalfie recalls that in 1988, following a seminar, he thought of the wonderful prospect of introducing a fluorescent protein into cells. He found Prasher and after a four-year period did indeed receive the gene. The paper that described the work that followed, entitled 'Green fluorescent protein as a marker for gene expression', appeared quickly thereafter (Chalfie, M. *et al.*, *Science*, 1994, **263**, 802). The remarkable spontaneous generation of the fluorescing group upon GFP production was established and the long awaited cellular marker had arrived. Almost concurrently, Roger Tsien working with Prasher's GFP gene established that fluorescence wavelengths could be tuned by mutations of GFP sequence, setting the stage for engineering the optical properties of the marker protein (Heim, R. *et al.*, *Proc. Natl. Acad. Sci. USA*, 1994, **91**, 12501). GFP which had lain dormant in the scientific literature for over a decade after Shimomura's work had been transformed into an indispensable molecular tool in biology. The key breakthrough was undoubtedly the cloning of the GFP gene by Douglas Prasher, who not only provided the gene but also coauthored the seminal papers by Chalfie and Tsien in 1994. Alfred Nobel's will precludes more than three awardees. Prasher, despite his critical contribution, must be content with being mentioned in the 'Scientific Background on the Nobel Prize in Chemistry 2008' produced by the award committee. He is indeed the 'fourth man'.

Who is Douglas Prasher? A search on PubMed reveals 16 publications, the most recent paper appearing over a decade ago in 1997. A short review in *Trends in Genetics* (1995, **11**, 320) details succinctly the 'uses of the green fluorescent protein cDNA' and notes that the accompanying articles 'describing the use of GFP represent only the first of many doors that GFP can potentially open for cell and developmental biology'. Shortly after the Nobel announcement, Prasher was discovered working as a 'shuttle car driver' in Huntsville, Alabama, earning $10 an hour. Browsing the Internet, I came across several sites (including Wikipedia) where the Prasher story is described. It is a tale that scientists who struggle for positions and funding may find haunting. Prasher lost tenure at Woods Hole, having little to show for his struggle with the GFP gene. A move to the US Department of Agriculture and job losses after funding cuts, forced Prasher out of science. In the many interviews that have appeared, Prasher's reactions are stoic: 'Do I feel cheated or left out? No, not at all. I had run out of funds and these guys showed how the protein could be used and that was the key thing'. Could the Nobel Committee have awarded a prize to an ex-scientist, who did not have any institutional address? Was it a pragmatic decision which minimized any controversy that might follow? The answers to these questions must, of course, remain in the realm of speculation.

For over a century, Nobel prizes have been awarded for major conceptual advances, lifetime achievements, although specific contributions are cited, and for discoveries that transform a discipline by providing new tools that permit advance on a broad front. The key steps in the history of GFP must include the discovery of the protein and its properties and the cloning of the gene, which opened the path to biological applications. Douglas Prasher chose his area of research, with an enviable degree of foresight. In retrospect, his choice of GFP satisfied the sage advice that is often given that 'one must work on problems of significance'. He did succeed in the laboratory by producing the clone. All subsequent experiments with GFP expression and fusion proteins worked much better than might have been anticipated in the 1990s. Yet, he failed in every other respect in his quest in science. He lost grants and jobs. The Prasher story is cautionary, emphasizing the imperfections of the peer review process. Unfortunately, like democracy, we have no better system. As competition for grants and positions intensifies, success sometimes depends on abilities to project, market and manage science and to network effectively in a complex community. Prasher, evidently, appears to have fallen by the wayside in an intensely competitive environment. Nevertheless, his research has illuminated the workings of living cells and there is still hope that the warm glow of GFP and the aftermath of missing out on the Nobel prize will light his path in the future.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Economics, Modelling and the Financial Crash

Every year when the placements at the Indian Institutes of Management (IIM) are complete, the newspapers carry reports of the remarkable salaries offered to some members of the graduating class. A career path which seems most rewarding is one that takes a student from the Indian Institutes of Technology (IIT) through the IIMs and on to an investment banking firm on Wall Street. My admiration for the modern day banker, of both the conventional and investment varieties has been unbounded; conditioned by own upbringing. Having grown up, decades ago, in a financially conservative, middle class environment, I have been taught to avoid taking loans (although one is sometimes compelled to give them) and to view credit cards with suspicion. Spending money that one does not have and to spend time coveting that which is unaffordable were activities that were best avoided. Clearly, these are not attributes that are compatible with the modern age. Banks are no longer safe havens for depositing one's money; they are instruments which provide loans to feed every desire; borrowing and lending are practices to be encouraged and large profits are generated by banks when the going is good. I would never have worried about banks and bankers if the current global financial crisis had not exploded with such intensity. The sudden collapse of once famous investment banks must intrigue even laymen, like me, who are completely unschooled in the ways of high finance. Curiously, not long before the stock market collapse of October, I had heard colleagues proposing a course in mathematical finance. This seemed an area that was completely distinct from the 'finance courses' normally taught to students of accountancy. Over a decade ago, courses in financial mathematics became popular in the United States and the term 'financial engineering' began to appear both in journals and the popular press. The use of the suffix 'engineering' in terms that describe new and emerging disciplines can sometimes be a pointer to future controversies; genetic engineering and geoengineering are examples. For nearly twenty years Wall Street investment banks have hired Ph Ds in physics and mathematics to man their 'derivatives trading desk'. These quantitative analysts ('quants') are key figures in the investment banking firms that are now in the limelight. Has a completely misplaced faith in quantitative modelling led financial institutions in the West to the brink of disaster? The consequences of the collapse of banks and insurance companies, which provide risk covers for loans, can damage large numbers of individuals and societies. In the globalized economy the shocks are felt almost everywhere. Should not the subject of 'financial engineering' and the assumptions that underlie quantitative analysis be scrutinized more rigorously and the application of the tools of the trade be regulated with greater diligence?

The journal *Nature* recently published a provocative essay, which argues that 'economics needs a scientific revolution'. The author, Jean-Phillipe Bouchaud, notes that 'compared with physics the quantitative success of economic sciences has been disappointing'. He asks: 'What is the flagship achievement of economics? Only its recurrent inability to predict and avert crises, including the current worldwide credit crunch'. He recalls a view advanced by Newton that 'modelling the madness of people is more difficult than modelling the motion of planets'. Physicists who have entered the field of finance and economics come armed with techniques that allow the prediction of the behaviour of large collections of particles and molecules, even though the individuals seem to move chaotically. Order can emerge from complexity and chaos. Bouchaud emphasizes that 'the crucial difference between modelling in physics and economics lies . . . in how the fields treat the relative roles of concepts, equations and empirical data'. His thesis that 'financial engineers have put too much faith in untested axioms and faulty models' will undoubtedly stir debate. His verdict on financial markets may find many supporters in the present situation, where talk of stock market crashes and impending recession is widely heard: 'In reality, markets are not efficient, humans tend to be over-focused in the short term and blind in the long-term, and errors get amplified, ultimately leading to collective irrationality, panic and crashes. Free markets are wild markets' (Bouchaud, J.-P., *Nature*, 2008, **455**, 1181).

The alarm bells warning the financial industry of the dangers of naively believing the methods and results obtained by 'modellers' have also been rung by the *New Scientist*. An editorial column, headlined 'Blinded by sci-

ence: Financial regulators have allowed themselves to be bamboozled', does not mince words in heaping criticism on the quantitative analysts. The column notes that the collapse of a hedge fund called 'Long-Term Capital Management' exactly ten years ago should have alerted the financial community. This collapse 'was particularly notable because its founders had set great store by their use of statistical models designed to keep tabs on the risks inherent in their investments'. The continued use of such models points to the fact that 'models' ostensibly based on the sound principles of physics and mathematics can be misleading when applied to complex areas of human activity. It would appear that the area of financial risk modelling may be resting on inherently weak foundations; key assumptions in models may sometimes be hidden from critical evaluation. The *New Scientist* assessment is harsh: 'Now that disaster has struck again, some financial risk modellers – the 'quants' who have wielded so much influence over modern banking – are saying they know where the gaps in their knowledge are and are promising to fill them. Should we trust them? Their track record does not inspire confidence. Statistical models have proved almost useless at predicting the killer risks for individual banks, and worse than useless when it comes to risks to the financial system as a whole'. The editorial is categoric in its advice: 'Banks should be careful not to assume that they have it right and the rest of the world has it wrong. And regulators – who have lately allowed themselves to be blinded by science – should have no qualms about shutting down activities they do not understand' (*New Scientist*, 2008, Sept. 27, p. 5). An accompanying article in the same issue of the journal attempts to analyse why risk modellers can be so wrong. The conclusion is that 'each liquidity crisis is inevitably different from its predecessors, not least because major crises provoke changes in the shape of markets, regulations and the behaviour of players'. Researchers in the area of risk assessment 'still lack the tools to predict when the next liquidity crisis will come' (Jameson, R., *New Scientist*, 2008, Sept. 27, pp. 8–9). What are the 'models' that researchers in 'mathematical finance' investigate? A quick search on the internet led me to a sample paper posted on the physics archive entitled 'Higher order potential forces observed in bubbles and crashes in financial markets' (Watanabe, K. *et al.*, arXiv:0808.3339v1, 25 August 2008). While I am ill equipped to understand such a paper the application of complex mathematical methods to predict financial crashes left me uneasy. Would the managements in financial institutions that invest their customer's money rely on modellers who are 'certain that the existence of a cubic potential function is a precursor of a big risk that might be encountered in the near future'.

Modelling and predicting the behaviour of monsoons, earthquakes, and financial markets is an inherently hazardous exercise. The parameters are many and the assumptions often untenable. Nevertheless, physicists and mathematicians have been employed in large numbers by Western financial institutions. Skills and familiarity with probability theory, calculus and partial differential equations have long been a pre-requisite for scientists and engineers to obtain well paid jobs in the financial sector. Nearly a decade ago a study commissioned by the UK's Engineering and Physical Sciences Research Council came to the conclusion that physicists are preferred over mathematicians. The reason advanced by a bank is compelling: 'Physicists want to find the answers to problems. Mathematicians have all the answers and want problems to solve' (*Physics World*, Editorial, 1999, June 3). Even a decade ago the gulf between academic research and the requirements of banks seemed unbridgable. There appears to have been limited progress in the years that have passed.

The connections between physics and finance go back a long way in capitalist or free market economies. The theory of Brownian motion has a precursor in Louis Bachelier's study of the fluctuations in the prices of stocks and shares, which he suggested 'could be viewed as a random walk' (*Physics World*, 1999, Jan. 1). The financial crash of 2008 has introduced lay readers to many new terms, 'sub-prime loans', 'option pricing' and 'derivatives' all of which seem critical in understanding what went wrong all of a sudden. Mathematical modellers are always on the look out for problems to solve. From biology to economics the same equations seem to surface. Analogies from physics can sometimes be enlightening, but are most often misleading. Eugene Wigner wondered many years ago about 'the unreasonable effectiveness of mathematics in physics'. Recent events suggest that it might be wise to reflect on the ineffectiveness of physics and mathematics in economics.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Revisiting the Origins of Life Experiment

Biology is a discipline that has two distinct faces. There is a remarkable diversity of form and function amongst living organisms. There is also an underlying unity of the molecular mechanisms that are responsible for the maintenance of life. Modern biology rests on a few major pillars; Darwin's evolutionary synthesis driven by the ideas of natural selection, Schwann's ideas of cells as a basic unit of biological structures, Mendelian rules of inheritance and the principles of coding and transmission of genetic information based on the Watson–Crick double helical structure of DNA. Biology is a wonderfully fertile field for research, with mathematicians, physicists, chemists, computer scientists, materials researchers and engineers entering the field, hoping to understand and sometimes exploit nature, using tools and techniques that have been successful in other fields. There is a messy side to biology, where elegant theories and unifying concepts seem to make little headway; the subdiscipline of biochemistry remains a formidable maze of complex, interlinked chemical reactions. The advance of molecular biology has not simplified matters. Indeed, regulation and control of chemistry in biological systems is turning out to be mired in complexity. The developing area of systems biology may offer hope, but its promise remains to be realized. Physicists, mathematicians and surprisingly even geneticists dismiss the complexities of biochemistry as an unnecessary distraction in understanding biology. At the level of molecules there is again a diversity of form and function. The modern day 'structural biologist' sometimes bears a striking resemblance to the practitioners of classical systematic biology. The former applies the principles of classification, 'taxonomy', to molecules while the latter surveys nature's organisms. At a recent discussion I heard the dismissive characterization of those who work to catalog structural detail in biological molecules as 'people who have mistaken the trees for the forest'. As one who has undoubtedly lost his way amongst the 'trees', I was nonplussed. The criticism was probably valid. But, were the signposts pointed in the wrong direction in the reductionist approach to biology? Not too long ago it was popular to talk about the 'molecular basis of life'; a term that promised deep insights if the trail of reductionsim was followed to the very end. Biology is, however, a subject that has not yielded entirely to a reductionist strategy. Classicists often take shelter behind Dobzhansky's famous dictum: 'Nothing in biology makes sense except in the light of evolution'.

The Darwinian view has dominated discussions of biological evolution. For the reductionst there is a related issue. How did the enormous diversity of molecules, that are central to the chemistry of life, evolve? Darwin, of course, cautiously avoided a consideration of the origins of life, a chapter in evolutionary history that must surely precede speciation and the evolution of diversity. His oft-quoted allusion to a 'warm little pond' in which life originated was a passing acknowledgement of an unsolved problem. Chemical evolution must surely have taken place in a distant past, when the Earth's surface and atmosphere were far from conducive for life. Some answers are to be found in a classic paper, unpretentiously titled, 'A production of amino acids under possible primitive earth conditions' by Stanley Miller, published over half a century ago (*Science*, 1953, **117**, 528). Miller, then a graduate student at the University of Chicago devised an apparatus in which a mixture of water vapour (H_2O), ammonia (NH_3), methane (CH_4) and hydrogen (H_2) was subjected to an electric discharge. The residues formed revealed the presence of a variety of amino acids, small molecules that go on to form the polymeric chains of which proteins are composed. This was the first demonstration of the formation of the elementary molecules of biochemistry from a mixture of gases that might have constituted a primitive atmosphere on Earth. The Miller experiment, which was based on the ideas of Oparin, Urey and Haldane captured the public imagination and was quickly dubbed in the popular press as an experiment that revealed the 'origins of life'. Miller's little, matter of fact note, which occupies slightly more than one printed page, is modest in its claims, eschewing speculation. He provides a clear and succinct description of his experiment and the results, avoiding any reference to the origins of life.

My attention was drawn to this old experiment by the appearance of a recent report entitled 'The Miller Volcanic Spark Discharge Experiment', in which Miller's original samples were reanalysed using modern instru-

mentation – 'high-performance liquid chromatography and mass spectrometry that allows for identification at the sub-picomolar ($<10^{-12}$ M) level' (Johnson, A. P. *et al.*, *Science*, 2008, **322**, 404). The group led by Jeffrey Bada, a former student of Miller, writes: 'After Miller's death in May 2007 we found several boxes containing dried residues. Notebooks indicated that the vials came from the 1953–54 University of Chicago experiments that used three different configurations'. Modern day geoscientists have been skeptical that the Earth's primitive atmosphere had the reducing conditions that Miller had used. Bada and his colleagues now establish that one of Miller's original experimental configurations 'possibly simulates the spark discharge synthesis by lightning in a steam-rich volcanic eruption'. In this scenario the first building blocks of biochemistry may have been born in an environment that forced molecules into existence, while biology might still await Darwin's 'warm little pond'; a description that seems more conducive for life. A few years ago Bada and Lazcano wrote a historical essay, 'Prebiotic Soup – Revisiting the Miller Experiment', looking back on the 50th anniversary of the paper that launched the field of chemical evolution (*Science*, 2003, **300**, 745). They begin with lines from an old and forgotten song: 'Isn't life wonderful' and provide a marvellous account of Miller's work and its impact on the public imagination. Science as practised in the Miller's experiment, provided the 'metaphor of the "prebiotic soup" ' which later 'found its way into comic strips, cartoons, movies and novels and continues to do so'.

The genesis of the Miller experiment can be traced to a remarkable book written in Russian by A. I. Oparin in 1936 titled 'Origin of Life', with the English translation appearing two years later (Macmillan, 1939; Dover Publications, New York, 1953). Oparin's opening sentences summarize the importance of the area of inquiry: 'The question of the origin of life, of its first appearance on Earth, still occupies the human mind, as it has done since antiquity. It may be safely said that it is one of the most important problems of natural history'. Oparin surveyed chemical and biochemical knowledge, then available, in masterly fashion. He also noted in his conclusion, after 250 pages of discussion, the effects of biological evolution: 'Natural selection has long ago destroyed and completely wiped off the face of the Earth all intermediate forms of organization of primary colloidal systems and, wherever the external conditions are favourable to the evolution of life, we find countless numbers of fully developed highly organized living things. If organic matter would appear at the present time it could not evolve for very long because it would be quickly consumed and destroyed by the innumerable microorganisms inhabiting the earth, water and air'. Oparin clearly saw biology obliterating all traces of its chemical origins. Despite his reference to natural selection, the sole mention of Darwin (p. 45) is related to his dismissal of a view that 'living things' originated from inorganic substances without the intervention of any specific vital force. Oparin discussed the steps by which organic substances might evolve and reached a pessimistic conclusion: 'The tremendously long intervals of time separating the single steps in this process make it impossible to reproduce the process as it occurred in nature under available laboratory conditions' (p. 251). Less than twenty years later, Miller demonstrated the feasibility of Oparin's models for abiotic synthesis.

Miller began his research career as a graduate student in Harold Urey's laboratory at Chicago. Urey won the Nobel Prize in Chemistry in 1934 for his work on deuterium at Columbia University. When he moved to Chicago in 1946 his interests were turning to geochemistry. He reached a conclusion, not too different from that of Oparin, that a primitive atmosphere would be reducing, a view not supported entirely, today. In a memorable seminar, he suggested that such conditions might be favourable for producing organic compounds. Miller, who originally began working with Edward Teller and then changed advisors, sensed an opportunity to try an experiment. In later years he was to say that when he arrived in Chicago he was convinced that 'experiments are time-consuming, messy and not as important . . . as theoretical work'. He was quickly rewarded when he 'produced an oily scum and a yellow brown water solution' that yielded compounds of importance in biochemistry. In an appreciation of Miller, written after his death, A. Lazcano and J. L. Bada describe reactions when Miller presented his results at a seminar, 'with Urey in the front row'. The results were clearly important. 'In the question period Enrico Fermi turned to Urey and said, "I understand that you and Miller have demonstrated that this is one path by which life might have originated. Harold, do you think it was *the* way?" Urey replied, "Let me put it this way, Enrico. If God didn't do it this way, he overlooked a good bet!" ' (*Orig. Life Evol. Biosph.*, 2008, doi 10.1007/s11084-008-9145-2; Arnold, J. R. *et al.*, *Biogr. Mem. Natl. Acad. Sci. USA*, 1995, **68**, 363).

Decades after the Miller experiment, the origins of life and the evolution of the complex organic molecules essential for its sustenance remain shrouded in mystery. There is a remarkable simplicity and directness with which Miller approached an apparently intractable problem. Few Ph D students, before or after Miller, have begun their careers with an experiment that launched a new field. The results of his analysis have been borne out by the recent reinvestigation of the residues of his experiments. The new technologies of chemical analysis have revealed far more than Miller could have seen in the 1950s. Can there be a better example of the importance of preserving samples and records?

P. Balaram

CURRENT SCIENCE

Volume 95 Number 11 10 December 2008

EDITORIAL

This Journal's Loss

K. R. Rao, Associate Editor of this journal passed away on December 2, 2008. Krishnarao Raghavendra Rao was born on May 7, 1936 in Bangalore and studied physics at Mysore University, obtaining his B Sc in 1955 and M Sc in 1957. He joined the Training School of the Department of Atomic Energy (DAE) immediately thereafter and went on to spend his professional career at the Bhabha Atomic Research Centre, rising to the position of Head of the Solid State Physics Division. I first met Rao in early 1999, when Sivaraj Ramaseshan persuaded him to help us with the running of *Current Science*. The title of Consulting Editor was invented and Rao came on board in March 1999. I knew little of his professional attainments, but was immediately taken up by his soft-spoken demeanour and his attention to detail. He had little difficulty in interacting easily with younger colleagues and soon became a central figure in the operations of this journal. As I came to know him, I realized that he possessed a deep understanding of physics and a remarkable ability to explain concepts and principles to people like me, who have only a rudimentary knowledge of the subject. Over the years I had heard Ramaseshan talk on diverse topics, while I was the sole listener, sometimes at a loss to tell him that I simply did not understand the subjects he discussed. More recently, Rao took his place, attempting to educate me on topics that seemed formidable. For several years we met on Saturday afternoons, in the journal office, where in addition to the mundane matters of the editorial desk he told me about neutron scattering, optics and the science and politics of nuclear power. We discussed authors, reviewers and the ethics of publishing. He had little interest in scientometrics and citation indices and was the only person who did not impress upon me the need to raise the impact factor of *Current Science*. Rao's erudition often amazed me. I was reminded of Oliver Goldsmith's famous characterization of the village schoolmaster:

'And still they gazed, and still the wonder grew,
That one small head could carry all he knew.'
(*The Deserted Village*)

Krishnarao Raghavendra Rao (1936–2008)

In the last couple of years I saw him less often; this journal's office adopted the 'five-day week' and the Saturday meetings were gone forever.

Rao was a meticulous editor. His ability to concentrate permitted him to detect more cases of plagiarism than all the rest of us. In March this year, he wrote a piece entitled 'Plagiarism, a scourge' which ended with the rhetorical question: 'Are we not in interesting times?' (*Current Science*, 2008, **94**, 581). As a pragmatist, I often advised him to return such manuscripts citing some general ground for rejection. He would never agree; often taking upon himself the task of educating and reforming offending authors. He was a liberal editor, ready to accommodate eccentric authors, sometimes working hard to shepherd manuscripts through to publication. His efforts were not always appreciated by disagreeable authors and I have seen him puzzled and hurt by rude responses. Rao was not a battle hardened and cynical editor, most commonly encountered at journals. He was sensitive, cautious and helpful. He was a very careful writer who measured

every word and his contributions were always meticulously researched. In the years that I knew him, Rao sometimes seemed pained by the changing mores of science and scientists. His letter in this issue 'Losing out on Fellowship of Academies' is his last piece; expressing his growing concern over practices at India's Academies of science. He was not often given to expressing his opinion in print, but when he did there was no ambiguity in his writing.

Rao had a subtle sense of humour. As I undertook the somewhat melancholy task of looking through his files, I came across his response in March 2000 to a letter continuing his services at *Current Science* and offering him an enhanced honorarium. He responded: 'As George Gamow said a long time ago, "some money is always welcome".' In a little pad that he left behind I found a quote that he had painstakingly copied: 'When people retire, decide to take things easy and say they don't have to keep up with the world anymore, the danger of "brain death" starts. A vicious cycle sets in. If you don't do an activity anymore, you begin losing the skills to do it. Then you are even less likely to engage in those activities'. Rao was active to the very end. He lost his wife, Soni, in June after a long struggle. He worked from home for several weeks and returned to the office on November 28, but collapsed the next day and was gone two days later. All of us at this journal will miss his presence and I will sorely miss his counsel and advice.

P. Balaram

CURRENT SCIENCE

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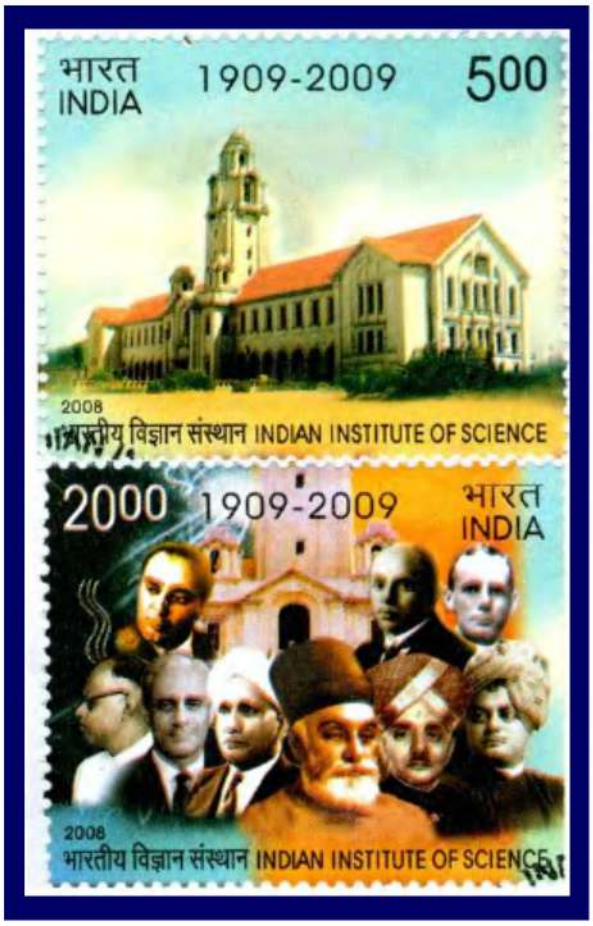
EDITORIAL

Burjorji Padshah: Realizing a Vision

The impending expansion of the number of central universities and institutes of technology and science comes fifty years after the establishment of the IITs, a century after the Indian Institute of Science (IISc) and a hundred and fifty years after the Universities at Bombay, Madras and Calcutta came into being. Each phase of the development of higher education and scientific research in India has happened at a critical point in history. The manner of establishment of these institutions, and the motivation for their creation have been widely different. Their evolution in the subsequent decades has also been distinct; the universities have blossomed and faded a little, while the IITs and IISc have weathered the passage of time and emerged as frontline institutions. An analysis of the growth and development of India's institutions may well prove instructive in planning the course for the current expansion. Can any useful lessons be learnt by studying the past? Embedded, as I have been, in the midst of IISc's centenary year, the past has loomed large, raising the question: 'Are anniversaries merely sentimental occasions for celebration or are they opportunities to reflect on history and ask questions about the future?'.



In seeking to preserve and document the history of the institution where I have worked for so long, a small group of colleagues joined me in attempting to put together an archives. IISc is a hundred years old and the preserved records are sparse. Cataloguing and organization have not been considered as necessary activities, adding to the enormity of the task at hand. We realized, very quickly, that 'collection' must precede 'cataloguing and preservation', while 'study and analysis' are best left to future scholars who might like to dig into the past. Even as we began our task of building an archives for IISc, I came across Francis Crick's dismissive characterization of the archivists and historians of molecular biology: 'The world seems to be made up of three kinds of people; Collectors who collect and don't always much care what. Scholars who hoard every scrap of paper. And people like myself who wonder why'. Our surroundings abound with people who share Crick's assessments of archivists and historians, making the task of collection sometimes unrewarding. But, digging into the past can on occasion reveal nuggets that provide insights into men and matters which are an integral part of institutional history.

The IISc centenary has been marked by the release of two postage stamps that provide a glimpse of an institution's past. Philatelists will undoubtedly search for legends to attach to these images. The building depicted on one stamp is of course the iconic 'tower' of the Institute; a structure erected nearly a century ago, its majesty undimmed by the passage of time. But it is the other stamp, featuring some of the men who built the institution and those who enriched it by their work, which reflects history. Most recognizably in the centre, with the tower in the background, is Jamsetji Nusserwanji Tata, the man whose vision for 20th century India was remarkable for its clarity and foresight. J. N. Tata's act of philanthropy in creating an endowment for setting up an institution for research has been unmatched in the century that has followed. The Maharaja

of Mysore, whose generous gift of land and promise of financial support brought IISc to Bangalore and Swami Vivekananda, who provided J. N. Tata with inspiration and moral support, are easily recognizable. Some of the men who worked at the Institute over the decades and contributed to its reputation as a centre for research must be easy to name: C. V. Raman, Satish Dhawan, G. N. Ramachandran and Homi Bhabha. Featured on the top right of the stamp are two men who may be unrecongizable to many, but are key figures in the history of the Institute—Burjorji Padshah (1864–1941) and Morris Travers (1872–1961) the first Director (*Current Science*, 2008, **94**, 1109). Of the two, Padshah was undoubtedly the more remarkable man. The main building of IISc has a commemorative stone on which are inscribed the names of those who were formally associated with the governance of the institution at that time. The stone has been seasoned over the century and the inscription is hard to read. Among those whose names are etched for history there is that of B. J. Padshah. When I began the archival task a couple of years ago, I had seen the name and knew he was an associate of J. N. Tata. But I had little idea of the critical role he played in ensuring the birth of the Institute and no idea of his untiring efforts towards realizing J. N. Tata's vision for the development of research and industry in 20th century India.

Burjorji Jamasji Padshah came under J. N. Tata's wing even 'when he was a child running about the horse stables' (*TISCO Review*, August 1941, p. 613). Armed with a degree in philosophy (a background that may explain many of his views in charting a course for the infant IISc) from Elphinstone College, he became a professor at the Sind Arts College (later D. J. Sind College) in Karachi in 1887. An obituary notice in the *Sind Observer* (June 25, 1941) provides the titbit that Ratan, J. N. Tata's son, 'lived with Padshah at Karachi and was a student of the D. J. Sind College'. This obituary is remarkable for its account of Padshah's early career, noting that he held 'the record for the number of subjects he taught – Philosophy, History, English, Mathematics. . . . The Engineering Branch (which subsequently developed into a full fledged college) also received great encouragement at the hands of Prof. Padshah'. His erudition and wide ranging knowledge of a remarkable spectrum of subjects is noted in every account of Padshah's career. He was a prolific letter writer and traveller. Even casual inspection of his correspondence, some of it now available at IISc, reveals a man of formidable intellect who tenaciously pursued the goals set by J. N. Tata's vision for modern India. Padshah's contributions to the Karachi of his times may have been recognized by the naming of a road after him, although the obituary in the *Sind Observer* notes that 'the pioneering work he did for Karachi and Sind deserves a more suitable commemoration'.

Padshah's contributions to the establishment of IISc were first recognized in the early 1960s when a young American researcher, Kim Sebaly, came to India to study the factors contributing to the development of the newly established IITs. Like Columbus who set sail for India and discovered America instead, Sebaly found that the roots of technical education in India could be traced to the founding of IISc by J. N. Tata. In researching IISc's early history Sebaly discovered Burjorji Padshah. A well documented scholarly account appeared two decades after Sebaly's visit to India (Sebaly, K. P., *History of Education*, 1985, **14**, 117). Padshah was involved in all the major projects conceived by J. N. Tata, but executed after Tata's death in 1904; the hydroelectric project, IISc and the Jamshedpur steel venture. He worked hard but unsuccessfully in obtaining 'imperial status' for the technical institute at Jamshedpur, which was to provide metallurgical training to Indians in the 1920s and 1930s. In an analysis published two decades ago, Sebaly draws attention to 'Tata steel and higher technical education in India: The Padshah Plan 1916–1921' (*History of Education*, 1988, **17**, 309). He notes: 'In 1942 (one year after Padshah's death) the central government established the Council (Board) of Industrial Research which would eventually oversee the creation of the National Metallurgical Laboratory in Jamshedpur in 1950'.

Padshah emerges as an extraordinary figure who corresponded with Viceroys from Curzon to Willingdon, Gokhale, Gandhi (with whom he disagreed on satyagraha), Ratan and Dorab Tata. Descriptions of Padshah by those who knew him, highlight his encyclopaedic knowledge and his courteousness even in disagreement. For a man trained in philosophy, his abilities to manipulate and remember numbers, as recorded by his contemporaries, seem remarkable. At IISc in its early years Padshah seemed intent on nudging the institution to embark on studies in the social sciences and medicine. The 1898 document for the proposed University or Institute of Research included a 'Medical Department' and a 'Philosophical and Educational Department'. The latter was envisaged to engage in a wide variety of disciplines ranging from Indian History and Archaeology to Statistics and Economics. Padshah met resolute opposition to his ideas from the first Director of IISc, Morris Travers. Between 1907 and 1913 there was little agreement but Travers had launched the institution towards its eventual focus of science and engineering. Both men severed their association with IISc in 1914, in none too happy circumstances, but lived long enough to see the institution emerge from its early difficulties. A complete account of Padshah's life and times may reveal the strength of character, intellectual abilities, tenacity of purpose and personal commitment that are necessary in realizing a vision.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Anniversaries at the Academies

The major Academies of Science in India celebrate 75 years of existence this year. The Indian Academy of Sciences, Bangalore and the Indian National Science Academy (INSA) were born in 1934/1935 under far from happy circumstances. An editorial in this journal in 1933, written by C. R. Narayan Rao had outlined, succinctly and elegantly, the case for an Academy. C. V. Raman's move to establish an 'Indian Academy' at Bangalore, even as moves were underway in Calcutta to found the National Institute of Sciences (later to transform itself into INSA, with its base in Delhi), led to considerable controversy. Newspaper reports in 1934 reflect a level of discord that must interest historians of science even many decades later. Raman, who appears to have always followed his own instincts, uninfluenced by others, singlehandedly created and fostered the Indian Academy of Sciences from 1934, until his death in 1970. His remarkable commitment to science, charisma and brilliance ensured that Bangalore's Academy would follow an academic trajectory. In the years after Raman, many dedicated Fellows of the Academy, Sivaraj Ramaseshan notable amongst them, preserved and protected the Raman legacy. A distinctive feature of Bangalore's Academy is its focus on the production of science journals. Raman was a strong advocate of the need for India to sustain its scientific journals. In recent times the Academy has experimented with the introduction of new journals, *Resonance* pre-eminent among them, and in restructuring old journals. This journal has been closely associated with the Academy, although the Current Science Association is a distinct body established two years before the birth of the Academy. Meetings, workshops, teachers training programs and summer internships for students have all become an integral part of the activities of Bangalore's Academy. The Academy's quiet location contributes to its ambience; at times appearing far removed from the hustle and bustle of modern day science.

The Indian National Science Academy (INSA) based in Delhi is a few months younger and clearly the more 'official' Academy. INSA is India's window to international exchange and collaboration and the body that represents the country in the world's scientific bodies and councils. There is a whiff of Government in its corridors, an inescapable consequence of its location in the capital. INSA's earlier years have seen some of the most famous names in Indian science occupy the presidentship; M. N. Saha, J. C. Ghosh, S. S. Bhatnagar, S. N. Bose, K. S. Krishnan, P. C. Mahalanobis and H. J. Bhabha amongst them. Here the contrast with Bangalore is evident. Raman reigned far away from Delhi, unencumbered by connections to government, preoccupied principally with fundamental science. A third Academy appeared on the scene when the National Academy of Sciences was formed at Allahabad in 1936. A provincial academy had metamorphosed into a national body. Some years ago after I had begun to write regularly in these columns, a distinguished past president of one of the academies asked me why we had three of them. This rhetorical question, to which no answer was expected, prompted me to examine the 'profusion of academies' (*Current Science*, 1999, 77, 4). A decade later the question remains, resurfacing as the academies of science prepare to celebrate their anniversaries.

Over the years academies have acquired the status of elite clubs, which admit to their membership scientists who satisfy specified academic criteria. Election to the academies confers a level of peer recognition that is much sought after. Unfortunately, the processes of selection can never be universally acclaimed. Nominations and elections dominate the proceedings of the academies. Scientists who have not been admitted to the club by a certain age find their chances slipping as the years pass; youth is an important parameter which sometimes overshadows accomplishment and scholarship. Promise and patronage can outweigh performance. In Raman's days the decision making process was simple. He judged individuals. The process is more complicated now as the subdisciplines of science have increased and become ever more specialized. Judgements are made by committees and councils in a complex process. As the number of scientists has grown, decision making is an increasingly hazardous task. The academies have a difficult problem in assessing nominations in interdisciplinary areas. Nominees often fall between two stools; disciplinary boundaries are jealously guarded. There are separate academies for engineering and medicine, although a fairly large number of clinical researchers and engineers are also fellows of the science academies. The Fellowship of the academies has become increasingly coveted following the decision by

government to provide a cash incentive to researchers who are members of more than two approved academies. This is a development which might have left the founders of the academies bemused.

In recent times the science academies have begun to cooperate in many science promotion activities; a welcome change from the early days. This transformation has become possible since a large number of fellows belong to more than one academy, sometimes holding office in multiple places, thereby facilitating cooperation. The report on the four year science degree program and suggestions for higher education in science are examples of inter-academy initiatives that are becoming common. At a recent meeting in Bangalore, the Indian Academy of Sciences considered the celebratory program planned for the platinum jubilee year. Several past presidents reflected on their experiences and the role of the Academy. There appeared to be a general feeling that academies are unable to have a significant influence on science policy of the government. Some noted the reluctance of the academy to take up a clearly stated position on issues of public importance. It is clear, of course, that on some issues there is only one acceptable position. For example, it is easy to state that the academy disapproves of plagiarism or data falsification. On the other hand, it is not clear if there can be one and only one clearly articulated position on the Indo-US nuclear deal. A starting point might be to prepare well presented reports on difficult issues which may permit informed judgement. Can the academy's report on restructuring the undergraduate science programs prove influential? In order for any reform to be effected the report needs wide discussion in the UGC, in the universities and colleges where the programs are conducted. Unfortunately, this may not happen easily as there are few champions for a restructuring process in these institutions. The representation of university teachers in the academies is also limited, restricting the reach of well prepared, well intentioned reports. Academy reports that present and analyse data in scholarly fashion may indeed fare better than reports that are prescriptive, although dissemination is always limited. Critical surveys of problems and opportunities in emerging fields may also find a receptive audience.

The anniversary is a good time for the academies to reflect on their past and to redefine their goals for the future. Would it be pragmatic to avoid seeking a greater role for the academies in influencing government policy and focus instead on promoting science, an activity that the Academy at Bangalore has so successfully done over the years?

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Good governance, accountability and responsibility

As an avid reader of newspapers I have always turned first to the sports pages, after a glance at the front page reassures me that no major disaster has befallen the world. The 24 hour TV news channels and the Internet have diminished the novelty of the morning paper. The print medium seems important only when a careful reading and re-reading is necessary. Innocent of the ways of high finance, business and industry, I have generally avoided the financial pages and the specialist financial newspapers. Curiously, I was once asked to write about Linus Pauling, shortly after his death. My unrewarded effort occupied a considerable portion of the centre pages of the *Economic Times*; a piece that I suspect was largely unnoticed and unread. Two financial newspapers are now delivered to my doorstep, on the presumption that those who head major institutions must be literate in the ways of high finance. For the last few months however, economics, financial institutions and corporations have captured the attention of lay persons, in a manner that could scarcely have been anticipated. The collapse of investment banks, the rumours of impending troubles at major conventional banks, some of which appear to have lent large sums of money to inherently risky enterprises, the turmoil in the stock markets and the loss of employment in corporations as recession takes hold, have brought finance to the front pages. The extraordinary happenings at Satyam Computers, an iconic company in the brave new world of information technology, have made for riveting reading. The fall from grace of a once celebrated leader of industry has been amazingly rapid, sobering and, at times, even saddening. The newspapers and magazines have now provided tutorials on auditors, internal and external, the roles of independent directors of companies, balance sheets with fictitious entries and the mysterious ways in which large sums of money can be spirited away, with little hope of easy recovery.

A notable feature of the Satyam episode is the sudden interest it has raised in the functioning of corporate India. Even as the economy has boomed, private industry has viewed with some disdain, sluggish public institutions which are mired in bureaucracy and slow to respond to the needs of a modern, rapidly expanding economy. Government institutions have often been plagued by petty corruption and inefficiency. Corporates seem agile and accountable to management boards and shareholders. The phrases 'corporate governance' and 'corporate social responsibility' have emerged into public view, with the growth of the new wave industries in information technology and biotechnology. Neither of these terms seemed popular decades ago when some of the standard bearers of Indian industry engaged with society in constructive philanthropy. In a curious twist, Satyam won two awards last year which celebrate the company's achievements in corporate governance (Golden Peacock Global Award) and corporate social responsibility (UK Trade and Investments India Business Award). Clearly, the knowledgeable watchers of the business scene had little inkling of the brittleness of Satyam's façade. There is a parallel here in the world of science. Awards, peer recognition and public acclaim have sometimes gone to scientists who have been guilty of extravagant claims and fabrication of results. The examples of the cases at the erstwhile Bell Telephone Laboratories and the Korean stem cell scandal are still fresh in memory. Awards and rewards are sought and prized, but are not always a true indicator of honest achievement. The tendency of well meaning bodies to recognize and applaud high performers after a cursory evaluation process can sometimes result in embarrassment.

The Satyam saga has led to the expected breast beating about the failure of the Indian regulatory system, which monitors the functioning of corporations. There is the often unstated assumption that regulators elsewhere, particularly in the West, do better. The critics forget that some of the most brazen and visible financial scams in recent times have occurred in the United States. The latest investment scandal has been dubbed the 'Madoff mystery'. Even a financially naïve reader like me could immediately appreciate the simplicity of a scheme 'in which new capital raised was partly used to pay off old investors so that they could earn a stable and reasonable return, independent of fluctuations in the market' (Chandrasekhar, C. P., *Frontline*, Jan. 14, 2009, pp. 47–48). This strategy must, of course, eventually unravel revealing a fraudulent enterprise. There is an old principle that is often forgotten in investment; it is dangerous to be seduced by high returns if one does not understand the process by which apparently huge profits are made.

A similar unease must be felt when laboratories produce a succession of spectacular results which appear impossible in other hands. Financial fraud and misconduct in science may indeed be compared. In the former, there is an outcry from investors whose money has been misappropriated and misused. Modern financial fraud appears to be a sophisticated exercise practised by highly educated individuals. Bank robbers seem to practice a more straightforward craft and achieve the same result. Detection necessarily results in criminal proceedings. Stealing money has always been a crime. The ends of justice and the demands of accountability are met when the guilty are imprisoned. In science, the detection of misconduct and the penalties to be imposed are less well defined. The sense of injury is felt more by those who believe that the frailties of human nature do not extend to scientists. When allegations of fraud in scientific research arise there is the inevitable outcry that the Indian system of ensuring transparency of investigation and oversight is far inferior to the systems in place in the West. Curiously, even in the recent Satyam case the same charge has been made against the Indian financial regulators.

Since I suffer from the weakness of drawing parallels between the world of science and the happenings elsewhere, my attention was quickly drawn to a provocatively titled article 'It's in the DNA', addressing Satyam's fall from grace. The author, a pillar of India's biotechnology industry, argues that 'more regulation is not the answer to the Satyam scam' (Kiran Mazumdar, *The Times of India*, Jan. 15, 2009, p. 14). Her thesis is compelling: 'I am tempted as a biologist to draw parallels between the diagnosis and treatment of disease with that of regulating against bad governance. In pharmaceutical parlance, biomarkers are used to evaluate a disease, whether it is at an early stage or advanced stage. Perhaps we need to develop markers that provide us with early warning signals of poor or inadequate governance.' Auditors are the key in institutions, if financial impropriety is to be detected before it gets out of hand. The author draws a parallel to cancer, where late detection leads to metastasis. Companies and institutions can succumb if the cancer of fraud is allowed to go undetected and untreated. Mazumdar notes 'that good governance can add long term value to an organization and that there is clear evidence that poor governance is a value destroyer'. Her conclusion may be of some interest to the growing tribe of genome researchers: 'I believe that the answer lies in deciding the good and bad genes that make up the DNA of any company'. I could not help wondering if there was an intrinsic weakness in corporations dominated by families; a defect that amplifies with the generations. Even in universities and research institutions the deleterious effects of 'in-breeding' are apparent, when the accumulation of the academic offspring of a once productive and capable leader can result in perceptible decline.

It is tempting to draw parallels between the transgressions of scientists and wrongdoing in the world of commerce. Plagiarism is the simplest and most easily detected offence in science, analogous to forgery although the latter can have more dramatic consequences. Fabrication of data is conceptually similar to the non-existent fixed deposits that appear as assets in creatively produced balance sheets. There are many different ways in which results have been fabricated in the celebrated cases of scientific fraud. Undoubtedly, there are multiple ways in which financial statements can be doctored. Indeed misconduct in science and financial fraud differ significantly only in their end effects. In science, perpetrators are driven by blind ambition. When misconduct is detected there is usually a sense of moral outrage, a feeling that the purity of science has been sullied. In the world of business fraud seems an almost inevitable result of insatiable greed. Unfortunately, as science and industry move closer the profit motive can sometimes obscure values even amongst the most capable of scientists. Ethical barriers can be breached when commercial interests are dominant. There have been several instances in the area of clinical trials and the marketing of new pharmaceuticals. In these situations good governance and social responsibility are more often observed in the breach.

Accountability, transparency, responsibility and good governance are terms that are easily used. It is much harder to ensure that they are indeed understood and applied in the running of corporations, institutions, states and countries. Ambition and avarice are not uncommon qualities. Together they form a perfect recipe for disaster when present in those who hold positions of responsibility.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science and Engineering, Theory and Experiment

Addressing gatherings at formal functions and listening to speeches are an occupational hazard for those who hold official positions. In recent times I have sat, uncomfortably and awkwardly, on the dais on many occasions. The declining interest of students in degree courses in science is a common topic on which many speakers hold forth at convocations and institutional days. This is also a topic that is a perennial favourite in committees which worry about the state of science and higher education in our country. Curiously, many committees honestly believe that a well meaning and well drafted set of recommendations may be sufficient to transform the system of science education. There is no doubt that colleges in some states are indeed starved of students for B Sc degrees in physics, chemistry and mathematics. Biology seems to do better by adopting a simple device. College managements rename any course with a moderate level of biology as 'biotechnology', conjuring up visions of an 'engineering' or 'professional' degree. If a B Tech degree is awarded, the transformation of biology into a subdiscipline of engineering and technology is complete. Both students and parents are comforted by the prospect of a degree that certifies 'technological' competence, upon completion of a course of study. Even entry into the 'information technology' industry seems more facile if one has an engineering degree, although it is not entirely clear as to the technical background that is considered most desirable by those who hire for IT companies. Public statements, which pronounce most engineering graduates as 'unemployable', imply that considerable retraining for specific tasks is necessary. The significantly greater employability of engineering, management and commerce graduates undoubtedly makes these courses more attractive to students. Their proliferation has often been advanced as one of the many reasons for the gradual extinction of science courses in colleges.

From my vantage point, I have often wondered: What are the differences between science and engineering? Is it not true that science and engineering are two sides of the same coin? What causes the huge difference in perception at the level of college degrees and why is this gulf less evident in the great universities of the West? Are the growing number of computer modellers, 'engineers'? Why are 'computer science' departments so named, especially when they are always staffed by faculty with engineering degrees? Curiously, it is this area which is most sought after by students in many institutions, its attraction undimmed by association with science. Several decades ago, before the electronic and digital revolutions hit science like a tsunami, it was easy to differentiate engineering students from those who studied science. The former carried T-squares and slide rules and spent time in workshops and were even taught to operate lathes. The latter carried 'log tables' and went to practical classes, which involved considerable physical labour. In the pre-computer era, both science and engineering courses seemed to emphasize experimental work, as a critical component of training. Students of physics seemed closer in their mathematical and analytical abilities to budding engineers. Chemistry and biology students on an average seemed less comfortable with equations and their solutions. I must hasten to add that I write this just after hearing an illustrious seminar speaker in a chemistry department display a cartoon which proclaimed: 'Into every life a little math must fall'. But, the chemists made up for their mathematical innocence by learning the many painful and arduous procedures of practical chemistry and were sometimes even instructed in the art of glass blowing, a rare skill in the modern age. The biologists were specialists in dissection, with frogs and cockroaches as their subjects, in an era where there was little opposition to such experiments. There was a common thread to all the undergraduate courses and that was the emphasis on practical training. For those who ploughed on to enter a research career, the lessons of practical classes have been invaluable. It is here that one learnt that experiments sometimes failed, even when one desperately wanted the right result in an examination, under the watchful eyes of an external examiner. It is here that one picked up the little secrets of the trade from experienced laboratory attenders, mechanics and demonstrators. I have watched the contents of a burette gradually slip through a leaky stopcock onto the table top in a practical examination forever reminding me, in a recurrent nightmare, of the importance of attention to detail in an experiment (and indeed in every other activity). I suspect that practical classes are much less rigorous nowadays for both science and engineering students. Even a cursory glance at most

college laboratories today will reinforce this feeling. There is a new future that epitomises the modern age. All colleges, even the most poorly equipped, boast of a 'computer laboratory', with dozens of desktop computers (laptops in some places) stacked in neat arrays in well furnished rooms. The distinctions between science and engineering students seem to blur in the computer classroom.

Coincidentally, I came across an article entitled 'Scientists as Inventors' (Petroski, H., *American Scientist*, 2008, **96**, 368), which draws attention to a distinction between scientists and engineers, attributed to Theodore von Karman. In many ways von Karman's definition of a scientist as one who 'seeks to understand what is' and an engineer as one who 'seeks to create what never was' does not correctly describe today's researcher in science and engineering. Petroski is quick to acknowledge that 'often considered distinct, engineering and science are frequently difficult to distinguish'. A long time ago there was indeed little distinction. There were scientists and inventors and disciplinary boundaries were much less pronounced. Michael Faraday can indeed be claimed by physicists, chemists and electrical engineers as one of their own. Louis Pasteur was an organic chemist, a microbiologist and a biotechnologist. Then there were the inventors: George Stephenson, Thomas Edison, Alexander Bell and Nikola Tesla among others. J. C. Bose was a physicist, biologist, physiologist and inventor, but these terms were much less well defined in his time. In his essay, Petroski highlights the story of Einstein as an inventor. He notes that 'Einstein himself made numerous forays into the form of engineering known as design and invention'. Einstein held many patents for practical devices, undoubtedly helped by his early experience in a patent office. His partner in later years in his excursions into 'engineering' was Leo Szilard who was, as Petroski notes, 'capable of working on scientific and engineering problems virtually simultaneously'. The 'Einstein–Szilard refrigerator' was the testing ground on which some of the technologies for later day nuclear reactors were conceived. Petroski concludes his essay by observing that 'science and engineering are – and always have been – coequal partners in the development of the world of thought and things that define civilization and culture'.

In institutions like my own, the Indian Institute of Science, where research is a prime focus, the distinctions between scientists and engineers are indeed blurred. Are there two distinct species of researchers who can be identified, whose characteristics mark them out as decidedly different? I might venture to suggest that a contemporary classification may separate theorists from those who do experiments. Even in the engineering departments which once boasted of large halls full of clanking machinery, the workshops are largely still and silent. The mechanics are largely gone, glassblowers nearly extinct and draughtsmen swept into memory by advancing technology. The lathes stand forlorn and unattended and large structures one used for experiments serve solely to remind us of an era long gone. I have seen many an old engineer look pensively at these relics of a rapidly fading past reminding me of Goldsmith's lament on the disappearance of rural England:

> 'Where once the cottage stood, the hawthorn grew,
> Remembrance wakes with all her busy train
> Swells at my breast, and turns the past to pain.'

Computer modelling is the key thread which binds diverse disciplines. The structural integrity of buildings and bridges, the design of molecules and machines, the simulation of monsoons and blood flows, the analysis of networks both electrical and biological appear to be drawn together by high performance computing. There are indeed few theorists who walk around with paper and pencil, armed only with mathematical skills and physical concepts. The terms computational chemistry and biology describe an increasing tribe of researchers far removed from the pain, excitement and thrill of experiment. In the area of materials research, scientists and engineers work on similar problems sometimes claiming that their approaches are different; practitioners of religions into which they were inducted at an early age.

The winds of change have long swept over the frontiers of science and engineering, destroying walls and eliminating boundaries. In the new world of research, success may require a facility to easily bridge the gaps between disciplines. It may indeed be critical to think of new experiments in undergraduate education, where science and engineering merge seamlessly to build a new generation of professionals and researchers.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Darwin's Champions

In the last two weeks I heard two seminars, in which the speakers projected images of Charles Darwin and quoted from his writings. Since 12 February marked the 200th anniversary of Darwin's birth, tributes to him were not unexpected. What did surprise me was that one of the speakers was an eminent chemist, while the other was a mechanical engineer interested in 'fracture'. Darwin's reach had clearly extended well beyond organismal biology, in the century and a half since he unveiled his theory of evolution in 1859. In an editorial entitled 'Humanity and Evolution' that appears in the February 12 issue of *Nature* (p. 763), attention is drawn to the fact that Darwin and Abraham Lincoln were born on the same day, two centuries ago. The editorial notes that 'these men shared more than just a birthday. . . . They shared a position on one of the great issues of their age: the "peculiar and powerful interest" of their fellow humans bound in slavery. When he circled the world in the 1830s, Darwin's delight at our planet's natural riches was repeatedly poisoned by the cruelties he saw meted out to slaves. "I thank God, I shall never again visit a slave country", he wrote at the end of the *Voyage of the Beagle*' (*Nature*, 2009, **457**, 763). The theory of evolution and the principle of natural selection have sometimes been extended to many spheres of human activity, sometimes in a manner that is certainly counter to Darwin's philosophies. Evolution, in the common perception, is most often associated with the inappropriate phrase 'survival of the fittest', whose origins can be traced to the writings of Darwin's contemporary, Alfred Russel Wallace. Totalitarian and repressive political regimes have found this a useful term. Darwin's ideas on biological evolution have always had opponents, although the major debates have long passed. 'Intelligent designers' are a minority group, who have been banished from any serious consideration by mainstream science. Curiously, any attempt to discuss 'intelligent design' is viewed as 'sacrilege' by the scientific establishment, as evidenced by the recent furore at the Royal Society. In a provocative commentary in this journal, Rustum Roy attacks the view of those who 'fearlessly equate the whole future, indeed the very survival of science to the acceptance (whatever that means) of Darwinian evolution as true (whatever that means to them)'. He goes on to add: 'The fact is that even though most non-biologists (like me) take the general concept of evolution as pretty solid, they never use it. "Evolution" is a tiny corner of, and hardly equal to, "science". It is *never* encountered in the entire world of physics, chemistry and engineering and is hardly used even in most biological research papers' (Roy, R., *Curr. Sci.*, 2009, **96**, 340). Roy has undoubtedly overstated his case, but he does draw attention to those who overstate the importance of Darwinian concepts in relation to the large world of science.

How important is the theory of evolution and natural selection in the world outside biology? Mark Pagel provides some interesting facts in an assessment of 'natural selection 150 years on' (*Nature*, 2009, **457**, 808). He notes that 'Darwin used the word "evolve" just once in *On the Origin of Species* and even then he waited till the final word of the book. Instead, Darwin wrote of "descent with modification" '. Pagel illustrates the growing usage of the term 'natural selection' in fields that are distant from biology. He uses the term 'natural selection' to search papers listed under separate subject areas in the ISI Web of Knowledge database in November 2008 and produces a subjectwise distribution of the occurrence of the term. The distribution is 'wide and long tailed'. Interestingly there are significantly large occurrences in fields like mathematics, computational biology and computer science. Engineering and chemistry also make an appearance, while the physics literature does not seem to have an appreciable presence of the term 'natural selection'. Darwin's ideas have gained acceptance over the course of more than a century; every challenge countered by formidable champions of the theory of evolution. Beginning with Thomas Henry Huxley, Darwin has had an illustrious line of supporters and interpreters, who have shepherded 'natural selection' through decades of attack by 'creationists' and distortion by ideologues. Even as the Darwin bicentenary was attracting attention, a happy coincidence brought two collections of essays by J. B. S. Haldane (1892–1964) and Stephen Jay Gould (1941–2002) to my table. Both authors have written extensively on biology and evolution, although the collections before me have only a small selection of their writings.

The volume entitled *What I Require from Life* (K. Dronamraju, K. (ed.), Oxford University Press, 2009) puts together 57 short essays by Haldane written largely

in the 1940s and 1950s. In a wide ranging lecture on *Relations between biology and the other sciences*', delivered at the 39th Indian Science Congress in Calcutta in 1953, he quotes from Francis Darwin's reminiscences of his father Charles Darwin: 'I used to like to hear him admire the beauty of a flower; it was a kind of gratitude to the flower itself, and a personal love of its delicate form and colour. I think he sometimes fused together his admiration of the structure of a flower and its intrinsic beauty'. There is also a short piece on *Darwin in Indian perspective* (1958). Haldane reflects on the fact that 'Christian theologians had drawn a sharp distinction between men and other living beings', whereas in 'India and China this distinction has not been made and according to Hindu, Buddhist and Jain ethics, animals have rights and duties'. Indeed, Darwin's ideas have met with great resistance only in the West, particularly in conservative Christian communities. Haldane has an interesting assessment of Darwin. He notes that: 'If Darwin had died young, Wallace would presumably have promulgated the theory of evolution by natural selection when he did, and it would probably have been accepted, though as Wallace's arguments covered a smaller field than Darwin's, the acceptance might have been slower. And as Wallace left loopholes open for supernatural intervention, which Darwin did not, the immediate effect on Western thought might have been less'. Haldane argues that 'Darwin's most original contributions to biology are not the theory of evolution but his great series of books on experimental botany published in the latter part of his life'. In Haldane's view, Darwin's analysis of 'those aspects of plant life which are most like animal and human life' leads to the discovery of facts which are 'momentous'. The applications that have followed include 'the discovery of plant hormones and the invention of weed killers which resemble them chemically, and the systematic outbreeding of maize, of whose importance for the agriculture of the USA I need not write' (p. 212). Haldane points to the last chapter of *Origins* as the starting point for 'a logic based on differences', foreshadowing the application of statistics to population biology and indeed, all other sciences. He concludes his broad assessment: 'Darwin was too great a man to assess just yet. In each succeeding generation new aspects of his work appear important'. Half a century after Haldane, the scholars are still at work on Darwin.

Stephen Gould was arguably one of the finest science writers of the second half of the 20th century. He died prematurely in 2002. An anthology of his writings *The Richness of Life: The Essential Stephen Jay Gould* (McGarr, P. and Rose, S. (eds), Vintage Books, London, 2006), includes several classic essays on evolutionary theory, size, form and shape, sociobiology, racism, religion and some biographical and autobiographical pieces. In their introduction the editors note that 'as a scientific essayist Gould's only peers were "Darwin's bulldog" Thomas Huxley in the nineteenth century and J. B. S. Haldane in the 1930s and 1940s'. The editors further note that: 'The comparison with Haldane is apt in two further ways. Both made fundamental contributions to evolutionary theory and both were politically engaged within science and in the broader political arena. In Gould's case these more fundamental contributions appeared in their turn in a number of major books, beginning in 1977 with *Ontogeny and Phylogeny* and culminating in his last great work, the 1400-page *Structure of Evolutionary Theory*, published only months before his death. Any one of these major works should have assured Gould an honoured place amongst evolutionary theorists. Yet, unlike Haldane, whose contributions to what became known as "the modern synthesis" of Darwinism with Mendelian genetics, is uncontested, Gould's remains controversial. . . What some saw as his Darwinian revisionism, but he would have regarded as extending evolutionary theory in a direction that the great pluralist Darwin himself would have approved of, earned him obloquy amongst more orthodox ultra-Darwinists. . . . And in some ways Gould's own felicity with words, his hugely popular essays and—to put it positively—self-assurance, won him enemies as well as friends' (p. 2). In this collection of Gould's writings he returns to Darwin many times. He has the remarkable ability to research an obscure fact and turn out a compelling narrative. One example, *The Darwinian Gentleman at Marx's Funeral: Resolving Evolution's Oddest Coupling*, reflects on the presence of an evolutionary biologist, E. Ray Lankester at the Highgate Cemetery on 17 March 1883, when Karl Marx was buried. Gould quotes Friedrich Engles who said, somewhat inappropriately, on that occasion: 'Just as Darwin discovered the law of evolution in organic nature, so Marx discovered the law of evolution in human history' (p. 167). Gould's piece on *The Evolution of Life on the Earth* is a clear statement of a long standing and poorly understood problem in science. He is emphatic that we will not 'complete Darwin's revolution until we find, grasp and accept another way of drawing life's history'. Gould quotes Haldane who 'proclaimed nature "queerer than we can suppose" '. There is a great deal that needs to be understood in the evolution of life and its origins on earth. There is little doubt that a hundred years later, the Darwin tricentenary will provide another opportunity to celebrate advances in evolutionary biology and marvel once again at Darwin's insights.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Biology in India: Through the Looking Glass

'The time has come,' the Walrus said,
'To talk of many things:
Of shoes – and ships – and sealing wax –
Of cabbages and Kings –
And why the sea is boiling hot –
And whether pigs have wings.'

—Lewis Carroll
Through the Looking Glass

In Lewis Carroll's magical sequel to *Alice's Adventures in Wonderland*, Alice speaks to her kitten: 'Now, if you'll only attend Kitty, and not talk so much, I'll tell you all my ideas about Looking-glass House. First, there's the room you can see through the glass – that's just the same as our drawing-room, only the things go the other way. I can see all of it when I get upon a chair – all but the bit just behind the fireplace. Oh! I do so wish I could see *that* bit.' When Alice dreams her way through the looking glass she notices 'that what could be seen from the old room was quite common and uninteresting, but that all the rest was as different as possible'. It is here that Alice encounters Carroll's immortal creations, Humpty Dumpty, Tweedledum and Tweedledee and the White Knight. I was reminded of Alice when I encountered the February 9 issue of *The Journal of Cell Biology* (*JCB*). Shimmering in the background behind the Taj Mahal was double-stranded DNA, stretching infinitely into the horizon. The journal's cover welcomed readers 'Inside Indian Bioscience'. Even as I wondered about the connection between one of the most enduring symbols of India's past and DNA, I found the article to which attention was drawn by the cover designer, *The Biological Sciences in India: Aiming High for the Future* (Vale, R. D. and Dell, K., *JCB*, 2009, **184**, 342). The authors who spent an 8-month sabbatical at the National Centre for Biological Sciences (NCBS), a unit of the Tata Institute of Fundamental Research (TIFR), provide an overview of the 'past, present and future of Indian bioscience'. As I quickly leafed through the article I found photographs and two line sketches of 'leaders in Indian biology'. The selection seemed curious. I could not help wondering if, like Alice, the authors were portraying the characters they had encountered. To my dismay, despite not being sighted by the authors, I found my own photograph on the last page, conjuring up visions of Humpty Dumpty. Normally, I would have allowed the matter to rest if I had not received a submission to *Current Science* entitled 'Indian biological sciences: aiming even higher' (see Ramaswami, M., p. 639, this issue). The author characterizes 'Vale and Dell's loud selection of 14 Indian scientists as leaders in Indian biology' as 'poorly considered'. This piece is a critique of sorts, describing the Vale–Dell article as 'limited, portraying as it did the status, achievements and challenges faced by Indian science, through a traditional western lens'. The author, presumably because of his closer association with Indian institutions, views biology in India through a lens which has a different tint. Now that 'Indian biology' is being examined through various lenses, one wonders how many views will emerge if the community looks inward, in an attempt to understand the past, assess the present and plot a strategy for the future.

The Vale–Dell *JCB* article and Ramaswami's critique must be read by biological scientists (and indeed scientists in other areas), for they represent the assessment of 'outsiders', which must undoubtedly be distorted by the prism through which they view their subjects. However, they do convey a feeling of optimism that is hard to generate from observers who are embedded in the Indian system. Vale and Dell suggest that biology in India took root much later than the physical and engineering sciences. For them, and I am sure their view is shared by many, the introduction of molecular genetics at TIFR in the early 1960s marks the foundation of a 'truly modern molecular biology research unit'. As an aside, I must note that in my early years as an observer of 'molecular biology', I was always mildly puzzled by the total absence of molecules as chemical entities in any discussion. My own prejudices would have led me to point to G. N. Ramachandran's work on collagen in the early 1950s and his seminal work on protein conformations in the early 1960s, work that finds a mention in the Vale–Dell article, as a turning point in the evolution of modern molecular approaches to biological problems. This work emerged from a physics department of a conventional university and was remarkable for its anticipation of the role of interdisciplinary approaches to the problems of biology. In some ways the 1950s did see the parallel evolution of two distinct

schools of 'new biology'; the structural approaches of Linus Pauling and the Cambridge crystallographers on the one hand and the geneticists of the Delbrück school on the other. The early work in India was indeed of a high order, comparable in many ways to cutting edge research in the West. Much has changed in the intervening decades. Biological research has exploded in all directions and the Indian research landscape has altered dramatically. Science in India has emerged from an era of limited funding and poor infrastructure only in the last twenty years or so. As new institutions have been built, modern laboratories staffed by young researchers have provided a fresh thrust to biology research in India. It is this changing research ambience that Vale and Dell attempt to capture.

The Vale–Dell view of the origins of biological research in India is limited; disregarding in one stroke, much of the base of biochemistry and classical biology that did exist for many decades. They note that, 'until the 1960s biological research was largely directed towards pragmatic applications in agriculture, nutrition and public health'. Curiously it is these very areas that are still dominant in the minds of those who hope to see that biological research is indeed translated into solutions for real-life problems. Unfortunately the foundation that existed in biochemistry and 'classical biology' was swept away when the molecular genetics revolution exploded in the 1970s. Molecular biology in India often distanced itself from its classical roots in botany, zoology and systematic microbiology in this period and was hesitant in building bridges to the chemical and physical sciences. The reintegration of biology in the wake of the genomics revolution in the 1990s and the coalescence of the broad biological concepts of natural selection, mechanisms of inheritance and the chemical basis of information transfer, together with the spectacular advances in the technologies of experimental research have raised new and formidable challenges for the practice of competitive and cutting edge research in biology.

For Indian biology to aim 'high' or 'even higher' an understanding of some constraints may be valuable. Vale and Dell point to some of these. They note that the total number of faculty in the biological sciences in 20 of India's key institutions is 'less than the number of faculty holding NIH grants at the University of California, San Francisco'. The number of postdoctoral fellows in Indian laboratories is very small and they note that the 'University of California, San Francisco alone has ~1100 postdoctoral fellows', a number considerably larger than the total number in all the modern biology laboratories in India. While 'postdocs' are the major labour force in US laboratories, Indian counterparts rely on graduate (Ph D) students. Training is an important activity and it is this group that moves on to Western laboratories where they perform very well. Another component that is largely missing in Indian institutions are skilled technicians. There is little appreciation for developing and promoting technical skills which have become critical to the advance of biological research. Even as Indian biology laboratories acquire sophisticated instrumentation there is a growing awareness that the full potential of the new facilities will not be realized unless there are 'technical experts', who understand their craft. There is a common failing which results in equating maintenance and operation with an understanding of complex methodologies. It is hard to see a dramatic change in the near future, unless there is a move to encourage technical proficiency. At the level of entry to biological research the problem of preparation of undergraduates needs to be addressed. The ubiquitous 'biotechnology' courses appear to be doing considerable damage by failing to teach the requisite chemistry, physics and mathematics. Indeed, it is this poor foundation that sometimes acts as a limitation in keeping up with the latest advances in biological research. In the 1980s Arthur Kornberg once lamented, in characteristically eloquent fashion that 'molecular biology in its turbulent advance washed away the bridges to cellular chemistry'. The integration of disciplines has since been established quite firmly in the West. This is still to happen in India. Indeed the creation of many new, small institutions as 'islands of excellence' has promoted over-specialization of faculty and students. The external view of Indian biology is provocative. Viewing ourselves through the looking glass may yield some interesting insights.

I must yield to the temptation of ending, as I began, with Lewis Carroll, although this may be an unwarranted digression. Some readers may wonder: 'Did he have any link to biology?' Curiously enough in *Looking Glass*, Alice wonders aloud to her kitten: 'How would you like to live in Looking-glass House, Kitty? I wonder if they would give you milk there? Perhaps Looking-glass milk isn't good for you.' This is indeed a reference to Pasteur's spectacular work on organic stereochemistry in the 1850s. Lewis Carroll in 1872 may have anticipated the consequences of chirality in biological recognition. For readers who are historically inclined the Van't Hoff-LeBel ideas were to appear in 1874 and Emil Fischer's 'lock and key' hypothesis was over two decades away.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Speculation, Simulation and Common Sense

In the context of financial markets 'speculation' is a well understood word. Speculators attempt to guess trends in short term price fluctuations of specific shares, hoping to buy at a low price, while selling at a higher price. Good guesses yield profits, while the penalties for bad guesses are clear. In science, 'speculation' is a word that is not always respectable. Referees and editors are often quick to dismiss unsupported conjectures and hypotheses as 'speculative'.

Indeed the words 'conjecture', 'hypothesis' and 'theory' can often be used in curious ways in writing about science. An untestable 'hypothesis' or one that requires an impossible investment of time, money and technology is hardly helpful to the progress of science. Almost by definition science is advanced by testable hypotheses. Speculation in science is often an attempt to expand and enlarge understanding of fields, that have grown by interpretation of well established bodies of facts. Hard evidence is needed in science before a 'hypothesis' graduates to the level of an accepted idea. When a broad conceptual framework is established, the words 'theory' and 'law' enter scientific discourse. Newton's Laws of Motion and Einstein's Theory of Relativity are familiar examples. The Darwinian synthesis, which forms the conceptual underpinning of biology has at various times during its evolution been referred to as a 'hypothesis' and a 'theory'. The word 'conjecture' is common in mathematics, where I suspect the word 'hypothesis' has no place. Words and their usage can be fascinating, sometimes revealing interesting insights into different fields and their practitioners.

I was drawn to this discussion of words by a recent essay entitled 'Predicting Molecules – More Realism Please!' (Hoffmann, R., Schleyer, P. von R. and Schaefer III, H. F., *Angew Chem. Int. Ed. Engl.*, 2008, **47**, 7164). The authors, all eminent computational chemists, seem worried about the word 'stability', which is used to argue for the existence of strange, new molecules predicted by theoretical chemistry, empowered in recent times by powerful computers and even more by readily available suites of programs, which permit a remarkable range of properties to be calculated. Most outsiders to the field of chemistry (and I suspect a significant number of insiders) believe that chemistry was, is and must remain an experimental science. There is surely a greater joy in finding an unexpected molecule as a reaction product or in a natural mixture (although the former may dismay a purposeful synthetic chemist), than in conjuring up 'hypothetical' molecules on a computer screen. Despite the joys of experiment, the inroads of 'theoreticians' and 'speculators' into chemistry have been broad and deep. To Hoffmann *et al.* the fact, that 'the body of computations of molecules for which there is as yet no experimental evidence is growing very rapidly', is 'simply wonderful'. The authors note these predictions are a 'marker of the reliability of theory' and indeed they are. These computational exercises are suggested to be 'sociologically' valuable 'in creating a tense and fruitful balance between theory and synthesis in chemistry'. It is the word 'stability' and its usage in the literature of computational chemistry that seems to leave the authors mildly concerned. For Indians today the word 'stability' seems particularly important with respect to governments after elections. Parliamentary majorities determine stability. Coalitions can often be metastable. In the world of molecules there are significantly different interpretations of the term 'stability' when viewed from the somewhat distinct standpoints of thermodynamics and kinetics. Unfortunately as Hoffmann *et al.* point out 'in our minds and in everyday discourse, stability in the thermodynamic sense merges with stability in the kinetic persistence sense'. For substances which can occupy a large 'structure space' there are minima of energy into which a molecule can descend. The deepest is, of course, the most 'stable' state, in accordance with thermodynamics. On occasion molecules fall into other wells, but are prevented from escape by energetically insurmountable walls, literally incarcerated in a 'kinetic trap'. These are the 'metastable' states. A political analogy would be the stability of a government that lacks a parliamentary majority but survives nevertheless for considerable periods of time. Hoffmann *et al.* note that it would be difficult to 'get jewelers to call their diamonds metastable', an allusion to the lower thermodynamic 'stability' of this glamorous form of carbon, as compared to its more common counterpart. Hoffmann *et al.* suggest that computational chemists who speculate on the existence of novel molecular species should use the common English words 'viable' and 'fleeting'. The former

would be applicable to persistent species, which survive under normal laboratory conditions. The latter would describe transitory species that are trapped under special conditions or are observable in unusual circumstances, like interstellar space. The authors acknowledge that 'experimental communities are inherently skeptical of the claims of theoreticians' and hope that a more circumspect claim for 'stability' of newly predicted species may be helpful in enhancing the credibility of computational chemistry. Their appeal for realism is engagingly phrased: 'A degree of realism in what is calculated would help allay the skepticism of experimentalists. And such realism is also consistent with the strain of humility which characterizes any honest spiritual activity. Such as science.' Since computer 'simulations' are now a part of everyday research in chemistry, both theoreticians and experimentalists alike should read the essay by Hoffmann *et al.*

There are many areas of science where speculative (sometimes wildly so) theories abound. The 'origins of life' area and the attendant problem of the origin of molecular asymmetry (chirality) in biology are fertile fields for speculators. Curiously, a recent essay on this area attracted my attention largely because of its wonderfully provocative title: '"If Pigs Could Fly" Chemistry: A Tutorial on the Principle of Microscopic Reversibility' (Blackmond, D. G., *Angew Chem. Int. Ed. Engl.*, 2009, **48**, 2). The speculators on the issue of how life originated (or more precisely the evolution of biological molecules and catalytic cycles) on earth are divided into two distinct camps; the 'geneticists' who believe in a primordial 'RNA world' and the 'metabolists' who 'believe that complex transformations characteristic of enzymes might have occurred prior to the evolution of informational molecules'. Blackmond notes that the 'tension' that exists in this area is reminiscent of Hoffmann *et al.*'s characterization of the 'tense and fruitful balance between theory and synthesis in chemistry'. Having quoted from Lewis Carroll in my last column, I was drawn inevitably to the phrase 'if pigs could fly chemistry' in the title of Blackmond's essay. This has been borrowed from the conclusions of an essay on 'The Implausibility of Metabolic Cycles on the Prebiotic Earth' by the late Leslie Orgel, a leading figure in the 'origins of life' area (*PLOS Biol.*, 2008, **6**, e18). Orgel, who in Blackmond's words 'sat in the geneticists camp' had a cautious view of theories that attempt to explain life's origin. In concluding his essay, that was published after his death, he suggested that 'solutions offered by supporters of geneticist or metabolist scenarios that are dependent on "if pigs could fly" hypothetical chemistry are unlikely to help'. The allusion here is to a famous exchange in *Alice's Adventures in Wonderland*, which is reproduced in Blackmond's essay: ' " I've a right to think" said Alice sharply, for she was beginning to feel a little worried. "Just about as much right," said the Duchess, "as pigs have to fly" '.

The Blackmond essay deals with hypothetical reaction schemes that are advanced to explain the origins of homochirality in nature. The author argues that 'models based on hypothetical reactions with no experimental corroboration have been shown to be in violation of a fundamental chemical principle', namely that of microscopic reversibility of chemical reaction at equilibrium. The origins of chirality and life have been the subject of much fanciful speculation. 'Hypothetical chemistry' may indeed be on much more treacherous ground than 'computational chemistry', but both Hoffmann *et al.* and Blackmond make strong pleas for common sense. In discussing 'significant figures in theoretical calculations' and the oft used terms of 'accuracy' and 'precision' the authors note that 'it's pretty much common sense. As is science'. Blackmond endorses this view noting 'that the reporting of non-experimentally corroborated conclusions carries with it a special responsibility ... and that common sense can be a practical aid'. Reaction schemes that attempt to rationalize the evolution of homochirality from an initially symmetric (racemic) world often postulate unusual behaviour. Blackmond suggests that 'common sense should guide the search for experimental systems exhibiting such behaviour; todate the balance between experiment and prediction has been more tense and fruitful.'

There is undoubtedly a need for speculation and hypotheses in science. It is sometimes fashionable to dismiss some areas of experimental research by arguing that they are not 'hypothesis driven'. There is still some merit in observation and careful cataloguing in many fields of science. An excessive reliance on deceptively sophisticated computational approaches in chemistry can be misleading. I cannot resist borrowing the last lines of Blackmond's essay—'As the Cheshire cat said to Alice: "If you don't know where you are going, any road will take you there." '

P. Balaram

CURRENT SCIENCE

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EDITORIAL

IITs: Two Histories

Editors are normally uncomfortable with abbreviations and acronyms in the titles of articles. After some thought and some misgiving I chose the title of this column. IIT seemed so much more familiar than the formal expansion – Indian Institute of Technology. I was emboldened by the use of this abbreviation in the titles of two excellent books on these institutions, which appeared in quick succession on my desk. *Monastery, Sanctuary, Laboratory: 50 years of IIT Bombay* by Rohit Manchanda (Macmillan India, 2008) and *An Eye for Excellence: Fifty Innovative Years of IIT Kanpur* by E. C. Subbarao (HarperCollins Publishers, India, 2008) look back on an eventful half century in the lives of two of India's best known institutions in the areas of engineering and science. The authors see their institutions from unique perspectives. Manchanda, a writer of uncommon talent and a young faculty member at IIT Bombay, views institutional history with a sense of professional detachment that is both valuable and instructive. Subbarao, a senior scientist of great distinction and an early pioneer at IIT Kanpur, provides an 'insider's view', which is at times both warm and personal. The IITs are truly creations of independent India, whose rapid rise to prominence within two decades of their establishment in the 1950s must rank as one of the outstanding achievements of the sector of higher technical education in India. In thinking of the IITs and the circumstances of their birth over half a century ago, I cannot help recalling Jawaharlal Nehru's words in 1956 at IIT Kharagpur, where he described the institution as 'representing India's urges, India's future in the making'. Nehru, more than any leader since, had a vision of a scientifically and technologically resurgent India. Manchanda begins his wonderfully crafted account of IIT Bombay going back to a March day in 1959 when Nehru arrived to formally lay the foundation stone for the institution at Powai, on the outskirts of the city that has now been renamed as Mumbai. He quotes Nehru: 'I suppose that among the many things that are being done in India today, the establishment of the great institutes of technical training and knowledge is perhaps the most important… . It is relatively easy to put up a factory or a plant, it is much more difficult and it takes much more time to train the human beings that will run a factory or put up another factory or plant'. Nehru went on to describe the event as 'another symbol of our attempt to grasp the future'. That future is here. In one of the many discussions on television about politicians, in the run up to the general elections, I recently watched a persistent and voluble anchor attempt to elicit an answer to the question 'whom do you admire among all the politicians in India since independence?' The audience was a group of young college students, many about to vote for the first time. After a long period of hushed whispers, smiles, nudges and some laughter one of the bolder students ventured the answer 'Mahatma Gandhi'. The anchor seemed relieved and moved on to criticizing the present, a somewhat easier task than reflecting dispassionately on the past. Nehru's fascination for scientists and technocrats (a word unknown in his time) and his romantic view of science, presumably influenced by his time in Cambridge, fuelled much of the scientific and technological development that was to take place in India after 1947. The Nehruvian vision created the generations of scientists and engineers, who have manned every institution that has come up in the last few decades. Nehru has, of course, been largely forgotten, but the institutions he helped create are visible symbols of an attempt to transform India. In an assessment of the Nehru years (1947–1964), albeit in the context of a different subject, the historian Ramachandra Guha notes: 'Yet more progress had probably been made in the first seventeen years of Indian independence than in the previous seventeen hundred' (*India After Gandhi*, Picador, London, 2007, p. 384). This conclusion may well apply to the sphere of higher technical education, where the IITs began to have a decisive influence by the 1960s.

Over the years the IITs have grown in stature and in public perception these institutions are preeminent in the field of undergraduate engineering education. They have remained unchallenged by the growing number of private and public institutions. In the new phase of expansion of higher technical education, in the ongoing 11th Five Year Plan, many new IITs are springing up, mentored and midwifed by the original group that have now been seasoned by half a century of existence. In his preface, Manchanda thoughtfully draws attention to the differing views of the IITs from 'without and within'. He notes: 'Although the IITs, especially of late, have become unapproachable objects of desire for those who seek their

stamp, to some of those who work, study, and live in them there has frequently been disappointment at the institutes' inner state of affairs... what is closer to realty, is the fact that the IITs despite their shortfalls, yet remain in a league of their own – then again a rather rarefied league – compared with the vast majority of Indian technical institutions, be it for rigor of education, evenhanded administration, inclusiveness of decision making or freedom of academic action' (p. x). The insider's view of institutions is always unflattering and outsiders most often seem to have a highly glamourised view, a phenomenon that is not restricted to the IITs. The two histories of the IITs must be required reading for those who wonder how the many new institutions, now on the anvil, will be built. They may prove inspirational for those who will struggle to build them. They will also undoubtedly educate those who study and work in these institutions on the processes by which academic settings evolve under the sometimes opposing demands of internal aspirations and external pressures.

The IITs at Bombay and Kanpur were born at the height of the Cold War. In keeping with the spirit of Nehruvian non-alignment, the institution at Bombay began with assistance from the Soviet Union, while Kanpur was supported by a consortium of some of the best known American Universities. Curiously, the true founder of the two institutions was one man, Purushottam Kashinath Kelkar (1909–1990). We are now in the centenary of P. K. Kelkar's birth and this may be an opportune moment to remember one of India's unsung institution builders. While Kelkar is revered by the early group of faculty he recruited at Kanpur, memories of his contributions are rapidly fading. The books by Manchanda and Subbarao serve to remind us of the debt of gratitude that succeeding generations of IIT faculty and students at Bombay and Kanpur owe this remarkable man. Kelkar was an alumnus of the Electrical Engineering Department at the Indian Institute of Science (IISc), Bangalore and later served on the faculty for several years, before moving to the Victoria Jubilee Technical Institute (VJTI), Bombay in 1943 (S. Ranganathan, *Current Science*, 1991, **60**, 185). I saw Kelkar momentarily and from a distance as a student at IIT Kanpur. All accounts of him suggest that he must have been an extraordinary man. The institutions he planned and built at Bombay and Kanpur stand as monuments to a man of very special talents. Kelkar was appointed the 'planning officer' for IIT Bombay in 1956 and began his work from one room at VJTI. By the time he left to become the first Director of IIT Kanpur, the Institute at Bombay had been launched. The transformation of Powai had begun, the first band of faculty had joined and the first batch of students had entered in July 1958. Kelkar repeated his magic at Kanpur in an environment that must have appeared formidably inhospitable to an advanced technical institution. Working out of makeshift rooms at the Harcourt Butler Technical Institute, Kelkar built an institution which brought an unprecedented degree of freshness and enthusiasm to the Indian academic scene. It was really with the flowering of the IITs in the 1960s that the winds of change began to blow in academic settings across the country. Kelkar's abilities in recruiting and encouraging colleagues must have been the key to the rapid growth of both the IITs. From 1970 to 1974 Kelkar returned to lead the institution at Bombay, to cap a remarkable career. Subbarao paints a warm and sympathetic portrait of Kelkar, quoting P. C. Kapur's tribute in 1990: 'But it was his value system – his simplicity, patience, commitment to excellence, humane and caring nature – that made him a legend in his own life time' (p. 19).

In the years that have passed since their inception the IITs have encountered and surmounted problems. Even as their impact on the international scene has increased, so have the number of critics. The Joint Entrance Examination has now spawned a vastly successful commercial enterprise, the 'coaching industry'. The need to emphasize research and postgraduate education has been pointed out by review committees. The ongoing expansion of student strengths poses logistical issues, even as the recruitment of faculty becomes more difficult. The burden of mentoring new institutions is a fresh challenge. Subbarao in treating a difficult period in the 1970s in IIT Kanpur's history observes sombrely: 'There appears to be a curse that condemns Indian academia to a life of excellence of just a decade or two before these institutions start to decay'. He goes on to describe the last two decades at IIT Kanpur as the beginning of a renaissance, opening the section with a quote from Churchill: 'Success is the ability to go from one failure to another with no loss of enthusiasm'. Success and failure, triumph and defeat, hope and despair are part of both individual and institutional histories. The stories of the two IITs are uplifting and inspirational.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Recession, Depression and Expansion

Economics invests common words with new meanings. As the economic slump is explained in innumerable newspaper columns, the terms 'recession' and 'depression' have acquired great significance. The dictionary defines 'recession' in an economic context as 'a mild but widespread slowdown in economic activity'. 'Depression' appears to be a more serious ailment being defined as 'a period during which business and employment decline or remain at a low level of activity'. As global economic indicators lurch downwards many columnists remind us of the 1930s and the 'Great Depression', an economic downturn of a magnitude that has never since been experienced in the United States. Historians of economics and political analysts now revisit the turbulent years of the 1930s, a decade which must remain etched in history as one of the 20th century's most critical periods. In today's globally interconnected economy an American 'slump' necessarily influences distant countries; the virus of recession and economic instability is quickly transmitted. The ongoing recession has brought in its wake a new prominence to unfamiliar words; 'deflation', a term used only in the context of tyres, now describes a rare economic trend. The elections and the Indian Premier League seem to have successfully diverted attention in India from gloomy economic forecasts, providing a recess from discussions of economic indicators and their interpretations. Economics and political ideologies are sometimes closely linked, with the result that even the most eloquently argued analyses are viewed with suspicion by ideological opponents. Statistics are quoted, misquoted, interpreted and misinterpreted in a manner that seems to set economic analysis distinctly apart from the sciences. However, a consensus view has emerged that the global economy is ailing, in the grip of a recession whose duration is hard to predict. Will a recession deepen into a 'depression', a condition that appears more serious by dictionary definitions? In reading and listening to the current discussions of economic issues, as a lay outsider, I have been struck by the fact that the public perception of economics and economists must be better informed and that the role of public financial institutions, banks and insurance companies amongst others must be better understood. In India the relationship between the private sector and government has become increasingly onesided; *corporate responsibility* must be a well defined and clearly appreciated term if governments are to use public resources in times of distress, especially when complete freedom of action is demanded in more comfortable times.

In thinking about the present economic situation an analogy from the world of medicine is helpful. For clinicians, the understanding of the causative factors in a disease is often the key to treatment and, more importantly, the key to prevention. A simple minded extrapolation suggests that this must be true for the ills of economies. Where did the virus which has caused the present global economic recession originate? For virologists this will be a familiar question, reminding them of the rise and spread of AIDS, whose causative agent the human immunodeficiency virus (HIV) appeared rather suddenly on the scene in the 1980s. The origins of HIV, its evolutionary history and its future remain a fascinating subject of study. In a curious coincidence the discoverers of HIV, Luc Montagnier and Francoise Barré-Sinoussi received the Nobel Prize for Physiology or Medicine last year at the same time that Paul Krugman was awarded the Nobel Memorial Prize in Economic Sciences. Krugman, an economist at Princeton University is even more widely known and read as a columnist for *The New York Times*. Readers of *The Hindu* will find Krugman's essays on the Op-Ed pages, topical, provocative and entertaining. Krugman's Nobel award recognises his contributions to 'trade theory' and his insights into the basic forces that shape economic activities across countries, giving rise to an 'economic geography'. Krugman is also an engaging writer whose themes may have been somewhat distant from the concerns of lay readers in India, until the global economic crisis began to loom ominously. The term 'meltdown' now appearing with great frequency in columns by economists might remind scientists of nuclear reactors heading into an emergency. In a recent column, attractively titled 'Making Banking Boring' (*The New York Times*, 10 April 2009), Krugman argues that the 'supersized financial sector' which was touted as 'the key to financial prosperity' has 'turned into the monster that ate the world economy'. In Krugman's view the attractiveness of finance and investment banking as careers has made 'wheeling and dealing' in the banking sector a sure recipe for disaster. For years now, outsiders to the area of management edu-

cation have watched with some disbelief the salaries drawn by engineers who by academic sleight of hand are converted into 'investment bankers', using risk analysis methods which may hardly be suitable to the real world. The traditional, conservative banker has been transformed over the years. Lending, and lending riskily has been the key to recent spurts of economic activity, even as uncovered debt has increased. Collapse of the frontline institutions is inevitable and a domino effect has already become visible. Krugman notes that the post-depression banking industry in America was 'tightly regulated, far less colourful... and far less lucrative for those who ran it. Banking became boring, partly because bankers were so conservative about lending'.

The recession has already begun to be a matter of concern for universities in the United States. The large private universities, which manage corpus funds, larger than the economies of small nations, have begun to experience a shrinking of their base of flexible capital. The Harvard endowment, the largest in the world, is reported to have shrunk by as much as $8 billion, forcing a rethink on expansion plans. Harvard is not alone, with several institutions considering salary and hiring freezes and increases in tuition fees for students, at a time when education loans are becoming tighter. Recessions also come with silver linings and some analysts expect that the demand for higher education will rise, as employment opportunities decline. Journals like *Science* and *Nature* have begun to carry reports on the effects of the recession on scientific research. Has India begun to worry about the recession or are we, as some would have us believe, 'insulated'? Will we wake up, after the two month long recess for the general elections, to find that an economic crisis is upon us? Will the ambitious plans for expansion of the higher education sector and the area of scientific research find the financial resources needed to carry the plans to fruition? Or will we find that resources are limited and that suboptimal funding retards the new institutions at their birth? Indian institutions do not have large reservoirs of corpus funds and necessarily maintain themselves on annual government grants. As expenditures mount on salaries and essential services, development and expansion must take a back seat. As the government's tax revenues show a downward trend it is hard to imagine that public funding of higher education and research will remain unaffected. It might be prudent to prepare for leaner days ahead, since belt tightening is an activity that is easier to do after an election.

When will the tides of the economic crisis recede? What is to be done? Krugman argues, in an essay in *The New York Review of Books* (2008, **55**, Dec. 18) that 'what the world needs right now is a rescue operation'. His prescription is one that is repeated almost daily by analysts: 'get credit flowing and prop up spending'. The first is more difficult in Krugman's view, but must be done. He argues for financial reform quoting John Maynard Keynes on the Great Depression: 'We have involved ourselves in a colossal muddle, having blundered in the control of a delicate machine, the working of which we do not understand'. In emphasizing the power of ideas he again quotes Keynes: 'Soon or late, it is ideas not vested interests, which are dangerous for good or evil'. Krugman argues against those who believe 'that our economic problems are structural with no quick cure available', by noting 'that the only important structural obstacles to world prosperity are the obsolete doctrines that clutter the minds of men'. I suspect that both communism and unrestrained capitalism spurred on by under regulated free markets may well be described as 'obsolete doctrines'. When will the recession end or has it already begun to retreat? In a recent column (*The New York Times*, 17 April 2009; *The Hindu*, 18 April 2009) entitled 'Green Shoots and Glimmers', Krugman borrows his title from two optimists, the Chairman of the US Federal Reserve (the equivalent of the Reserve Bank), who sees 'green shoots' and President Obama who 'sees a glimmer of hope'. He is more cautious warning that 'history shows that one of the greatest policy dangers, in the face of a severe economic slump, is premature optimism'.

In my idle reading of columns on economics I could not help wondering if monetary policy which involves central banks raising and lowering interest rates in response to economic indicators, was a conditioned response. There are other aspects of policy like tax cuts and tax breaks that are discussed leaving the ordinary reader mildly puzzled. In reflecting on issues that seem important but difficult to understand, I was reminded of a response to a scheme in the days of the US President Ronald Reagan which hoped that 'top end tax cuts would trickle down to produce unparalleled growth'. The late Harvard economist and former US Ambassador to India during the 1960s, John Kenneth Galbraith is quoted (R. Parker, *Boston Globe*, 7 May 2006) as commenting on the scheme in characteristically acerbic fashion: 'After feeding oats to the horses one should not gaze too closely on what trickles down to the sparrows'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

John Maddox: Promoting Science Through *Nature*

A few years ago, the turn of the century saw an explosion of writing looking retrospectively at the events of the period between 1900 and 2000. In 2003, a little book appeared, entitled *A Century of Nature* (L. Garwin and T. Lincoln, University of Chicago Press). To the undiscerning browser the title might have led to the conclusion that here was another book for the naturalist. The subtitle *Twenty one discoveries that changed science and the world*, establishes that this is a compendium of 21 papers from the pages of the science journal *Nature*, which are among the reports that defined the face of 20th century science. These papers are reproduced along with extremely readable 'explanatory essays', which provide a historical context for each major scientific advance featured in this collection. The book must be required reading for all those practising science and everyone else who is interested in the development of science and its place in furthering our understanding of the physical and natural worlds and ourselves. Of the 21 chosen articles, eight belong to the periods 1966–1973 and 1980–1995 when John Maddox was at the helm at *Nature*. Among these classics, we find the discoveries of pulsars (A. Hewish), reverse transcription which established that information in biology can flow from RNA to DNA (D. Baltimore and H. Temin), magnetic resonance imaging, then called zeugmatography (P. Lauterbur), the ozone hole (J. C. Farman, B. G. Gardiner and J. D. Shanklin), buckminsterfullerene, the C_{60} molecule, to be later described as the 'celestial sphere that fell to earth' (H. W. Kroto, R. F. Curl and R. E. Smalley) and the effects of mutation on development (C. Nusslein-Volhard and E. Wieschaus). The position of *Nature* as arguably the most influential publication in the world of contemporary science was established during the Maddox years. When Maddox began, *Nature* was considered as a 'science magazine' occupying a niche that was distinctly different from the staid, conservative journals produced by scientific societies. When he died on 12 April, the world of science journals lost one of the key figures in the transformation of science publishing, which took place in the 1980s and 1990s.

Nature is an unabashedly commercial enterprise. But its success and influence, in recent times, stem not from its circulation and revenue, but from the esteem in which it is held by authors and readers and from an editorial tradition that stretches over a century. Maddox transformed the journal even as science itself underwent dramatic changes in the last decades of the twentieth century. Although *Nature* is in every way an attractive model for an interdisciplinary science journal, its pages are not always readily opened to disciplines that seem to attract limited general attention. Biology, molecular, cellular and structural, the earth sciences and climate, astronomy and astrophysics are favoured subjects. Chemistry and many areas of physics are rarely featured on *Nature*'s pages, although these are disciplines in which a large number of journals and authors are to be found. Biochemistry, a once valued discipline and a forerunner of molecular biology was not too long ago magisterially dismissed in a *Nature* editorial as a subject 'associated primarily with the study of enzyme kinetics' (3 August 2006). In his preface to *A Century of Nature*, Steven Weinberg wryly notes: 'In this collection of articles from *Nature* one can read seminal articles from every field of twentieth century science. Every field, that is, except my own: elementary particle theory. For some reason particle physicists rarely submit their work to *Science* or *Nature*. (But *Nature* had its chance: Enrico Fermi submitted to *Nature* his great 1932 paper on the theory of beta decay, which founded the modern theory of weak interactions, but it was rejected).'

Nature came into existence in November 1869, published by Macmillan and edited by Norman Lockyer, who was at the helm for half a century. The journal's founding was aided by T. H. Huxley, then at the peak of his scientific influence. Lockyer was a colourful figure and his biography is entitled *Science and Controversy* (A. J. Meadows, MIT Press, 1972). His biographer notes: 'Lockyer like Huxley, thrived on controversy; but whereas Huxley generally came out the winner, Lockyer, in his scientific controversies at least, generally came out the loser'. It is in Lockyer's time that *Nature* established deep roots in the world of science, a periodical that allowed the communication of science to a broad audience and established a vehicle for communication between scientists; in a manner that seemed impossible in the conservative journals of the times. Lockyer's successor, Richard Gregory was to firmly establish *Nature* as the

journal in which many dramatic advances in science were recorded. He was aided in this task by the fact that in the period between the two World Wars, Britain and Germany were the two main centres of scientific activity. Gregory's criticism of the expulsion of Jewish scientists led to a decree banning *Nature* in Nazi Germany in 1937, a distinction of which the journal must be justly proud. The rapid rise of American science in the postwar world saw the emergence of a competitor, *Science*, the Journal of the American Association for the Advancement of Science, which today shares with *Nature* a preeminent position in the world of science journals. When the first editor of this journal, C. R. Narayan Rao began his effort to produce an interdisciplinary journal in India in the early 1930s, Richard Gregory was a strong supporter (*Current Science*, 2007, **92**, 5). *Nature* began in modest fashion in 1869. Lockyer undoubtedly an enthusiast and an optimist claimed that '*Nature* in the first year of its publication reached nearly 5000 subscribers ... '. His biographer notes that it was 'generally believed that Lockyer's estimate was a considerable exaggeration' and that the number 'may have been only between 100 and 200' (Meadows, p. 29).

John Maddox first came to *Nature* as editor in 1966, only the fourth editor in almost a hundred years. Lockyer had batted for fifty and Gregory for twenty years. In 1966 *Nature* was in the midst of a crisis that must seem familiar to editors, who enter to reverse the tide of decay. The most depressing sight in a journal's office (one that the electronic revolution has banished forever in most journals) are stacks of unattended manuscripts. In the 1960s Maddox found that the concept of peer review was not established at *Nature*. As the scientific enterprise expanded the cosy practice of informally consulting a few local eminences on the suitability of a manuscript did not seem practicable. In his parting editorial, *Valediction from an old hand*, Maddox repeats his apocryphal story of how the Watson–Crick paper on the DNA structure was accepted by *Nature* in 1953. A few phone calls to and by the then editor L. J. F. Brimble were enough to launch the double helix into history. Maddox describes a backlog of around 2000 manuscripts neatly stacked on a windowsill in the editor's office, 'arranged in piles, one for each month, providing a histogram of Brimble's problem soon to be mine. There were fourteen monthly piles when I first saw them' (J. Maddox, *Nature*, 1995, **378**, 521). Maddox, in retrospect, asks: 'The state of *Nature* in 1966 raises a raft of questions. Can, for example, a journal with such a reputation survive for long if scientific manuscripts are hardly ever refereed?'

Maddox engineered a remarkable turnaround. He introduced a brand of science journalism that was instantly attractive, boosting circulation and readership. Maddox introduced refereeing, although he noted in his last editorial 'that the confidentiality of the refereeing process breaks down only when it is important that it should not'. He reveals that in his first editorial stint he decided never to send papers by Louis Leakey and Fred Hoyle to referees, on the ground that referees would invariably reject them. According to Maddox, Leakey 'provoked otherwise reasonable colleagues to declare his paper not to be publishable'. Hoyle, he said, 'entertained and instructed the readers of *Nature* on matters as different as the function of Stonehenge and the nature of the universe. Most referees found him, "unsound", often without saying why'. The outpouring of obituaries on Maddox emphasize his love of controversy, plunging headlong into debates on issues that had an impact on science and its practice. His attack on Rupert Sheldrake's book *A New Science of Life: The Hypothesis of Causative Formation* (Park Street Press, Rochester) was famously titled 'A book for burning?'. Readers who normally do not go beyond headlines, assumed that Maddox was suggesting that the book be burnt, although he did go on to say that 'even bad books should not be burnt'. Even in retirement Maddox returned to attack Sheldrake's more recent work *Dogs That Know When Their Owners Are Coming Home: And Other Unexplained Powers of Animals* (Crown, 1999). He begins his critique charging that Sheldrake 'is steadfastly incorrigible in the particular sense that he persists in error' (*Nature*, 1999, **401**, 849). The publication of the paper by Jacques Benveniste on the 'memory effect' in aqueous solution, in 1988, and his subsequent investigation using a team that included a magician is the most dramatic example of his provocative approach as an editor. In the string of tributes that have appeared, Maddox is remembered warmly by his colleagues. William Grimes, writing in *The New York Times* notes: 'His enthusiasms were often far ranging, intense and brief, and his fondness for 11th-hour editing could be unnerving. He was renowned for leaving *Nature*'s editorial page blank until the last possible moment, then sitting down with a secretary and dictating his contributions as the presses rolled'. Maddox challenged and provoked his readers. In a brief essay entitled 'Is biology now part of physics', he was critical of the biologists' reluctance to embrace the ways of physics and chemistry. A one liner at the head of the article says it all: 'Reductionism is almost a dirty word, especially in biology, but after thirty years of DNA, it is high time that biologists paid attention to the question of what constitutes an explanation' (*Nature*, 1983, **306**, 311). A quarter of a century later the situation has not changed dramatically. As a regular reader of *Nature* during much of the Maddox years, his writings always left me entertained and informed. This must surely be a sentiment shared by legions of readers whom Maddox has left behind.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Metrics of Science: Loosening the Stranglehold

Counting citations, flaunting *h* indices and advertising average impact factors of publication lists have become a common practice amongst members of India's growing community of scientists and researchers. Modesty and reticence do not seem to be desirable qualities for career advancement in science, worldwide. The reclusive laboratory scientist is an extinct species, driven into oblivion by the rise of the aggressive, managerial professor. The administration of very large research teams and the need for constant projection of even preliminary results in conferences across the globe requires managerial and marketing skills, more suited to the world of commerce. In a recent lecture I heard a distinguished colleague looking back on research done decades ago, proclaiming wistfully at the end: 'it was fun'. Even as we collectively lament on the lack of enthusiasm of young students for science, there is little discussion of how our institutions and the researchers within them are perceived from the outside. 'Fun' may be a word that is hard to associate with a scientific community, obsessed with quantitative performance parameters; reminding cricket lovers of batsmen who play with an eye on averages. My thoughts turned to citation analysis following a recent episode where the Indian Academy of Sciences asked for scientometric parameters to be provided with the nomination form of candidates being considered for election to its Fellowship. Shortly after, the requirement was withdrawn, presumably because it seemed unbecoming of a body that should be able to assess scholarship by studying all the supporting material provided by the nominators. I would not have given a second thought to this episode if a colleague, distinguished by election to the Academy, had not walked into my office and charged me with some responsibility for the Academy's change of stance. He said somewhat accusingly: 'You have written favourably about the metrics of science, on occasion, and at other times dismissively about the obsessive use of citation counts. It is such fickle opinion that results in confusing readers'. I was momentarily flattered that someone should even think that casual essays can mould opinion. However, my reaction was somewhat cautious and I advanced the defence that none of my columns had promoted the use of citation statistics as a robust measure for assessing individuals. To further buttress my case, I argued that in any event famous moulders of public opinion had noted that 'consistency is the virtue of an ass'. But my critic was in no mood to listen, walking out scarcely listening to my mild expression of protest. My voluble critic seemed to like the *h*-index and seemed annoyed at the Academy's reluctance to endorse its use for evaluating scientists. Even as I wondered about the epidemic proportion of the obsession with scientometric indicators within the Indian scientific community, my attention was drawn to a new parameter with an impressive name – the eigenfactor. Having waded, somewhat unsuccessfully, through a world of eigenvalues and eigenvectors in my student days, this seemed a term that would undoubtedly be a force to reckon with; sweeping through the scientific community with a degree of infectious success, that would shame even the most prolific virus.

Over the years as a bemused observer of both scientists and the expanding discipline of scientometrics, I have often wondered about their relationship. Professional practitioners of scientometrics seem dedicated observers of science, developing parameters and methods to quantitate scientific output and impact. Scientists, their journals and their institutions use these parameters largely for self promotion, rarely for introspective analysis. Most professional scientometric analysts have little feeling for science itself, hoping that insights might emerge from impersonal quantitative methods. In recent times practising scientists, often physicists, have entered the field of scientometrics; bringing with them increasingly complex ways of analysing the exploding volume of scientific literature. In returning to the topic of the metrics of science, I must confess that over the last eight years or so this is a topic which has engaged me on half a dozen occasions. Most recently, a column was entitled 'Scientometrics: A Dismal Science' (*Curr. Sci.*, 2008, **95**, 431–432), clearly positioning me on the side of those who view quantitative measures with some suspicion. Despite my prejudices, I was drawn to a commentary irresistibly entitled 'The most influential journals: impact factor and eigenfactor' (Fersht, A., *Proc. Natl. Acad. Sci. USA*, 2009, **106**, 6883). The author asks if 'there is a simple statistical metric of the influence of a journal?'. He goes on to dismiss the 'impact factor' and 'total citations' arguing that both measures are affected by the total number of papers published in the journal. He then promotes the 'eigenfactor', a ranking procedure that is 'similar to that used by Google for ranking the importance of Web sites in a search'. Readers who wish to jump on the eigenfactor

bandwagon might like to read papers emanating from a biology department, which explain the new metric (Bergstrom, C. *et al.*, *J. Neurosci.*, 2008, **28**, 11433; Bergstrom, C. and West, J. D., *Neurology*, 2008, **71**, 1850). There is a strong correlation between the total number of citations and the eigenfactor, with *Nature*, *PNAS* and *Science* emerging as the journals with 'the most overall influence on science'. Biochemists and molecular biologists with a traditional upbringing will be pleased to note that the *Journal of Biological Chemistry* (*JBC*), an old warhorse, appears higher ranked than *Cell*, reversing their perceived importance. Unsurprisingly, *PNAS* chose to publish this analysis; an indication that even staid and venerable journals need to self-consciously promote themselves in a fiercely competitive world of publishing. If the 'eigenfactor' is a good single number indicator of a journal's influence, can the 'average eigenfactor' of a list of publications be far behind? Fersht worries about 'the terrible legacy' of the impact factor that is 'being used to evaluate scientists rather than journals'. He concludes that 'the least evil of the metrics of individual scientists is the *h*-index'.

When did the practice of ranking journals originate? Clearly, this must be a subject of some importance for librarians with limited budgets. An early attempt is described in a paper entitled 'College Libraries and Chemical Education', which addressed the issue of providing journals 'necessary for the stimulation and intellectual development of the faculty' in an undergraduate college. The authors noted that journals were needed because of 'the demand of the colleges for instructors with the doctorate degree. Such men are reluctant to accept positions in colleges where facilities for continuing the research which they have learned to love are lacking' (Gross, P. L. K. and Gross, E. M., *Science*, 1927, **66**, 385). This report, which was limited to chemistry journals, used a simple procedure. They selected *The Journal of the American Chemical Society* (*JACS*) as a standard ('most representative of American Chemistry') and counted citations in *JACS* to other journals as a measure of influence. The ranking that appeared over eight decades ago must interest analysts, with a weakness for history. The top five included three German journals (*Berichte*, *Annalen* and *Zeit. Physik. Chem.*), one French journal (*Compt. Rend.*) and only one English journal (*J. Chem. Soc.*). Many of these journals have disappeared, as the face of science has changed dramatically over the intervening years. Interestingly these authors discuss criteria for the purchase of 'back files', noting that the *JBC* should be provided for 'students looking forward to the study of medicine'. The authors noted in 1927 that such an analysis would be helpful in other fields. The subsequent introduction of the journal impact factor revolutionized the field of ranking journals. Ironically, libraries and librarians appear to be minimally interested in impact factors. Instead it is scientists who now use impact factors as a means of judging other scientists. As criticism of journal impact factors as a criterion of judgement has mounted, new performance indices based on citation counts have appeared, with the *h*-index currently ruling the roost. The area of scientometrics is now in the grip of two powerful commercial interests – Thomson Scientific, which now markets the *Web of Science* and Elsevier, which promotes the *Scopus* database. Clever marketing strategies, including awards for 'citation laureates' in chosen countries have increased the number of scientists obsessed and addicted to citation counting. It is common to see Ph D students and postdoctoral fellows choosing journals to send their papers, based on impact factors. Gone are the days when one 'just knew' the most desirable journals in a field. Since the highest ranked journals are often unattainable, impact factors are used to make choices between what are considered 'mid-level' journals. In many ways this is an undesirable practice; personal judgements may at least permit students to come to their own conclusions.

The field of measuring scientific impact continues to grow. A formidably titled analysis, 'A principal component analysis of 39 scientific impact measures' begins engagingly: 'Science is a gift based economy; value is defined as the degree to which one's ideas have contributed to knowledge and impacted the thinking of others'. The conclusion of this study should be music to those who worry about the impact of the impact factor: 'The commonly used citation Impact Factor is not positioned at the core of this construct, but at its periphery, and should thus be used with caution' (Bollen, J. *et al.*, arXiv: 0902.2183vl, 12 February 2009). The age of the Internet has also led to the use of the number of 'downloads' to rank articles (and their authors), although it seems apparent that downloading, reading, digesting and citing are distinct activities that may reflect the influence of a paper on a reader. Despite many warnings over the years, scientists and managers of science in India increasingly use measures like impact factors and *h*-indices to make comparisons of individuals, journals and institutions with little regard for differences between disciplines. A recent analysis examines differences across fields and draws attention to 'inflation' in impact factors over time (Althouse, B. M. *et al.*, *J. Am. Soc. Inf. Sci. Tech.*, 2009, **60**, 27). The average impact factors for journals in defined fields vary widely. The numbers for some representative fields are: Molecular and Cell Biology 4.76, Chemistry 2.61, Physics 1.91, Chemical Engineering 1.29, Fluid Mechanics 0.80, Materials Engineering 0.82, Mathematics 0.55). Clearly, cross-discipline comparisons must be avoided and inter-institutional comparisons may turn out to be flawed.

Evaluating science is probably best done, even today, by reading and listening. Authors must write and readers must read, without self-consciously worrying about the status of journals, to the point where it becomes a distracting obsession. Administrators cannot abdicate a responsibility for judgement, when selections and promotions are involved. If the stranglehold of the *h*-index and the impact factor is loosened science might again be fun.

P. Balaram

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EDITORIAL

Paleontology: A Minefield of Controversies

Publishing scientific journals can sometimes be a hazardous exercise. A routine academic activity can be transformed into a contentious arena, when a controversy erupts over a published paper. A few months ago, this journal featured on its cover a fossil, that may have appeared fearsome to the average reader; the remains of the head of a long extinct dinosaur. I am no paleontologist, but did realize, in the final stages leading to production, that dinosaur fossils do make striking cover illustrations. In these intensely competitive times even journals of science must do all they can to attract a readership. A long standing fascination for dinosaurs, fuelled in more recent times by *Jurassic Park*, aided a quick choice of a cover figure. The paper entitled 'Skull of *Minotaurasaurus ramachandrani*, a new Cretaceous ankylosaur from the Gobi Desert' appeared especially suited for an audience of Indian readers; the dinosaur's name had a curiously South Indian ring. The description of the skull seemed detailed and comprehensive, with computer tomography providing a unique anatomical view. A cursory glance established that the skull had been purchased by the highly visible neuroscientist V. S. Ramachandran and displayed in a California museum (Miles, C. A. and Miles, C. J., *Curr. Sci.*, 2009, **96**, 65–70). At a fortnightly journal, run by academics on extra time, there is little scope for editors to recall the history of manuscripts before acceptance; publication effectively erases editorial memory. A few weeks after appearance of the paper on the Mongolian dinosaur, I received an e-mail from a paleontologist of considerable repute, who pointed out that 'there is turmoil in global paleontological circles because such specimens bought in clandestine sales will give a boost to vandalism and the commercialization of science and give scientific sanctity to specimens that have no scientific value'. This expression of concern was catalysed by a communication from a fellow paleontologist, who was 'disturbed to note that the specimen has no provenance (but thought to be from Mongolia) and was purchased at a Gem & Mineral show. There is no paper trail for where this fossil comes from or how it was obtained'. This correspondent also drew attention to a *Naturenews* report which invited readers with a characteristically provocative title, 'Paper sparks fossil fury' (Rex Dalton, 2 February 2009; doi:10.1038/news.2009.60). This news item somewhat condescendingly referred to 'an article in an Indian journal' and carried the comment of a vertebrate paleontologist who remarked that 'it is totally inappropriate to publish on this specimen; it is stolen patrimony'. Online news sites have the ability to elicit instant comments, with *Nature* exhorting readers to be 'as critical or controversial as you like, but please don't get personal or offensive'. The comments on the Mongolian fossil seemed mixed, with some readers voicing a view that seemed to support publication: 'More importantly, the origins of a fossil must not become an impediment to what we can learn'.

The criticism of the decision to publish the paper on the ankylosaur fossil prompted me to reexamine the file on the manuscript. I then noted that the paper was received on July 20, 2007. Three referees read and wrote detailed reports; all commenting on the absence of the geographical location where the fossil was found. One referee described the fossil as 'a beautiful skull of a new ankylosaur', but did express concern about the 'ownership of the specimen from Mongolia'. The reviewer also added that the specimen had 'great scientific value' and seemed reluctant to be a part of a 'fossil feud'. At this point editorial discretion might have tilted in favour of rejection to avoid a controversy. However, my ignorance of the byzantine world of paleontology pointed me along a more author-friendly route. In February 2008, a revision and responses to the reviewers comments was asked for. A revised version was received in October 2008. In addition to several changes, the authors admitted in a note to the editor: 'The skull originated in the Gobi desert, whether from the Mongolian or Chinese side, we do not know. This is consistent with the information provided by the original owner in Japan. Given the nature of the surrounding matrix, which is similar to other Gobi desert fossils that have been described, we are confident of a Gobi desert locality'. Should the paper have been published, if the origin of the fossil was not clearly established? Should a skull of a dinosaur, specially prepared for display in an American museum remain unstudied, even if useful information can be obtained? Does publication of such a paper encourage smuggling of fossils or are such activities promoted anyway by collectors and even museums? Should the skull be returned to the country of origin, if indeed that can be established? Despite the public

debate on these issues within the paleontology community, no clear consensus seems to emerge. Ironically, rare manuscripts, records and historical artifacts transported away from the country of origin have long been displayed and studied in the museums and libraries of Europe, Britain and the United States. There has been little talk of returning these records and artifacts to the 'countries of origin'. The sensitivity of the paleontology community seems to be more a result of the fact that provenance of a fossil cannot be established in such cases, resulting in loss of valuable locational information. Despite the absence of such data, much can still be learned from fossils on display. To argue that publication will encourage 'smuggling and vandalism' seems an overstatement, based on an unrealistic assessment of the value and importance of a paper in a scientific journal. Fossil smuggling is driven by the prices paid by collectors, including museums.

Paleontology is a field that fascinates the lay public whenever fossil remains of ancient animals are reported, especially if there is a link to the human lineage. The most recent example of extraordinary public interest in a fossil find is the coverage in the popular press of a paper entitled 'Complete primate skeleton from the middle Eocene of Messel in Germany: Morphology and paleobiology' (Franzen, J. L. *et al.*, *PLoS One*, 2009, **4**, e5723). The fossil estimated to be 47 million years old is of an early primate, a precursor in the evolutionary branch from which apes and humans emerged. The fossil found in Messel, Germany has a 'curious history'. The authors provide a complete description of the fossil's remarkable journey to stardom: 'The fossil was apparently unearthed in 1983 by private collectors who split and eventually sold two parts of the skeleton on separate plates; the lesser plate (herein plate B) was restored and in the process fabricated to make it look more complete. This was eventually purchased for a private museum in Wyoming and then described by one of us who recognized the fabrication. The more complete part (plate A) has just come to light, and it now belongs to the Natural History Museum of the University of Oslo (Norway). When made available for study, plate A was immediately recognizable as the complete complementary and unaltered counterpart of plate B' (Franzen, J. L. *et al.*, *PLoS One*, 2009, **4**, e5723). The explosion of interest in the fossil, now described as *Darwinius masillae* has been fuelled by a well-orchestrated publicity campaign that began in advance of publication in the journal. The critics seem to disapprove of excessive publicity; curiously the provenance of the fossil and its fragmentation and reunion across continents have attracted little attention. Private unnamed collectors and buyers and museums, seem an integral part in the life of paleontologists and the published literature is not always clear whether 'permits' for transport of fossils across national boundaries are always obtained. Somewhat battered by the criticism of the Mongolian dinosaur paper, I could not help wondering if the ethical concerns of paleontologists are selectively voiced.

Paleontology is a field that is not new to controversy. There is romance and adventure in fossil hunting. The debate over the fossils of dwarf humans, *Homo floresiensis*, found in the island of Flores, Indonesia in 2004, has received a shot in the arm, with new studies supporting the idea of a new species (Jungers, W. L. *et al.*, *Nature*, 2009, **459**, 81; Weston, E. M. and Lister, A. M., *ibid*, 2009, **459**, 85). New hypotheses on the evolution of *H. floresiensis* are already appearing. Innocent, as I am, of the ways of paleontology, paleobiology and anthropology, I could not but be enthused by the exhortation at the conclusion of a commentary on the new papers: 'But the only way to test these and other hypotheses is to find more fossils, especially in Asia. Get out your shovels!' (Lieberman, D. E., *Nature*, 2009, **459**, 41). Paleontology's many controversies bring back memories of 'The Great Piltdown Hoax', an episode in which the famous Piltdown skull, pieced together between 1908 and 1911 was shown to be a hoax in 1953, using improved methods of chemical analysis. The skull was once thought to belong to the 'dawn man' or the 'earliest Englishman' (Straus, W. L. Jr, *Science*, 1954, **119**, 265). A recent report using the most recent analytical methodologies provides a new twist to another of paleontology's many controversies regarding the evolution of eukaryotes based on fossil evidence. In a paper entitled 'The controversial "Cambrian" fossils of the Vindhyan are real but more than a billion years older', a team of Swedish and Australian scientists support the claim of R. J. Azmi made a decade ago for fossils of eukaryotic organisms in the Vindhyan basin. Controversies over the age of the geologic formations led to doubts regarding these claims, with an emerging consensus that the fossils did not exist or were artifacts. The new study concludes that 'the discredited report of "Cambrian" fossils turned out to be an important discovery'. This vindication of an Indian study published years earlier does not seem to have laid the old controversy to rest. Instead, a debate on Azmi's treatment by the community of geologists and paleobiologists appears to have been sparked (*Naturenews*, 22 April 2009; doi:10.1038/news.2009.383).

Paleontology is a fertile field for controversies. While procedures for dating and analysis improve continually, fieldwork, fossil identification and integration with geochronology and anthropology require special talents and a flair for multidisciplinary work. Hypotheses sometimes run far ahead of the evidence. The problems of 'ethical' issues in papers dealing with rare and interesting fossils adds a new dimension to an area that is a minefield of controversies.

P. Balaram

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EDITORIAL

Economics and Economists

Science writers are a rare species. Some years ago, in an effort to promote the profession of science writing in India, the Ramaseshan Fellowships were introduced at this journal. The scholarships, named after the Editor who transformed this journal in the late 1980s, were designed to allow prospective writers to intern at the journal. There have been very few promising applicants for the fellowships, since most students graduating with Bachelor's and Master's degrees in science do not view writing as a possible career choice. Writing skills appear scarce amongst science students in search of employment. This year's selections provided me an opportunity to talk to some of the applicants, revealing interesting insights on the perceptions that exist about science and science writers. Almost all applicants denied any knowledge of well-known popularizers of science. One candidate for the fellowship seemed interested in science policy and during the discussion the conversation drifted to the relationship between policy and economics. Since he was from the Jawaharlal Nehru University, I asked him whether he knew any economists who wrote columns in magazines. He was quick with his answer, naming Jayati Ghosh and C. P. Chandrasekhar, who regularly inform readers of *Frontline* on economics and public policy. Both of us seemed to veer around to the same conclusion that economists (at least in India) are better writers than scientists. Could it be that economics as a discipline promotes better communication skills?

In thinking about disciplines and courses I turned inevitably to the problem of career choices by students. June is an important month for students who seek to enter college, embarking on the quest for a university degree that may determine the course of their careers. For the very best among the hundreds of thousands of young men and women jostling for college admissions the choices are clear. High ranks in the various entrance examinations ensure a place in the most sought-after courses and institutions. The overwhelming majority of students drift into courses for which they have limited interest, driven by pressures that build from an environment dominated by peer groups and families. In an uncertain economic climate, employability after a course is a dominant concern. Engineering courses are an overwhelming favourite in India, because of the popular perception, often correct, that employers find engineering graduates 'employable', even in areas where their special training serves little purpose. The pressure for admission to the best of engineering institutions ensures that the students who are selected come through the rigours of an extremely difficult 'entrance examination' procedure. The very high thresholds for entry to these institutions ensures that the average graduate amply fulfil the expectations of employers. June is also the month when colleges report the precipitous fall in interest of students in science courses, setting off the inevitable discussion on how to attract students to careers in science. There are two major schools of thought on this issue, espousing opinions that carry little conviction. One school argues incessantly for making science careers financially more attractive. In this view, money will solve most problems. The alternative view, often hesitantly expressed, is that science in India has few 'role models' who serve to inspire young students to follow their path. I suspect role models are hard to find in most professions; the most visible symbols of success are also extremely difficult to emulate. In thinking about the diminishing interest in undergraduate science courses I could not help wondering about other subjects, far removed from engineering. History, philosophy and literature seemed so distant from science that it appeared inappropriate to consider these disciplines in this column. I was then left with no option but to return to a subject that I mentioned earlier and one that seems, at times, so close to science—economics.

Is economics a science? If the large-scale entry of physicists and engineers into the area of high finance is an indicator, it would appear that quantitative, mathematical approaches will permit modelling of the behaviour of financial markets. But economics is a much wider subject than present day concerns with stock markets would indicate. There was a time when the distinction between economic theories and dogmas seemed blurred. The triumph of the free market (at least at present) seems to have put an end to the debates about the relative virtues of capitalism and strict government control of economies. The current financial crisis has introduced uninitiated observers to unfamiliar terms like recession and deflation, reviving interest (and doubts) in the predictive power of economic theories. Economists appear to be the most influential drivers of public policy. Economics would appear to be an extremely attractive discipline as a career choice for a young student since the field appears poised for a revolution. In wondering if there are role

models in economics research who might inspire a new generation, I came across an interesting compendium entitled *Lives of the Laureates: Twenty-three Nobel Economists* (Breit, W. and Hirsch, B. T. (eds), MIT Press, 2009). In science, the Nobel prizes confer on the recipients an unmatched aura of distinction; the prestige of the prize having grown over a century. The prize for 'Economic Sciences' is a late addition; instituted in 1968 by the Bank of Sweden in 'memory of Alfred Nobel'. The award has over the years acquired a degree of acceptance amongst academic economists and attracts much less controversy than the prizes for literature and peace. Subjects like game theory which have entered the discourse of computer science and biology are recognized as critically important in economics. The collection of autobiographical essays edited by Breit and Hirsch provides a fascinating insight into the careers of nearly two dozen laureates in economics. The compendium originates in lectures given at Trinity University, San Antonio, Texas, which were intended to use the medium of an autobiographical memoir to trace the evolution of post-World War II economics and economists. I must confess to a weakness for reading autobiographical essays, which appear as prefatory chapters in the volumes of *Annual Reviews*. Such personalized accounts of successful careers in academic life are often inspirational, encouraging and surprisingly informative. I have found such essays easy reading, even when the disciplines have been very distant from my own. The accounts of the economists are uniformly readable, despite the fact that the laureates happen to be largely drawn from a limited academic environment, mostly from the United States. This collection does not include essays by Amartya Sen or the late Gunnar Myrdal, who was at one time strongly critical of the institution of a Nobel award in economics. Others whose recollections might have been interesting are Herbert Simon, a polymath who could hardly be labelled as a conventional economist and Friedrich Hayek who has been described 'as a distinguished political philosopher whose views on economic methods were antediluvian' (Brittain, S., *The Financial Times*, 19 December 2003).

An interesting section of this book is an 'afterword' by the editors which asks the question 'Do these autobiographical essays reveal anything of significance? Does biography matter?' In many ways this collection, based on lectures that reflect on a career, constitutes a form of oral history; subjective and intensely personal. Autobiographies can sometimes be self serving, but the accounts in this collection are largely straightforward and appealingly candid. Students and young academics will find accounts of difficulties in making career choices, switching subjects and early career difficulties reassuring to read. The more seasoned readers will be comforted to find accounts of academic politics that appear familiar. Even more important are the factors which influenced the choice of areas for research. The editors take the example of Milton Friedman, once famed as a champion of free markets when controlled economies were still fashionable, and note that his choice of economics over mathematics was motivated by a feature of the Great Depression; 'the paradox of great need on the one hand and unused resources on the other'. Curiously, this paradox may be evident in India even today. There appears to be great merit in choosing 'less travelled roads' in research that is ultimately considered of the highest impact. But such choices are difficult to make. Reading these essays one cannot help reflecting on the role of the environment, teachers and colleagues in shaping academic careers. Friedman quotes, and the editors repeat, Robert Frost's famous lines:

'Two roads diverged in a yellow wood
And sorry I could not travel both
I took the one less travelled by,
And that has made all the difference.'

Friedman, tongue in cheek (presumably), lists the requirements for winning the Nobel award in economics. To be male, a US citizen and to be at the University of Chicago are attributes he lists; a reflection of the choices made over the years. To understand American dominance one can turn to a section in Paul Samuelson's essay, subtitled 'Pax Americanus'. He notes candidly that 'the sheer number of American economists gave them unfair advantage over economists abroad. In addition, Hitler gave us even before the war the cream of the continental crop. Just as Chicago is the biggest Polish city after Warsaw, and New York the greatest Swedish city after Stockholm, America obtained practically the whole of the Austrian school in economics. Along with such names as Einstein, von Neumann and Fermi go such American economist names as Koopmans, Leontief, Schumpeter, Marschak, Haberler and many others. Later, as strength draws to itself strength, there begins to appear in the American lineup the names of Hurwicz, Debreu, Theil, Bhagwati, Coase, Fischer and many others'. Samuelson reflects: 'Evidently science itself is not subject to the law of constant returns to scale. The book of Mathew applies: To him who hath shall be given. Unfair? you say. Well, as Jack Kennedy observed: Who said life is fair?'

This compendium of personal accounts of a life in economics and academia has evolved through five editions over two decades. The case for writing and reading such essays is strong. Such collections in other fields may serve to attract and inspire a new generation. Reading these essays it is clear that success is, as the editors note, 'sometimes the result of happenstance and sometimes not'. Nevertheless, reading tales of intellectual journeys made through widely differing circumstances is enlightening. Economics has sometimes been labelled as 'a dismal science'. The 'Lives of Laureates' does much to dispel this notion. It also reinforced my prejudice, acquired in the search for budding science writers, that one is more likely to find good writers amongst economists than amongst scientists.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Teaching and Research: Inventing a Connection

Education appears to have been catapulted to centre stage in recent times with a great deal of discussion and debate on both school and higher education. On the one hand there is the suggestion that the 10th standard board examination be abolished or made optional in schools; on the other is the recommendation that the system of higher education be completely restructured, by the introduction of a new, overarching body that incorporates all the diverse commissions and councils, which are meant to police the vast network of colleges and universities, both public and private. Clearly, the sudden ferment, in the arena of education, is the result of a long felt need for change, catalysed by the pressures of privatization, globalization and expansion of higher education. Over the years there have been many committees, commissions and reports; most recommendations have been welcomed and then ignored. Even with the best of intentions, implementation of suggestions which may significantly perturb the existing system appears to be insurmountably difficult, with resistance to change being the determining factor. While a major expansion has been set in motion, with announcements of many new institutions to be created across the country, the need to rejuvenate and transform many old institutions is clearly recognized. In the many fields of science falling enrolment in B Sc courses, in many states, has been the subject of considerable discussion. There have been many seminars devoted to considering strategies for attracting young students to science. New scholarship schemes have been announced in the hope that money will entice students and parents away from professional courses in engineering or medicine. The situation in humanities and social sciences may not be much better, although public expressions of concern are muted. The debates about the content and quality of college education in sciences have thrown up many suggestions that merit serious consideration; most notably the introduction of a four-year Bachelor's degree program, with considerable flexibility in courses and a component of research. The 10 + 2 + 4 system would align with many programs the world over, reduce the time for entry of students into research programs and broaden the exposure of students to multiple disciplines, at the level of their college degree.

In all the discussions on higher education in science, one of the key issues has been the need to increase both the quantity and quality of research in science, emanating from Indian institutions. Research output is determined to a considerable extent by the number of graduate (Ph D) students and postdoctoral fellows in an institution and their quality. These in turn are determined by the size of the faculty, their quality and the level of infrastructural and research support. At the level of a research degree there are many areas of current interest where science, engineering and medicine merge seamlessly into one another. The scientific revolutions of the past fifty years in materials science, biology and computational methodologies have had a dramatic impact, emphasizing the need for interdisciplinary approaches. Unfortunately, the fragmentation of higher education in India is a reality, with the creation of separate universities for engineering and medicine in many states. The traditional university often has only a minimal core of humanities and sciences. In a majority of cases, undergraduate programs are localized in constituent colleges, with university science departments focused only on Master's degrees. The extent of research in university departments varies widely, with relatively few centres recognized as vigorous promoters of research. The barriers between the various disciplines of science are generally impregnable, leaving students with very little opportunity to acquire a taste for subjects which may help them enter areas of interdisciplinary research. Fragmentation begins early, with subjects like biology and computer courses becoming mutually exclusive even at the level of classes 11 and 12. Specialization is favoured in most programs, with the result that students are ill prepared to enter many areas of research, which involve the need to cross disciplinary boundaries. Overloading students with excessive syllabi at high school level is hardly the prescription for attracting students to science. Colleges have also struggled to maintain science courses as enrolments fall and the costs of running laboratories rise. For students who survive the course up to a Master's degree, financial support to continue on to a Ph D is available through national examinations. It is not uncommon to find research students in university departments struggling on without financial support. Can

meaningful research be accomplished when the level of preparation and support is so low?

The higher education sector in India is also dominated by the rise of the 'deemed university'. Many specialized, centrally supported research institutions which often have only one or two disciplines have been declared to be 'deemed universities'. This, of course, allows them to grant Ph D degrees to researchers. Many of these laboratories are too small in size and extremely limited in their scope, making the label of a 'university' meaningless. Their small size, high level of per capita funding and superior infrastructure make them attractive places for students seeking research degrees. However, there is no formal teaching in most of these national institutions, with students entering directly into a research laboratory. This situation may be contrasted with any American 'graduate school', to which many of our science students are admitted, where a mandatory course program must be completed and a qualifying exam hurdle must be crossed before entry into research. It is the course program that broadens a student's knowledge and outlook and allays fears about subjects which may be needed later. Very few institutions in India insist upon courses for Ph D scholars, thereby missing an opportunity to improve the level of preparation for research. Courses also introduce students to a wider cross-section of faculty; a bonus that can often prove to be useful. Why is there so little teaching in our research institutions? Is it because senior researchers believe that teaching is a distraction that takes away valuable time from research? Is it because that teaching requires a certain discipline that is constraining? Is it because teaching at the highest level requires a level of preparation and commitment that researchers find uncomfortable?

The segregation of teaching and research may be one of the most limiting and undesirable features of the Indian higher education scene. George Bernard Shaw once wrote, in characteristically dismissive fashion: 'He who can, does; and he who cannot, teaches'. However, Shaw was referring to creative writing and not to the practice of science. In science, teaching often benefits the teacher; sometimes even more than it does the student. For students an enthusiastic and knowledgeable teacher can be inspiring, catalysing their interest in subjects that may not be directly related to a narrow research problem. Excessive focus in a specific area during a Ph D program, can be limiting in a future research career. Good teachers can also train a new generation of teachers. An editorial in *Nature Chemical Biology* (Dec. 2007, p. 737) summarized the case for teaching succinctly: 'Those who can teach, should'. India now finds itself in a curious situation. There are a large number of national laboratories, well funded and well staffed, where there is research but no teaching. At the same time we have a large number of colleges and universities where there is considerable teaching, with little research. The IITs and the newly created Indian Institutes of Science Education and Research (IISERs) are examples of attempts to promote undergraduate and postgraduate programs in an environment where research is encouraged. The IITs have half a century of experience, while the IISERs are in their infancy.

Is there an academic case for promoting teaching in research institutions and to embed undergraduate programs in a research environment? I believe there is and that the present concerns on the state of science education are best addressed if scientists begin to fulfil their responsibilities as teachers. The imperatives of interdisciplinary research also demand a fresh approach to undergraduate education. Broad based programs in which the barriers between science and engineering are breached may allow us to nurture a new generation of students, who are comfortable with multiple disciplines. Exposure to research projects, conducted as a group effort, may provide the incentive to explore a future career in research. In many ways, research can be addictive and an early introduction to research may be a device that makes science more attractive. A flexible system of courses that includes exposure to social sciences and modules that enhance communication skills may be desirable. Implementing a transformation of undergraduate science education will be a formidable task. There is however a clear need to experiment and refine our approaches with experience. Maintaining the status quo will be comfortable, but it is an option that must be discarded if science in India is to grow and the requirements of research institutions and agencies are to be met. Despite many well meaning reports by committees and commissions, the burden of change rests with institutions and their faculties. The responsibility for engineering a transformation in the arena of higher education must fall not only on universities and governments, but also on research institutions. The time seems opportune to invent a robust connection between teaching and research.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Genes, Disease and Translational Research

'Translational research' is a term that is used in an almost entirely biomedical context; it describes the process by which the fruits of biological research enter the realm of clinical practice. An editorial in *Science* (2009, **324**, 855), authored by as many as thirteen scientists, administrators and editors entitled 'Translational careers', emphasizes the need to strengthen the support systems for promoting 'multidirectional translation'. In the United States a major investment has been made through the creation of a consortium of 39 translational centres 'with an annual funding component of $500 million by 2012'. This unprecedented effort, in recessionary times, is testimony to the belief that 'powered by the computational muscle of bioinformatics and the broad perspective of systems biology, advances in biomedical science now have the capacity to transform medicine'. The editorial notes that for effective realization of this hope it is necessary to have both 'systems and people', who will 'ensure a vibrant flow of information between the basic sciences and clinical medicine'. The steady advance of medicine through the 20th century has been fuelled by pathbreaking research in biochemistry, microbiology, physiology, chemistry and physics. Many discoveries have been serendipitous, reminding us of Pasteur's famous dictum that 'chance favours the prepared mind'. Drugs, diagnostics and vaccines have all been the end products of research that has been pursued with considerable patience and perseverance. In the last two decades the progress of biology has hastened dramatically; the genome sequencing revolution has raised public expectation of new cures for old diseases to a level that is, at times, alarming. When biology's advancing front meets with the ever expanding power of computers, the resultant mix evolves into the heady world of systems biology. Can all of modern biology's potential be 'translated' into successes in the clinic?

Sixty years ago Linus Pauling and his colleagues published a short paper in *Science* establishing that the haemoglobins from normal individuals and sickle cell anemia patients were different molecular species. A few years later, in the mid 1950s, Vernon Ingram established the difference as arising from a mutation of a single amino acid residue in a protein consisting of several hundred residues. Pauling was to comment that 'on such atomies man's fate depends'. Since sickle cell disease was inherited, Pauling correctly concluded that 'genes precisely determine the structures of proteins'. For students of biochemistry and genetics the sickle cell story is encountered early in their careers; a simple illustration of Mendelian inheritance and a dramatic example of the fragile thread that links biomolecular structure to physiological function. The thalassaemias, another well studied example of disorders of blood, present a related situation where gene defects translate into faulty protein production, with consequent clinical manifestations. Once again, the relationship between genes and disease is clear. The haemoglobin diseases are classic cases where the basic scientific issues are clearly understood. Have the insights gleaned over decades of painstaking research led to cures? The answer is sobering. While much has been learned about these diseases and clinical management, there are no simple, widely accessible 'cures' that have been discovered as yet. In the 1990s it seemed that 'gene therapy' would indeed emerge as the approach of choice for the treatment of single gene disorders. The apparent conceptual simplicity of this approach masked the complexities inherent in gene transfer from vectors to hosts and the interactions between hosts and vectors. The number of clinical trials in the United States grew dramatically by 2000, but the unfortunate development of 'treatment associated cancers' and the tragic death of a patient in a trial in 1999 marked the fall of gene therapy from favour, in the arena of translational research.

The current favourite in biomedical research is the field of stem cells. In the United States, legal restrictions on funding of human embryonic stem cell research have hampered basic progress. The Obama administration lifted these restrictions a couple of months ago, but the American President was quick to add a cautionary note: 'At this moment, the full promise of stem cell research remains unknown and it should not be overstated'. He added that, 'I cannot guarantee that we will find the treatments and cures we seek'. Indeed, stem cell enthusiasts promise cures for almost every neurodegenerative disease, diabetes, cancer and cardiac ailments and speak optimistically of regenerative therapies. Will stem cells

be medicine's new frontier? Are public expectations based on a solid foundation of scientific research or is the field being driven by marketing strategies and hyperbole? James Wilson, who led the gene therapy trial that led to the death of a patient in 1999, presents a sobering assessment in an essay entitled 'A history lesson for stem cells' (*Science*, 2009, **324**, 727). He applauds the new American initiatives that have given 'scientists longer leashes as they explore this exciting field'. However he adds, somewhat sombrely, that 'in today's clamor of stem cell enthusiasm it is possible to detect haunting echoes of the early and ultimately troubled days of gene therapy'. Wilson describes the growth of the field in the early 1990s as 'hyper-accelerated translation to the clinic' driven among other factors by 'unbridled enthusiasm of some scientists in the field, fuelled by uncritical media coverage; and by commercial development by the biotechnology industry during an era in which liquidity could be achieved almost entirely on promise, irrespective of actual results'. Wilson notes that an NIH panel addressed growing concerns in a report in 1995, recommending a greater emphasis on a basic understanding of gene transfer mechanisms. The proponents disregarded the cautionary notes, leading to the debacle of the late 1990s. The tragic turn in clinical trials coincided with the 'bursting of the overall biotech bubble' leading to a general loss of confidence in gene therapies. The field in Wilson's words has now 'adopted a more sober approach to clinical trials and bolstered its commitment to basic vector biology and disease pathogenesis'. In considering the new frontier in translational research, stem cell therapies, Wilson notes that 'despite advances, our understanding of the biology of human embryonic stem cells and induced pluripotent stem cells remains thin with regard to clinical safety and utility'. Wilson's thoughtful perspective, based on harsh and tempering experience, must be required reading for policy makers and regulators in India, where stem cell research and translation may move faster than the gene therapy area. There is a role for scientific leadership which Wilson argues, 'must steadfastly discourage overselling the clinical reality of stem cell therapeutics and must effectively communicate how long it takes to go from laboratory bench to bedside'.

The promise of modern biology has been overwhelming. 'Biotechnology', a term that encompasses all the current areas of molecular and cell biology, has immense potential for 'translation' in fields as diverse as agriculture and medicine. Nowhere has the unity of biology been more manifest than in the genome; a unity that is most evident in the reductionist view of molecules and cells. How patient must one be in waiting for laboratory successes to transform the clinic? The case of cystic fibrosis is worth recalling. It is estimated that 'approximately one in twenty Caucasians carries a defective cystic fibrosis gene and about one child in 2000 inherits such a gene from both parents and consequently dies of the disease, usually by the age of 30' (Newmark, P., *Nature*, 1985, **318**, 309). The hunt for the gene began in the mid 1980s, resulting in the cloning and sequencing of the gene coding for a protein named the cystic fibrosis transmembrane regulator (CFTR). Disease causing mutations lead to chemical differences and defective function of the CFTR protein. In marking 20 years of the discovery of the cystic fibrosis gene, a *News Focus* article in *Science* (Couzin-Frankel, J., 2009, **324**, 1504) revisits the field. Two decades after the successful conclusion of the gene hunt, there is still no cure for cystic fibrosis. The early promise of gene therapy has not been realized. The author notes: '. . . as new gene discoveries pile up weekly and hype over the power of genes to transform medicine flows fast, cystic fibrosis offers an object lesson in how difficult it is, and how long it takes, to convert genetic knowledge into treatments'. Several researchers quoted in the *Science* article seem to agree that 'being humble in science' is a lesson that has been taught by the cystic fibrosis saga. The technologies of cloning and sequencing have propelled gene hunting to the frontline of biomedical research. Extravagant claims for the discovery of genes responsible for depression, schizophrenia and compulsive criminality appear regularly in the press. Despite the experiences of the past there is still a fond hope, widely held, that identification of disease genes will quickly translate into cures. The linkage between a variant of a serotonin transporter gene and depression has been laid to rest, prompting an article in *Science* entitled 'Back to the drawing board for psychiatric genetics' (2009, **324**, 1628).

Translational research is a term that is now commonly heard in India. Several new centres are on the anvil, which will face the formidable challenge of bridging the gulf between basic research in biology and the real life problems of clinical medicine. The absence of a large number of clinical centres which emphasize medical research and the separation between research laboratories and hospitals will prove to be a major impediment. Traditionally, research and education in medicine have been supported by the Indian Council of Medical Research and the Ministry of Health, while a multiplicity of agencies, the Department of Biotechnology foremost amongst them, fund biological research and efforts in related disciplines. Any purposeful attempt at achieving translation will require an unprecedented level of inter-departmental and ministerial cooperation, together with efforts to widen the base of collaboration between basic scientists and clinicians. This may happen only when basic researchers perceive the potential insights that will undoubtedly result from studies of clinical problems, and clinicians in turn, recognize the enormous power of modern biological techniques, that may sometimes prove invaluable in the clinic.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Research Universities: Learning from Experience

For much of my career, spent in the confines of a research laboratory, the problems that confronted me and many of my colleagues seemed amenable to solution, even if they eventually turned out to be intractable. Unsuccessful lines of research often yielded byways into which one could wander, many times with little accomplishment, but the experience seemed to be always useful in the investigation of apparently unrelated problems. The dimensions of a research problem are invariably determined by the investigators; pragmatic researchers work towards attainable goals, practising the 'art of the solvable'. In contrast, dreamers and those who are endlessly ambitious set the bar very high. Some remain optimistic, cheerful and committed to their goals, while the endless quest leaves others embittered and disappointed by failure. The realm of governance of academic institutions and the arena of research and educational policy at institutional and national levels seem far more complex, with many recent discussions leaving observers like me both puzzled and uneasy. Problems are plentiful, solutions offered by a wide spectrum of interested individuals are abundant and progress is painfully slow. Good ideas can usually be implemented with relative ease in laboratories, but make little headway in the sphere of public policy. The cacophony of debate and discussion inhibits even incremental changes of approach to long standing issues. The public discussion of higher education and the state of universities in India has now reached a crescendo. There appears to be a consensus on only one issue; the need to enhance the academic quality of our old institutions and to create new ones, to expand the opportunities for college and university education on a broad front. The calls for creating 'centers of excellence' and 'world class universities' are mildly unsettling; if the protocols and procedures (terms that I borrow from the laboratory) for their creation are self-evident, why have they not been implemented so far? Can excellence and world class status be brought into being by legislation? In a slightly more cautiously worded strategy, institutions are identified as having 'potential for excellence'; the expectation here is that the catalyst for transformation, into truly high quality academic centres, will be the infusion of central or state funding. Even as we lurch inexorably into a phase of major expansion of the system of higher education and research in India, an examination of the results of experiments conducted around the world may be of more than academic interest.

Coincidentally, as the debate on higher education and state of research in India heats up, I suddenly received a clutch of books and reports, all of which seemed to have a bearing on the subject. A slim volume entitled *The American Research University from World War II to World Wide Web* by Charles Vest (University of California Press, Berkeley and Los Angeles, 2007) is a wonderfully readable account of the American university system, arguably the world's largest and most successful experiment in higher education. The author, a former President of the Massachusetts Institute of Technology (MIT) and a mechanical engineer of great distinction, presents a fascinating view of the evolution of the American research university, on which many institutions in the developing world have been modelled. The book is based on the Clark Kerr Lectures delivered at the University of California, in 2005. As Vest notes: 'Kerr was a doer and builder, not just an observer and theoretician. Kerr's shadow looms large over the American educational landscape. His understanding of the emergence of the *multiversity*—as he famously termed it in his 1963 Godkin Lectures at Harvard—crystallized our view of the tectonic changes that occurred in US research universities at the middle of the twentieth century'. There are two words in the quote reproduced from Vest which deserve our attention. The first is the term 'multiversity'. I will not define it but merely borrow more of Vest's text, where he describes his education at the University of Michigan in the 1960s: 'The corner of the emerging multiversity was very attractive and exciting. What a joy it was to pursue my engineering education in this heady environment and also to have friends who were students of medicine, law, history, chemistry, mathematics, social work, education and philosophy'. Vest emphasizes that he 'learned and worked at the new boundaries of academic engineering, and yet still felt very much a part of the great centuries-old tradition and values of academia'. Even as the debate on the Indian university intensifies we might do well to think about the 'multiversity', which Vest describes as a 'noble and enabling place'. He is clear in his assessment decades later: 'What appeared to many to be sources of tension, cross-purposes, and potential conflicts of values and interests were for me a great web or mosaic to be

savored and celebrated. This was what I expected a university to be. And, despite the passage of over forty years, it still is'.

A second word that seemed particularly apt in Vest's description of the changes in the American university system in the mid-20th century was the term 'tectonic'. Even as we await tectonic changes in our own university system, half a century later, we might ask what was Vest referring to. He was describing 'what has been termed the engineering sciences revolution', which flowered after World War II, with MIT's Radiation Laboratory producing a generation of scientists and engineers who developed 'the new engineering science which relied on intense research and required an entirely new panoply of textbooks and laboratories, drove change in a broad range of fields, among them the space program, defence, transportation, telecommunications, computing and medicine'. Vest notes proudly, justifiably so, that in 'study after study, ranking after ranking, and through that greatest of all compliments—emulation—we really are the proverbial "envy of the world" '. He narrates an interesting story in which the rector of the Humboldt University at Berlin asks Vest 'in all seriousness' as to 'how the US research university model could be successfully transplanted to Germany'. The delicious irony, as Vest points out, 'is that in the nineteenth century the United States, and specifically the Johns Hopkins University, imported the concept of the research university from Humboldt!' Curiously, in the late 1890s Johns Hopkins was a model on which Indian Institute of Science, Bangalore, was conceived as a 'university of research'.

In his compellingly readable narrative, Vest surveys on a very broad range of topics, many of which are most relevant to the current concerns in Indian higher education and research. He notes that 'other nations could profitably consider' the factors responsible for the successes of the US system and 'integrate them into their own cultural and political context, and perhaps improve upon them'. I suspect it is our understanding of our own 'cultural and political contexts' and our ability to adapt and modify successful models elsewhere, that may eventually stimulate the tectonic upheaval that is needed in our system of higher education. Diversity of funding sources, improved governance structures, faculty responsibility, maximizing diversity amongst faculty and students and the promotion of interdisciplinary activity in both teaching and research are some issues that merit attention. Vest remarks that 'intelligence, curiosity and creativity have no national boundaries'. In sentences that must please all of us in India, Vest adds: 'Great universities based on this residential, research-intensive model can and do arise anywhere in the world. As demonstrated by the enormous success and impact of the Indian Institutes of Technology, which were established in the 1960s, higher education can leap forward very rapidly'. We must, however, view this praise dispassionately and ask whether the IIT model needs replication or modification and whether the fresh thrust to create purely science focused institutions like the Indian Institutes of Science Education and Research (IISERs) will lead us even further away from an ideal 'multiversity'. Whether the 'multiversity' is a desirable, or even achievable model in today's India may also need to be considered.

In his wide ranging essays, Vest touches upon almost all the issues that must concern academic administrators. He discusses the role of diversity and race in student admissions in the United States, adding that 'nothing in our times has been so bitterly contested . . . and it has not been resolved through orderly political and administrative processes. Rather, it has frequently led to acrimonious conflicts and has followed multiple pathways including public referenda and Supreme Court cases. It has torn at the heart and soul of our populations and institutions'. The issues of race and gender that are discussed in America find echoes in India where the problems are larger and need constant and sympathetic attention. Vest argues for admission procedures that enhance diversity, arguing that the nation's future is well served by building 'a future scientific and engineering workforce and leadership that reasonably reflects our population and its spectrum of cultures'. There are key issues of concern to academia that Vest addresses: philanthropy, finance, interactions with industry and the role of institutions in promoting progress of local communities. His analysis, of the role of research in universities and its relevance, will please most of us in academia: 'Ultimately, long-term basic research is what universities do best and should not be sacrificed. That said, in this age of increasingly cooperative innovation and fast-paced change, there are many opportunities to serve through "relevant" research and development that will complement, not distort, our core academic mission and bring new intellectual challenges to our faculty and students'. The role of philanthropy in building the US university system has been significant. Fund raising and maintaining endowments are central to the well being of these institutions. The rationale for private support of higher education is simply put in a quote that Vest uses, attributed to Milton Eisenhower, a president of Johns Hopkins in the 1950s and 60s: 'Higher education and business are basically inter-dependent. One needs money to produce educated people, and the other needs educated people to produce money'.

In his concluding essay, Vest asks if in the age of globalization and the Internet and 'what lies beyond' higher education will be fundamentally reshaped. He asks: 'Are residential universities dying dinosaurs or models to be propagated further?'. His personal assessment is reassuring: 'First, I remain hopelessly in love with the residential universities – with Clark Kerr's multiversity'. He argues that 'teaching is a fundamentally human activity' and points out 'that the rate of technological progress is almost always dramatically underpredicted, and the rate of social progress is almost always dramatically overpredicted'. In reinventing the university in India, we might learn from experience elsewhere.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Naming, Remembering and Recalling Things

Collection, classification, organization, storage and retrieval of information are the central occupations of the new age information scientists. The vast, chaotic and unorganized material on the world wide web miraculously organizes itself under the spell of key words and the power of Internet search engines. The tasks of classification, naming and recall seem almost too trivial to discuss in a journal of science. My attention was sharply drawn to this subject by an attractively titled column, 'Reviving the Lost Art of Naming the World' (Carol K. Yoon, *The New York Times*, 11 August 2009). The author, a regular writer on biology and biologists, begins recalling student days when she was 'tutored in the ordering and naming of life – the science of taxonomy'. Yoon notes, in sentences that have a familiar ring: 'Despite the field's now blatant modernity, with practitioners using DNA sequences, sophisticated evolutionary theory and supercomputers to order and name all of life, jobs for taxonomists continue to be in steady decline. The natural history collections crucial to the work are closeted or tossed.' Taxonomy, whose origins can be traced back to the seminal work of Carl Linnaeus (1707–1778), is now a term used not only in botany and zoology, but has permeated many other disciplines, where classification of objects becomes necessary. For example, the many diverse folding patterns of protein molecules require a 'taxonomic' classification of shapes. Yoon provides anecdotal evidence for the feeling that naming objects may be a skill that is intrinsic and independent of culture. The example that she provides in defence of her argument for the 'deep-seatedness of taxonomy' comes from the case of a patient, who lost the ability to recognize living things but continued to identify non-living objects, following a swelling of the brain as a result of a herpes attack. Although the *New York Times* column does not cite a reference, the energy and enthusiasm of a colleague, together with the power of search engines, led me to the original report. The selective impairment of semantic memory was first described by Elizabeth Warrington (*Quart. J. Expt. Psychol.*, 1975, **27**, 635) in a study which showed that cerebral lesions correlate with the 'failure to recognize or identify common objects'. In a subsequent report, years later, she described the case of J.B.R., a university student, whose memory deficits followed viral infection induced brain swelling (Warrington, E. K. and Shallice, T., *Brain*, 1984, **110**, 1273). In subsequent years many such examples have been described, including the formidably titled report 'Questioning the living/nonliving dichotomy: Evidence from a patient with an unusual semantic dissociation' (Siri, S. *et al.*, *Neuropsychology*, 2003, **17**, 630). The authors report the case of a patient J.P. who had undergone a lobectomy, following a viral attack, who also showed a severe impairment in his ability to name and identify fruits, vegetables and musical instruments. There appears to be evidence that the living–nonliving distinction may be blurred and that 'semantic knowledge is organized in the brain on the basis of object properties'. The increasingly sophisticated technologies of neuroscience, particularly magnetic resonance imaging, will undoubtedly be brought to bear on the problem of semantic memory deficits.

I would not have been drawn to this topic if Yoon in her *NY Times* column had not asked with some eloquence: 'How to tell the carrot from the cat – which to grate and which to pet?'. Her statement of the problem is compelling: 'As curious as they are, these patients and their woes would be of little relevance to our own lives, if they had merely lost some dispensable librarianlike ability to classify living things. As it turns out, their situation is much worse. These are people completely at sea. Without the power to order and name life, a person simply does not know how to live in the world, how to understand it . . . . They are utterly lost, anchorless in a strange and confusing world. Because to order and name life is to have a sense of the world around, and, as a result, what one's place is in it.' A developing hypothesis, that Yoon draws attention to, is 'that there might be a specific part of the brain that is devoted to the doing of taxonomy'. Yoon's lament about the decline of taxonomy is not new; indeed, as far back as the 1920s the alarm bells, announcing the impending demise of the field, have been rung. A couple of years ago the 300th anniversary of Linnaeus' birth provided an opportunity for biologists and essayists to celebrate taxonomy's importance in furthering our

understanding of nature. In an engaging commentary H. C. J. Godfray reflected on 'Linnaeus in the information age' (*Nature*, 2007, **446**, 259). He argues that 'Linnaeus's brilliance was that he initiated the solution to the first bioinformatics crisis: how to organize knowledge about the increasingly large number of species that were being discovered during the age of exploration'. Godfray suggests if Linnaeus had begun his work today, 'he would have been a "techie", exploiting the Internet and other modern means of sharing and coordinating data'. The Linnaean approach does indeed lie at the basis of the growing number of databases that collect, curate, organize and share the exploding information on gene sequences and expression data, protein structures, biochemical pathways, molecular structures and dozens of other bodies of data that have spawned the information revolution in biology and chemistry. A new breed of librarians is emerging schooled in the new taxonomy of scientific publications; classifying, organizing and retrieving information acquires new dimensions as the fields of science begin to merge at broad and porous boundaries. In the area of molecular bioinformatics an understanding of biological classification and the evolutionary underpinning of biological phenomena is valuable, but is almost always absent in most discussions. As the public perception on the importance of recognizing and maintaining biodiversity increases, so will the demands on taxonomy and taxonomists. There is a technological solution that is being persistently advanced which may soon dominate the field, consigning Linnaeus' descendants to an obscure corner of the discipline. The use of 'DNA barcodes' and 'hand held readers', connected by wireless technologies to gigantic computerized databases may one day enable 'bar coding' of every organism on the planet. In a curious way, 'bar coding life' (*Science*, 2005, **307**, 1037) appears conceptually related, in a taxonomic sense, to the recently launched effort in India to provide a 'unique identification number' to over a billion people. Identifying individual members of a species is an interesting task; one which 'Linnaeus the techie' might have approved of. In keeping with the generally pessimistic view of the future of classical taxonomy, the *Science* feature was entitled, 'Will DNA bar codes breathe life into classification?'. Since all plants and animals have a gene coding for the protein cytochrome oxidase I, which shows significant interspecies variation in sequence and much smaller difference within a species, an ideal identification tag appears to have been found. The estimates of named species range between 1.5 and 2 million, while the total number of organisms on earth may range between 10 and 20 million. This would catapult the 'unique bar code' project into the league of 'big science' projects with cost estimates of about $1 billion. Efforts are already underway for specific classes of organisms, with other short DNA pieces being advanced as unique identifying tags. With DNA sequencing costs falling, any effort to use DNA coded information seems to appear feasible. As high technology invades and overruns taxonomy, there are those who watch, with some discomfort, the slow extinction of the practice of careful collection, observation, annotation and classification that characterized the Linnaean approach. However, the tools of the information age are central to modern taxonomy. A commentary published during the Linnaeus tercentenary year emphasized the case for the importance of keeping track of species names, the rules governing naming and the need to encourage web-based approaches, bringing the field firmly into the electronic age (Knapp, S. *et al.*, 2007, **446**, 261). In reflecting on 'the legacy of Linnaeus' a *Nature* editorial (2007, **446**, 231) suggests that 'the realization that the second edition of his dreamed-of universal catalog would be slimmer than the first would surely strike him as a melancholy one'. A major problem for modern taxonomy is to impose and maintain codes for naming, as the number of identifiable species increases. Amateurs coexist uneasily with professionals in taxonomy; a characteristic of a field which flowered in the Linnaean era, when exploration of the planet provided the basis for new discoveries.

While technology may catalyse a revival of taxonomy, the current awareness of the importance of biodiversity and the rise of the conservation movement may provide the foundation on which modern taxonomy builds a secure future. In an editorial published in *Science* over five years ago, three leaders in biology argue that 'the great biological challenge of our age is to create a legacy of knowledge for a planet that is soon to be biologically decimated' (Wheeler, Q. D., Raven, P. H. and Wilson, E. O., *Science*, 2004, **303**, 285). But to emphasize the beauty and importance of naming, I must return to Carol Yoon and her column: 'Because once you start noticing organisms, once you have a name for particular beasts, birds and flowers, you can't help seeing life and the order in it, just where it has always been, all around you.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Teachers and Teaching

Teachers and mentors are remembered fondly on 'Teachers Day', which is observed on 5 September. Everybody remembers a favourite or influential teacher, especially as they grow older. Teachers are a diverse community. While primary and secondary school teachers deal with children in their formative years, high school teachers wrestle with the problems of groups of adolescents, in the difficult years when childhood recedes and adulthood beckons. The early undergraduate classes are almost an extension of high school; although college brings with it a sudden loosening of the constraints that sometimes seem so restrictive for teenagers on the verge of a transition. Teachers are to be found all the way up to the level of research degrees. Ph D's are earned under 'guides' and 'supervisors', who must really act as mentors and teachers. By this stage the relationships are more nearly equal, with experienced students transforming into valuable colleagues in research for their supervisors. Teaching is traditionally a term reserved for those who lecture, explain and inform in the confines of a classroom. For teachers, the blackboard and chalk were their traditional instruments; their only other requirement was a sheaf of well-thumbed notes or a prodigious memory, from which facts could be recalled at will. Times have changed in institutions of higher education, where visual aids and computers have invaded classrooms. 'Powerpoint' allows instructors to project in rapid fashion reams of information, while the internet provides an inexhaustible and valuable resource, after class, if used wisely. In the electronic age even teachers may need to change, as they move into a position where they must guide students to learn. In schools little has changed and hundreds of thousands of classrooms across India might well be described in Goldsmith's vivid verse, penned in the 18th century:

'There in his noisy mansion, skilled to rule,
The village master taught his little school.
A man severe he was, and stern to view;
I knew him well, and every truant knew;
Well had the boding tremblers learned to trace
The days disaster in his morning face
Full well they laughed with counterfeited glee
At all his jokes, for many a joke had he;
Full well the busy whisper circling round
Conveyed the dismal tidings when he frowned.
Yet he was kind, or, if severe in aught,
The love he bore to learning was in fault;
The village all declared how much he knew;
'Twas certain he could write, and cipher too;
. . . .
In arguing too, the parson owned his skill,
For even though vanquished, he could argue still;
While words of learned length and thundering sound
Amazed the gazing rustics ranged around;
And still they gazed and still the wonder grew
That one small head could carry all he knew.'

–The Deserted Village

Even as I write, a faint and distant memory appears of a high school teacher reciting Goldsmith, occasionally breaking into Telugu in conveying an interpretation which left even bilingual students puzzled and bemused. Memories of teachers, both kind and difficult, rarely fade. The mentors of later years, of course, remain with us to the very end.

Teaching has never been a financially rewarding profession. In a highly globalized and commercialized world, teaching careers are unattractive and few young and bright college graduates will opt to become teachers. At all levels, from school to the university, there is a growing shortage of trained, committed and enthusiastic teachers. The dramatic expansion of the higher education system, especially in the areas of science and engineering, has suddenly highlighted the great demand for faculty. It has been easy to create IISERs and IITs by decree; it appears harder to recruit faculty who will be both researchers and teachers. As the demand for increasing India's scientific productivity grows, so too will the demand for scientists in national laboratories where there is no teaching of note. The oft repeated demand by senior scientists, who influence policy, for 'more Ph Ds and more papers' is sharpening the divide between those who do only research and those who teach. The same quarters also lament the poor quality of science students, endorsing the need to strengthen the base of science teaching, especially in college at undergraduate level. While teachers seem to be wanted, they do not seem to be adequately compensated. Researchers have received many hastily conceived sops; based on the level of peer recognition, in a community in which the credibility of the rewards sys-

tem is rather low. Election to two of India's four academies, recognized for this purpose, provides the favoured few an addition to their salaries until they retire from the fray. Recipients of CSIR's Bhatnagar prize are also rewarded similarly; a curious case of providing continued compensation for the recipient of a cash award. There is no requirement for evidence of continued productivity. Unsurprisingly, large sections of faculty in national institutions are now obsessed with election to Academies, especially if they are barred by age from aspiring for the Bhatnagar prize. In such an environment there are few takers for academic programs that may serve long term goals. The unhappiness of teachers, even in the best of our institutions like the IITs, was evident on 'Teachers Day' when faculty protested even as some of their most committed colleagues were honoured by the students. The present protests follow the government's order outlining the pay formulae to be followed, as a fall out of the 6th Pay Commission. Inevitably, every Pay Commission leaves most beneficiaries unhappy; a ten-year cycle which unfailingly stirs up resentment and discord amongst those paid by the public exchequer. Each Commission must appear to be broadly consultative, going around the country raising expectations, most often to unreasonable levels. Every Commission must also leave its mark, introducing apparently innovative measures which are invariably greeted with suspicion and hostility. The 6th Pay Commission, whose recommendations are to be implemented from the beginning of 2006, has been no exception. 'Pay bands and grade pay', increments that need to be computed and 'performance linked incentives' are features that have been widely discussed. Commissions are always followed by special committees, almost as surely as night follows the day. For many central institutions of research and higher education, the waiting time before the dust settles will almost take us to the halfway mark between the 6th and 7th Pay Commissions. These debates on compensation address only the high end of the academic profession. Improvements in salary structures and recruitments in colleges and universities in different states are slow and vary significantly. Retirement ages of faculty and benefits also differ, adding new dimensions to the problems faced by teachers across the country. At school level the shortage of teachers, who are competent and knowledgeable, will continuously increase as the profession is perceived as unrewarding and unattractive. The problem of a shortage of faculty in the sphere of higher education will undoubtedly become even more severe as the new institutions grow in student strength.

Concerns about science education are global as evident from the discussions that have been going on in the United States for the past few years. A National Academies Committee on 'Prospering in the Global Economy of the 21st Century' produced a report in 2005, dramatically titled 'Rising Above the Gathering Storm'. The title evokes memories of Winston Churchill's famous volumes on World War II. The report itself is intended to draw a blueprint for 'energizing and employing America for a brighter economic future'. There are four principal recommendations which address school education ('10,000 teachers, 10 million minds'), research ('sowing the seeds'), higher education ('best and brightest') and economic policy ('incentives for innovation'). In a hark back to the days of the launch of the *Sputnik*, 'the committee notes that the nation is unlikely to receive some sudden "wake-up" call; rather the problem is one that is likely to evidence itself gradually over a surprisingly short period'. The American drive to increase the number of science and mathematics teachers is based on an ambitious plan to provide the highest quality undergraduate education to those who wish to become teachers. Financial incentives are proposed for both the students and the universities that educate them. There are also ambitious plans to strengthen skills of existing teachers through summer institutes and master's programs in science and mathematics. Interestingly, summer internships and 'inquiry-based learning' are suggested even for high school students. There are major proposals for expanding research funding and promoting early career researchers.

In considering the requirements for science and mathematics teaching, two recent editorials in *Science* offer insights. Both refer to the drive to strengthen science, technology, engineering and mathematics (STEM) education in the United States. Maxine Singer asks a key question: 'How to develop and cultivate great STEM teachers?' She notes that many teachers have no STEM qualifications and argues that scientists must 'work to help develop STEM teacher education programs that are rigorous and relevant for teaching students who have grown up in the internet era'. She notes that 'STEM teachers must . . . be considered vital members of the professional scientific community' (*Science*, 2009, **325**, 1047). The need for galvanizing science departments at research universities by transforming teaching methods and shifting the focus to 'how students learn, pedagogy issues, and evidence of learning' is highlighted by Carl Wieman (*Science*, 2009, **325**, 1181).

The driving force for current interest in STEM teaching is the need to move ahead in a highly competitive global environment. Norman Augustine who chaired the National Academies Committee ended his statement before a US Congressional Committee (2006) quoting a poem by Richard Hodgetts, that he notes 'summarizes the essence of innovation'.

'Every morning in Africa, a gazelle wakes up
It notes it must outrun the fastest lion or it will be killed.
Every morning in Africa a lion wakes up
It knows it must outrun the slowest gazelle or it will starve.
It doesn't matter whether you're a lion or a gazelle –
when the sun comes up, you'd better be running'.

Teaching and teachers will help us run faster.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Cooperation, Conflict and Competition

'Survival of the fittest' is a phrase that is most often used as a succinct, but misleading, summary of the essence of modern ideas on biological evolution. A decade before Darwin's *Origins*, Tennyson wrote these famous lines in 1849:

'*Who trusted God was love indeed*
And love Creation's final law-
Tho' Nature, red in tooth and claw
With ravine, shrieked against his creed.'

In later years, Tennyson's image of 'Nature, red in tooth and claw' has been used as a poet's view of the struggle for survival in the natural world, most often in discussions of Darwinian ideas. Tennyson's verse may have been inspired by an 1849 pamphlet entitled 'Vestiges of the Natural History of Creation', which was published anonymously at that time. The idea of competition and conflict as drivers of evolution seems implicit in the phrase 'survival of the fittest'. Selection and adaptation are less often perceived as important in the common view of evolution. Extrapolation from biology and nature to human affairs has made the phrase 'survival of the fittest' an apt descriptor of a ruthlessly competitive world, in which the 'fittest' move ahead, while the less endowed fall by the wayside. While conflict and competition seem to be commonplace, it is surprising that biology abounds with examples of cooperation and altruism. Human civilizations have also developed over centuries by evolving models for cooperation and conflict resolution, albeit imperfect models which often breakdown. As a science administrator, I have often wondered at the reluctance of scientists to collaborate and cooperate and the difficulty of generating a passion for shared visions and goals. The individual always seems to take precedence over the collective. I was therefore immediately drawn to a recent essay entitled 'On the origin of cooperation', written to commemorate the 'Year of Darwin' (Pennisi, E., *Science*, 2009, **325**, 1196).

The social insects, organisms where 'workers' display remarkable discipline in working in orderly fashion 'for the good of the colony', attracted Darwin's attention, posing as Pennisi notes, 'one special difficulty which first appeared to me insuperable and actually fatal to my theory'. In the century and a half that has followed, Darwinian ideas and the existence of cooperation and altruism have been reconciled. Organization of multicellular forms of life requires synchrony and cooperation between individual cells; flocks of birds, swarms of bees, herds of elephants, groups of monkeys and schools of fish are examples of cooperation in nature. Dissimilar organisms can cooperate forming unique ecosystems; 'nitrogen fixing bacteria team up with plants', an example that Pennisi cites. There are others that are evident in our environment; the termite mounds, that sometimes tower in silence on our campuses, are the product of cooperation between the termites and a specific soil microorganism that enter into a symbiotic relationship. E. O. Wilson termed the fungus-farming termites, 'One of evolution's master clockworks, tireless, repetitive, and precise, more complicated than any human invention and unimaginably old'. In her essay, Pennisi advances the compelling view that cooperation, so pervasive in biology, may be considered as 'the third pillar of evolution, alongside of mutation and natural selection'. From Darwin's day, kinship has been advanced as a major element in cooperation. Pennisi highlights a key issue that must strike a chord in most readers, who wonder about the benefits to individuals of cooperating with larger entities: 'But cheaters – those who benefit without making sacrifices – are likely to evolve because they will have an edge over individuals who spend energy on helping others, thus threatening the stability of any cooperative venture'. In emphasizing the importance of genetic relatedness as a basis for cooperative behaviour, Pennisi quotes Haldane who is reported to have, famously, said: 'Would I lay down my life to save my brother? No, but I would to save two brothers or eight cousins'. In human societies cooperation appears to transcend genetics. Trivers' concept of 'reciprocal altruism', in his own words quoted by Pennisi, is summarized in readily understood terms: 'You scratch my back, and I'll scratch yours'. Large scale cooperation in humans is driven by complex factors and is a subject that has fascinated both sociologists and game theorists. The fact that human societies may be anomalies in the animal world and the mounting evidence that genes and kinship may be

overwhelmed by other factors has attracted researchers from diverse fields. 'Culture', a term that is harder to define than 'gene', is often invoked to rationalize human behaviour. Cooperation is important in the progress of fields as distant as science and commerce.

The 'nature of human altruism' has been analysed in many different studies. Ernst Fehr and Urs Fischbacher argue that 'there is much individual heterogeneity and the interaction between altruists and selfish individuals is vital to human cooperation. Depending on the environment, a minority of altruists can force a majority of selfish individuals to cooperate or, conversely, a few egoists can induce a large number of altruists to defect' (*Nature*, 2003, **425**, 785). The literature of sociobiology and human behaviour abounds with terms that are widely used in everyday life; cooperation, punishment, reputation and reward. Game theorists describe individuals with 'heritable strategies' as 'defectors', 'cooperators' and 'shunners', although the last descriptor is used in a manner that needs some explanation (Panchanathan, K. and Boyd, R., *Nature*, 2004, **432**, 499). A recent study authored by groups with interests in evolutionary dynamics and economics has a title that is immediately attractive: 'Positive interactions promote public cooperation' (Rand, D. G. *et al.*, *Science*, 2009, **325**, 1272). These authors use the device of the 'public goods game', which all of us engage in during the course of daily life. The authors note that 'reducing CO_2 emissions by driving fuel efficient cars and minimizing waste is a global public goods game'. So too is 'cleaning your dishes at home and doing your share of work at the office'. In contrast to many previous studies, Rand *et al.* conclude 'reward is as effective as punishment for maintaining public cooperation and leads to higher total earnings'. These authors suggest that 'reward outperforms punishment in repeated public goods games and that human cooperation in such settings is best supported by positive interactions with others'. The game theory literature is full of everyday terms and problems; the analyses and conclusions, based on mathematical models and simulation, are often equivocal. Nevertheless, the factors that govern large scale behaviour are beginning to be slowly understood. There are some lessons that might even be learned from the titles of papers in this area. I particularly liked the title 'Winners don't punish', a conclusion that seems statesmanlike (Dreber, A. *et al.*, *Nature*, 2008, **452**, 348). These authors note that within the 'framework of direct reciprocity, winners do not use costly punishment, whereas losers punish and perish'. Biologists and game theorists have been attracted by the possibility that 'defectors' and 'cheaters' can be distinguished from 'cooperators'. Similarity appears to promote sympathy. Recognition, in a biological sense, may be rooted in genetics and chemistry. Even physical similarity appears to be important in evoking trust in a game. A flurry of studies in this area point to the great interest that is focused on understanding the determinants of human cooperation (Sigmund, K., Sympathy and similarity: The evolutionary dynamics of cooperation. *Proc. Natl. Acad. Sci. USA*, 2009, **106**, 8405; Antal, T. *et al.*, Evolution of cooperation by phenotypic similarity. *ibid*, 2009, **106**, 8597; Krupp, D. B. *et al.*, A cue of kinship promotes cooperation for the public good. *Human Behaviour*, 2008, **29**, 49).

Cooperation within groups and conflict between groups are common features in the animal kingdom and rampant in human societies. In a provocatively titled essay, 'Conflict: Altruism's midwife', Samuel Bowles argues that the 'grisly evidence of a warlike past may help explain our distinctly cooperative nature'. He suggests that this distasteful idea is based on the evolution of 'parochial altruism', but notes that this 'is puzzling from an evolutionary perspective because both altruism and parochialism reduce fitness or material well-being compared with what a person would gain were he or she to eschew these behaviours'. Ethnic, racial and caste conflicts are rationalized by the idea of 'parochial altruism', but Bowles looks forward to a better future when he suggests 'that a parochial form of altruism is part of the human legacy, it need not be our fate' (Bowles, S., *Nature*, 2008, **456**, 326).

In science and in academic institutions the advantages of cooperation and collaboration are obvious. The rise of the multi-authored paper, the growing importance of interdisciplinary research and the need for large, collegial and cohesive teams in many areas of cutting edge research have resulted in a dramatic increase in scientific collaboration. Paradoxically as the global scientific environment becomes even more intensely competitive, alliances are forged between individuals, groups, institutions and even countries in an effort to survive and move ahead in the face of competitive pressures. The reward system in science has always promoted individuals over groups; credit sharing has been a potent inhibitor of cooperation. Curiously, in the globalized environment of higher education and research, 'survival of the fittest' may be a term that may soon apply to disciplines, departments and institutions. 'Fitness' may indeed be enhanced by cooperation and collaboration. Indian institutions must begin to think of a future where collective endeavour creates an environment which encourages both cooperative success and individual achievement. 'Cooperators' must eventually overwhelm 'cheaters'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Water: Molecule, Material and Resource

Biographies come in various forms. Many are hagiographies which sing paens of praise to a famous individual, providing little by way of insight into the lives of the subject. Others are scholarly tomes of formidable size, which deter the average reader from going beyond the prefatory chapter. The best biographies are those in which readers are drawn into the life of the subject, unencumbered by detail, providing a portrait that arouses interest and, on occasion, inspires. Biographical essays are always attractive, short enough to capture a reader's attention, sometimes focusing on relatively obscure and forgotten subjects. Readers are invariably drawn to both the successes and failures of famous individuals. During an idle period of browsing in a bookshop, almost inevitably located in an airport, I was drawn to a volume curiously titled, *H_2O: A Biography of Water* (Phillip Ball, Phoenix, Orion Books, London, 2000). Can a molecule, a material and a resource be the subject of a legitimate biography? The author, an accomplished and compelling writer on chemistry and materials, has produced a narrative that breathes life into a subject that must interest both scientists and general readers. As a molecule water epitomizes simplicity; its formula known to almost everyone who has studied science at school. As a material it is encountered constantly in daily life. As a resource it is widely discussed, with doomsday scenarios predicting future wars over water. As I turned the pages, snatching a quick look at paragraphs in random order, my decision to invest in the book was sealed by the title of chapter 2: 'Blood of the Earth. Seas and Rivers of the World'. This section begins with a quote from Leonardo da Vinci's *Notebooks*, which I must reproduce:

'*. . . as man has within him a pool of blood wherein the lungs as he breathes expand and contract, so the body of the earth has its ocean, which also rises and falls every six hours with the breathing of the world. . . .*'

The idea of the Earth as a giant living organism conjures up an image that is remarkably similar to the Gaia hypothesis advanced centuries later by James Lovelock. In a recent book *The Revenge of Gaia* (Penguin, 2006), Lovelock notes that he continues 'to use the metaphor of "the Living Earth" for Gaia; but do not assume that I am thinking of the Earth as alive in a sentient way, or even alive like an animal or bacterium. I think it is time we enlarged the somewhat dogmatic and limited definition of life as something that reproduces and corrects the errors of reproduction by natural selection among the progeny'. Lovelock argues that 'unless we see the Earth as a planet that behaves as if it were alive, at least to the extent of regulating its climate and chemistry, we will lack the will to change our way of life and to understand that we have made it our greatest enemy' (Lovelock, pp. 20–22). Water is of course central to any discussion of 'climate and chemistry'.

I have been drawn in this column to water, by all the recent excitement about the discovery of water on the moon following the Chandrayaan mission. Phillip Ball describes 'lunar prospecting' for water as 'looking for gold in the gutter' (p. 28) adding that 'its discovery elsewhere continues to be greeted with surprise and excitement. Yet planetary scientists and astronomers know better – they know that water pervades the Universe. . . . The issue then is not whether there is water elsewhere, but whether it is liquid – for only in the liquid state does water seem to be capable of providing the matrix of life'. Ball is eloquent when he says: 'A tour through the solar system reveals a rich panoply of watery environments – some irrevocably icy, some steamy, some more barren than the harshest Arctic tundra, some laced with ancient, dried-up waterways. . . and some perhaps, awash with the precious fluid, tempting us to speculate about the life that might be lurking in these celestial oceans' (p. 78). Three recent papers focus renewed interest on lunar water. 'The Moon Mineralogy Mapper (M^3)' on the Indian spacecraft Chandrayaan-1 'has now detected absorption features near 2.8 to 3.0 μm on the surface of the Moon' is a succinct statement that summarizes the first report (Pieters, C. M. *et al.*, *Sciencexpress*, 24 September 2009). Two accompanying reports revisit data from earlier fly-by missions; Cassini in 1999 and Deep Impact in June 2009 (Clark, R. N., *Sciencexpress*, 24 September 2009; Sunshine, J. *et al.*, *ibid.*, 24 September 2009). The moon, of course, must have had plenty of water delivered by comets to its surface over eons. Clark cites an estimate of '10^{13} kg of water over 2 billion years, or about 0.5 kg/m^2'. Even a cursory reading of these reports suggests that most stories in the popular press of 'moon water' and the possibility of 'human settlements' are wildly exaggerated. The paper

by Pieters *et al.* ends provocatively: 'Perhaps most importantly, harvesting the lunar regiolith for volatiles now becomes a serious option for long-term human activities'. There are ongoing missions that will probe the surface of the Moon, undoubtedly prospecting for water. A Lunar Crater Observation and Sensing Satellite (LCROSS) is due to 'slam into a polar crater' on 9 October, 'with the intention of ploughing up a plume of water ice for many telescopic eyes to see' (Hand, E., *Naturenews*, 18 September 2009). While advocates of plans for manned flights and 'lunar settlements' seem to have been energized by the detection of absorption bands of water, Phillip Ball strikes a contrary note: 'To my mind, if the day comes when we have made our occupation of the Earth so unsustainable that we have to look to so barren a place as the Moon to rescue us, we are in deep trouble. In terms of resources the Moon doesn't have a great deal to offer' (Ball, p. 80). He has an interesting point of view on manned flights: 'Perhaps the most socially important product of these flights was gained by a look backwards – at the blue globe of the world. In comparison with that, it will take more than a few frozen lakes to make the Moon look like an attractive place to live' (p. 81).

Water, as a molecule, made its first appearance at the beginning of the 19th century when John Dalton began to wonder about 'the union of atoms into compounds'. Ball describes this early history noting that 'the picture of a water molecule as a union of two atoms of hydrogen with one of oxygen is a long way from Democritus' idea of an element comprised of rounded, slippery, indivisible particles. And yet this is just our starting point. For there is no clue here as to why water is so special, why it is the quintessential liquid and solvent, the stuff of snowflakes and glaciers and the essence of life on Earth' (p. 139). Students of science learn about water's anomalous properties quite early in their careers. Water is such a familiar substance that most people find it difficult to believe that much about water is still shrouded in mystery. Ball quotes Felix Franks who many years ago summarized the view of experts: 'Of all the known liquids, water is probably the most studied and least understood'. Ball is an outstanding expositor of science. In discussing water's anomalous properties he draws attention to the fact that 'water's large heat capacity means that warm ocean currents can carry a phenomenal amount of heat. The Gulf Stream which ultimately keeps Northern Europe warmer than Labrador (at the same latitude) by carrying heat from tropical South America northwards across the Atlantic Ocean, bears with it every day twice as much heat as would be produced by burning all of the coal mined globally in a year'. I learnt about the Gulf Stream in high school geography decades ago but I am sure that we were never told that 'the effect of ocean circulation on global climate depends on . . . water's most pronounced anomalies'. Water and its properties were heatedly discussed in chemistry departments in the late 1960s and early 1970s, when 'polywater' was 'discovered'. This form of water was produced in thin capillaries by Russian scientists and quickly became enmeshed in controversy. The Cold War as Ball notes produced 'a cloak of paranoia between East and West that has not been matched since'. J. Desmond Bernal, one of the earliest to propose a structure for liquid water, allowed ideology to cloud his judgement when he hastily pronounced his verdict, that 'this (polywater) is the most important physico-chemical discovery of the century'. Ball's account of the polywater saga, as controversy turned into a debacle, is gripping; there must be a tinge of regret that genuine errors of technique and judgement might have blighted otherwise commendable careers in science. For Indian readers there are nuggets. Ball recalls the episode in the mid-1990s when 'an Indian chemist named Ramar Pillai claimed to have found a way to turn water into "petrol" – a yellow inflammable liquid – by brewing it with a local herb'. I can almost hear the murmurs of protest at the description of the protagonist as 'an Indian chemist'. Water has been central to some of science's most hotly debated controversies in recent times. The 'cold fusion' story of Martin Fleischmann and Stanley Pons was based on the electrolysis of water. The aim of producing energy from water can be seductive and Ball describes the rise and fall of cold fusion in compelling terms. The 'memory effect' exhibited by water in the infamous paper published in *Nature* by Jacques Benveniste was based on experiments that 'claimed that infinitely diluted reagents retain their biological effectiveness'. The aftermath left both the authors and *Nature* bruised. Ball recalls that his 'Ph D supervisor, a physicist, widely respected for his work on the liquid state' remarking with a grin 'So you are going to work for a journal that believes water has a memory'. The Benveniste experiments appeared to provide a basis for the apparent efficacy of homeopathy, water serving as the 'vehicle for information'. Why is water the subject of continuing study? In Ball's words: 'Water is inspirational. It infuses us with spirit. It quickens the heart to watch a clear, gurgling brook or the pounding of wild surf. The rhythmic lap of the waves on a beach soothes and reassures. So we bring to the study of water an emotional response that does not accompany the investigation of, say, bismuth.'

The epilogue of Ball's book treats water as a resource – blue gold. He reminds us of Coleridge's *Ancient Mariner*: 'Water, water, everywhere, nor any drop to drink'. Even as Andhra Pradesh and Karnataka reel under unprecedented floods, as rivers rage and dam gates must be opened, the problems of water as a resource are both pressing and difficult. The technologies of desalination are not yet readily accessible to address shortages. The management of both drought and floods is constantly discussed, with solutions being elusive. Water is essential for life but water, as Ball notes, can also be a destroyer. He quotes the Book of Job:

'*If he holds back the waters, there is drought*
If he lets them loose, they devastate the land.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Nobel Reflections

The world of science is catapulted into the public eye each year in October, when the Nobel prizes are announced. Recognition from Stockholm is considered the pinnacle of achievement in science; the prestige of the prize conferring an unmatched aura of distinction on its recipients. The awards in science generally recognize work and achievement, which are usually very well known amongst the practitioners of each field. The only discussion that accompanies the awards is on the names of those who have been overlooked, sometimes squeezed out by the limitation that restricts the number of recipients to three. This is in marked contrast to the Peace and Literature Nobel prizes, which generally stir up a lively controversy. In an editorial that appeared days before this year's announcements, the journal *Nature Chemistry* reflected on the soon to be awarded prize, noting that the Nobel Committee's decision would be accompanied by 'the usual speculation, angst, disagreement and elation'. The journal added: 'It has been said that the history of the Nobel Prize is the history of science. This may be true, but only to the extent that history itself is written as the story of monarchs and generals – in other words, ignoring the story of everyday people who make up the bulk of the population' (*Nature Chemistry*, 2009, **1**, 509). I was struck by the phrase 'monarchs and generals', which immediately conjured up visions of dominating, managerial scientists commanding large work-forces, that appear to have become important in the world of 'big science'. Happily, when this year's awards were announced the only 'monarch and general' was awarded the Peace Prize, while the scientists and economists seemed to be model researchers, whose efforts and success may inspire a new generation of committed scientists. The history of science is replete with examples of exemplary work done by individuals and small groups, choosing to work courageously and indefatigably on important and difficult problems; following trails of research that can be formidably difficult and unfashionable, at times.

The Nobel Prize in Physiology or Medicine was an anticipated recognition of the work done by Elizabeth Blackburn, Carol Greider and Jack Szostak, which led to the discovery of 'how chromosomes are protected by telomeres and the enzyme telomerase'. Their findings have implications in the areas of human aging and cancer, raising the hope that new therapeutic approaches to cancer may emerge from studies of inhibition of the enzyme telomerase. This award recognizes basic research in molecular biology, with applications in medicine yet to materialize. Molecular and structural biology has also been emphasized by the award of the prize in Chemistry to Venkataraman (Venki) Ramakrishnan, Thomas Steitz and Ada Yonath, who made independent and remarkable contributions towards determining the detailed atomic level architecture of ribosomes. These magnificently assembled macromolecular complexes are the site of protein synthesis in living cells; they are the stage on which the genetic message is translated into proteins, molecules which are central to all of life's chemistry. Enzymes, of which telomerase is an example, lie at the heart of biochemistry catalysing myriad reactions in cells with astounding specificity and efficiency. In reading of the work of this year's laureates I could not help but recall a statement of the late Arthur Kornberg: 'In my theater the nucleic acids write the script but the enzymes do the acting'. The determination of the atomic level three-dimensional structure of the ribosome is a technical *tour de force*, a tribute to the dramatic progress of X-ray crystallography and cryoelectron microscopy over the past two decades. The ribosome structure reveals a breathtakingly detailed view of the process of protein synthesis; its complexity reminding us of how little we know about the evolution of the chemistry of life (Schmeing, T. M. and Ramakrishnan, V., *Nature*, 2009, doi:10.1038/nature 08403). Nearly half a century ago, in 1962, the Nobel awards in medicine and chemistry recognized the birth of molecular and structural biology, with Watson, Crick and Wilkins honoured for their elucidation of the double helical structure of DNA, while Perutz and Kendrew were recognized for the determination of the first protein structures, haemoglobin and myoglobin, by X-ray crystallography. This year's awards highlight the growing intimacy of chemistry and biology, with the hope that the confluence of these disciplines will fuel the progress of medicine. The ribosome structure provides insights into the mechanisms by which specific antibiotics inhibit protein synthesis in bacteria, promising new lines of attack in the perennial battle against infectious disease. While the Medicine and Chemistry prizes rewarded pure basic research, the Nobel Prize in Physics recognized seminal work that has impacted modern life in spectacular

fashion. Charles Kao's development of optical glass fibres has revolutionized communication. From the first discovery in the mid 1960s to the production of ultrapure fibres which could transmit light over long distances took only a few years; developments which preceded the birth of the Internet by many years. The present day applications of optical fibres could scarcely have been anticipated in the 1970s, reaffirming an old saying that 'the best research will someday be applicable'. The second half of the award in physics recognized 'the invention of an imaging semiconductor circuit – the CCD sensor'. The charge coupled device (CCD) has led to digital photography and is at the heart of a large number of instruments, which must convert light into electrical signals. This is a prize that recognizes work that lies at the borders of engineering and has been one of the drivers of the information revolution. The Economic Sciences prize instituted by the Sveriges Riksbank, in memory of Alfred Nobel, recognizes work in the area of what may be loosely termed as 'behavioural economics'. The laureates, Elinor Ostrom and Oliver Williamson work in distinctly different areas. Ostrom is cited for her work in the 'analysis of governance, especially the commons', while Williamson has been honoured 'for his analysis of economic governance, especially the boundaries of the firm'. Ostrom's work challenges, in the words of the Nobel announcement, 'the conventional wisdom that common property is poorly managed and should be either regulated by central authorities or privatised'. This is an area of great relevance in India, where the management of the 'commons' is an increasingly difficult and contentious arena. Her work contributes in a major way to the debate initiated four decades ago by Garret Hardin in his famous essay on 'The Tragedy of the Commons' (*Science*, 1968, **162**, 1243). Hardin perceived a 'need to impose public or private ownership on all natural resources'. This is a view that Ostrom has challenged by studying the models by which cooperative groups manage common resources. In a recent study she concludes: 'When users are genuinely engaged in decisions regarding rules affecting their use, the likelihood of them following the rules and monitoring others is much greater than when an authority simply imposes rules' (Ostrom, E. and Nagendra, H., *Proc. Natl. Acad. Sci. USA*, 2006, **103**, 19224). I suspect that Ostrom's work may be of special interest to policy makers in India, who wrestle with the problems of the environment, water and land resources.

Inevitably, the Nobel awards stir up a minor controversy or two every year. 2009 is no exception. Chemistry has, in recent years, been redefined by the Nobel Committee. The phenomenal growth of molecular biology has resulted in a dramatic expansion of the borders of chemistry. The award for the work on the green fluorescent protein in 2008 has been followed by the recognition of the ribosome structure this year. Traditional chemistry seems to be out of favour. But even a cursory glance at the structure of the ribosome and what has been learned about the mechanisms of protein synthesis in cells, will reveal a richness of structural and mechanistic chemistry that can hardly be matched in the laboratory. Some years ago I heard a seminar speaker proclaim: 'Chemistry is the engine that powers biology'. It might also be true to say that 'biology is the fuel that powers chemistry today'.

This year's awards had a special significance in India. Venki Ramakrishnan's Indian origins, catalysed a remarkable outpouring of public interest, after the Nobel announcement; even the reflected glory of a Nobel prize can be dazzling, at times. The adulation suddenly dissolved into controversy; the rapid transformation demonstrated the power of the media to influence opinion. Ramakrishnan's understandable discomfort at the deluge of e-mails and phone calls from India and his characterization of his Indian origins as an 'accident of history' have been widely reported and discussed. But even as I write, an extraordinary piece of invective has appeared. While such pieces normally deserve to be ignored, the fact that the author happens to be an articulate Member of Parliament and a 'spokesman of the Congress party' suggest that a comment may be merited. Abhishek Singhvi writing in the *Times of India* (20 October 2009) argues that Ramakrishnan's somewhat plaintive request to be spared the excessive adulation, suggests that he is in some way insensitive to the 'patriotic' urges that come to the fore, when an Indian (or one of Indian origin) gets a major international award. Curiously, Singhvi is aware, as he should undoubtedly be, that patriotism can be 'the refuge of the scoundrel'. He notes that patriotism 'has an intersection of noble values which in this case, appear to have completely escaped the mind of a brilliant Nobel laureate'. Singhvi adds that 'success has many fathers while failure is an orphan', a phenomenon that is also widely observed in the West. Singhvi's diatribe is both distasteful and inappropriate, coming as it does from one who is distinguished in public life. He would do well to remember that scientific success can sometimes be an orphan in India. Ironically, one of the founders of the field of structural biology, an area recognized by this year's Chemistry prize, was an Indian; G. N. Ramachandran who determined the structure of collagen in the 1950s and developed the conformational analysis of protein chains in the 1960s, at Madras University. Ramachandran died in 2001, unhonoured by the Government of India even in the annual Republic Day awards, which are given by the dozen every year. Cholera researchers also celebrate the 50th anniversary of Sambhu Nath De's famous work on cholera toxin this month. De died in 1985 unhonoured even by the Indian scientific community. The fact that Ramachandran and De did not get the call from Stockholm may only be an 'accident of history'. Patriotism can often be misplaced. Our reactions to this year's Nobel prize in chemistry are undoubtedly an example.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Biology and Chemistry: Life and Death

A few days ago a report in the newspaper asked a provocative question in its title: 'Did algae kill off the mighty dinosaurs?'. The subheading suggested a possible cause: 'Toxins released by tiny organisms may've played major role in 5 mass extinctions' (*Times of India*, 31 October 2009). Mass extinctions also featured in *The Economist* (24 October 2009) under an even more intriguing heading: 'I am become Death, destroyer of worlds'. Both reports seemed to focus on the causes of the sudden disappearance of dinosaurs about 65 million years ago. The generally accepted (or so I thought) view is that the cataclysmic events following the impact of an asteroid on Earth exterminated the dinosaurs. Nearly three decades ago, Luis Alvarez and his colleagues advanced an 'extraterrestrial cause for the Cretaceous–Tertiary extinction' (Alvarez, L. W. *et al.*, *Science*, 1980, **208**, 1095). Using the higher iridium abundance in deep sea limestones, as a diagnostic for an extraterrestrial origin, these authors argued that an 'earth-crossing asteroid would, upon impact, inject about 60 times the object's mass into the atmosphere as pulverized rock. The resulting darkness would suppress photosynthesis and the expected biological consequences match quite closely the extinctions observed in the paleontological record'. They estimated the diameter of the asteroid as 10 ± 4 kilometers. The possible site of impact was later suggested to lie in the Yucatan peninsula, in what is now Mexico. The Chicxulub crater appears to have a diameter (180 km) that may account for the proposed asteroid impact. *The Economist* article raises an interesting possibility 'that everyone was wrong'. The report describes a presentation made by Sankar Chatterjee of Texas Tech University which suggests that a much bigger crater, 500 km across located off the coast of Mumbai, may be a better candidate. 'Bombay High', an underwater mountain, is evidence for a squeezing upwards of magma, following the dramatic impact. Chatterjee has christened the crater as Shiva, a tribute to the awesome power of the impact. There are anomalies in extinction theories following asteroid impact which suggest that this will remain an area that will continue to be a fertile ground for new hypotheses. While there is compelling drama in asteroid impact scenarios, I found the suggestion that algal toxins are likely suspects in mass extinction, intriguing. Here, death would come slowly, creeping up unseen upon the victims.

The algal toxin hypothesis has been advanced in a recent paper by J. W. Castle and J. H. Rodgers Jr (*Environmental Geosciences*, 2009, **16**, 1), 'based on evidence from the geologic record and modern environments'. In this scenario the responsibility for mass extinctions of life that have taken place over hundreds of millions of years rests on microorganisms, whose ability to produce poisonous molecules seems unmatched. Castle and Rodgers base their hypothesis on the fact that 'mass mortalities of invertebrates, fish, birds and mammals caused by algal-produced toxins are occurring in modern environments'. They note that 'in addition to direct effect of these toxins, the large mass of organic material produced by algal blooms can lead to oxygen depletion during decay, which indirectly causes death of some biota'. The authors base their hypothesis on geologic evidence that is obtained from the fossil record, which 'demonstrates a pronounced increase in abundance and environmental range of algae, including stromatolitic cyanobacterial mats, coincident with Phanerozoic mass extinctions'. The evidence for the alleged involvement of cyanobacteria in the mass murder of other forms of life seems circumstantial; their entombment in rocks that date back to the geological period of the mass extinctions and their ability to cause death in modern environments seem to come together in allowing Castle and Rodgers to point an accusing finger. Unschooled, as I am, in the subjects of geology, microbiology, paleobiology and ecology, this report would not have attracted my attention if the *Times of India* subeditor had not chosen a dramatic headline 'Did algae kill off the mighty dinosaurs?'. Curiously, the authors of the original paper are circumspect. They provide evidence for 'increased microbial activity associated with mass extinction' for the first four of the five major extinctions that are now established. The most recent that occurred about 65 million years ago (End Cretaceous) does not seem to have a clear correlation with any rise in cyanobacterial populations. It is the end of the Cretaceous period, which follows the Jurassic (a name brought into the public consciousness by Michael Crichton and Steven Spielberg), which marked the end of the dinosaurs. The possible role

of algal blooms in mass extinctions assumes importance in view of the various projections for global warming, climate change and the attendant consequences. Castle and Rodgers provide a warning note: 'Environmental changes such as climatic warming, sea level fluctuations, and increased nutrient supply may have promoted algal blooms over vast expanses of marine to freshwater environments. From the increasing frequency of modern, toxin-producing algal blooms, which may be related to global warming, another massive biotic crisis could be forthcoming'.

In thinking about the unseen agents of large scale extinctions, I was reminded of an essay entitled 'Planet of the Bacteria' written by Stephen Jay Gould over a dozen years ago (*Washington Post Horizon*, 1996, **119**, 344). Gould begins, in characteristically engaging style, noting that his 'interest in paleontology began in a childhood fascination with dinosaurs'. He recalls the old method of dividing the fossil record 'into a series of "ages" representing the progress that supposedly marked the march of evolution: the "Age of Invertebrates" followed by the Age of Fishes, Reptiles, Mammals and finally, with all the parochiality of the engendered language then current, the "Age of Man" '. In his critique of this system of developing a 'history of life', Gould argues that 'we certainly should honor life's constant mode. We live now in the "Age of Bacteria". Our planet has always been in the "Age of Bacteria" ever since the first fossils – bacteria, of course – were entombed in rocks more than 3 billion years ago'. Gould's essay must be required reading for every student of biology and all those interested in the history of living organisms. The antiquity of bacteria, their resilience over geological time scales, their chemical virtuosity and their central role in the ecological cycles which recycle organic matter justifies Gould's assertion: 'On any possible, reasonable or fair criterion, bacteria are – and always have been – the dominant forms of life on Earth'. His eloquence is evident when he concludes: 'Bacteria have been the stayers and keepers of life's history'.

The microbial world also throws up somewhat more complex unicellular organisms, fungi, which are also capable of producing an amazing repertoire of molecules, some of which prove to be extremely potent toxins. Recent interest in the role of microorganisms in causing a precipitous decline in amphibian populations has focused on a group of fungi called chytrids, specifically on the formidably named organism *Batrachochytrium dendrobatidis*, which rapidly kills frogs. 'The pathogen infects over 350 species of amphibians and is found on all continents except Antarctica' (Fisher, M. C. *et al.*, *Annu. Rev. Microbiol.*, 2009, **63**, 291). This ongoing extinction provides an unparalleled opportunity to examine a biological phenomenon that must have repeatedly shaped the planet's biodiversity. An analysis of an outbreak of chytridiomycosis in Panama concludes that 'it is no longer correct to speak of global amphibian declines but, more appropriately, of global amphibian extinctions' (Lips, K. R. *et al.*, *Proc. Natl. Acad. Sci. USA*, 2006, **103**, 3165). These authors suggest that a commonly held view that infectious disease may not lead to extinction may need to be reexamined. They note that 'understanding the causes and consequences of diminishing biodiversity and understanding the ecology and evolution of infectious diseases are two of the eight grand challenges in environmental sciences'. The mechanisms by which microorganisms commit mass murder are invariably mediated by molecules, most often misleadingly called 'secondary metabolites'. It is clear that microbes invest a considerable genetic effort in synthesizing complex organic molecules, which target other organisms with a frightening degree of efficiency. Chemistry is central to both life and death. While a large number of algal (cyanobacterial) toxins have been identified, the molecular weapons used by the frog killing fungus remain unknown. Can toxicity be avoided? Curiously, frogs may host another bacterium on their skin which in turn produces a metabolite that eliminates the pathogenic fungus (Bruckner, R. M. *et al.*, *J. Chem. Ecol.*, 2008, **34**, 1422). A renaissance of an old discipline, 'natural products chemistry' is undoubtedly being catalysed by the growing realization that novel molecules may be central actors in shaping the biological world.

The morbid side of chemistry is wonderfully highlighted in a delightful book, entitled *Molecules of Murder: Criminal Molecules and Classic Cases*', by John Emsley (Royal Society of Chemistry, 2008). He examines some of the most famous cases of poisoning 'from the point of view of the forensic scientist with a special focus on the toxic agents involved'. In a section titled 'From medicine to murder', Emsley features some of chemistry's most infamous molecules; ricin, a protein for which a lethal dose for an adult is estimated to be as little as 70 micrograms, the alkaloid hyoscine and the more common chemical, adrenaline. A second section establishes that extremely simple molecules can exhibit high toxicity. Emsley's text is compelling as he mixes chemistry and murder; each case proving to be a formidable forensic challenge. But Emsley's catalog of the 'molecules of murder' can be no match for the weapons of mass destruction that have been perfected over hundreds of millions of years by microbes. Biology and chemistry merge seamlessly when we consider life and death.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Climate Change: Uncertain Science, Certain Controversy

Ever since the Nobel Peace Prize was conferred on Al Gore and the UN Intergovernmental Panel on Climate Change (IPCC) in 2007, there has been widespread acceptance of the fact that we are in the midst of rapidly changing weather patterns. The connection between rising carbon dioxide (CO_2) levels and global warming is now common knowledge. No area of science has captured the public imagination as quickly as the science of climate. CO_2 and methane, once limited to classes in chemistry, are now familiar substances, always in the public eye as discussions on 'greenhouse gases' are now commonplace. The connection between rising atmospheric CO_2 levels and global temperature rise is largely accepted as an established fact. A recent editorial in *Science* (2009, **325**, 1599) by Steven Chu, the US Secretary of Energy and a Nobel laureate in Physics begins emphatically: 'Overwhelming scientific evidence shows that CO_2 emissions from fossil fuels have caused the climate to change, and a dramatic reduction of these emissions is essential to reduce the risk of future devastating effects'. Several alarming scenarios have been widely projected; rising oceans that submerge low-lying coastal regions, melting glaciers, a slow loss of the polar ice cap, altered rainfall patterns, droughts and floods. The rise of disease, with malaria anticipated to be pre-eminent among the public health issues of concern in Africa, parts of Asia and Latin America in future, will be an inevitable consequence in these scenarios. Public interest in climate change issues is mounting and the impending summit in Copenhagen has further fuelled interest in this area. International treaties and protocols have been more honoured in the breach. Whether Copenhagen will be more successful than Kyoto, in achieving a global consensus in evolving a collective strategy, remains to be seen. In the build-up to the UN climate talks there is already an atmosphere of pessimism. A *Nature* editorial observes that 'the pessimism has spread so widely that it could be considered a global pandemic'. The price for the excesses of the twentieth century must be paid in the twentyfirst. Dramatic reductions in CO_2 emissions cannot be achieved without a substantial cost. There may be little support for a view that the developed nations, particularly the United States must drastically cut back on emissions, even while large developing economies like China and India continue to expand industrially, with an attendant increase in the overall share of global CO_2 emitted. The 'clean and green' energy technologies are still on the drawing board, with some strategies being prohibitively expensive, at present.

In the midst of all the concerns on global warming, a report released by the Ministry of Environment and Forests presents data to suggest that fears about receding Himalayan glaciers may be ill founded. The report is in sharp contrast to the 2007 IPCC report, which presented an exceedingly alarming picture, suggesting a complete disappearance of the glaciers in about three decades. The new report (Raina, V. K., Himalayan Glaciers: A State-of-Art Review of Glacial Studies, Glacial Retreat and Climate Change, 2009), specifically notes that the Gangotri and Siachin glaciers have hardly shown any abnormal retreat over the last several years. An interesting analysis of this report suggests that experts are divided on the data. Projections do not seem to be completely supported by observations. The analysis concludes on a predictable note: 'Forecasts hold little water, so only a robust observation campaign will reveal whether the third pole's resistance to climate change is durable or ephemeral (Bagla, P., *Science*, 2009, **326**, 924). Elsewhere in the Himalayas, glaciers seem to be melting. In a news feature under a section titled 'Road to Copenhagen', *Nature* carries a report that cites the IPCC study: 'Glaciers in the Himalayas are retreating faster than in any other part of the world and they could completely disappear by 2035'. The article suggests that Bhutan is at a special risk with its '983 glaciers and 2794 glacial lakes some of which have burst to produce deadly glacial lake floods' (Nayar, A., *Nature*, 2009, **461**, 1042). This grim assessment is based on research carried out over the last few years, triggering some effort in 'adaptation'. The resources for mitigating measures must presumably come from the Least Developed Countries Fund set up as a result of UN Climate Change talks in 2001. Difficult terrain is a major impediment in realizing technical solutions to minimize the effects of glacial floods. High resolution satellite imagery may be valuable in furthering studies of glaciers, but it is not clear whether the expenses for such

projects can always be met by the countries that may need the data the most. Are glaciers melting or not, elsewhere in the world? A recent study of Mount Kilimanjaro in Africa suggests that 'glacier loss on Kilimanjaro continues unabated' (Thompson, L. G. *et al.*, *Proc. Natl. Acad. Sci. USA*, 2009, online doi: 10.1073/pnas.0906029106). This analysis notes that 'of the ice cover present in 1912, 85% has disappeared and 26% of that present in 2000 is now gone'. Is this loss of ice cover directly related to global warming? The authors are cautious in drawing firm conclusions about the causes for the Kilimanjaro meltdown: 'Regardless of the contributions of various drivers, the ice fields atop Kilimanjaro will not endure if current conditions are sustained and adaptive action to minimize the potential impacts should be developed quickly'.

The build-up to Copenhagen in the most visible science journals has been impressive. Both *Nature* and *Science* have had special sections and several editorials and news reports. The positions of India and China are discussed in 'Opinion' pieces by R. K. Pachauri and Jiahua Pan, respectively (*Nature*, 2009, **461**, 1054 and 1055). Pachauri notes that sea level rises along the Bangladesh coastline and increasing frequency of cyclones 'could result in large numbers of migrants fleeing to India'. He adds: 'Equally serious are the problems associated with glaciers melting in the Hindu Khush region', arguing that this will lead to a 'decline in river flows' and consequently, water scarcity. Should these assessments be correct, we can anticipate a grim future. A National Action Plan on Climate Change (NAPCC) has been unveiled. It remains to be seen how plans translate into purposeful action and the extent to which economic development and mitigation measures can be balanced. The titles of the 'Opinion' pieces are a study in contrast. The article on India is entitled, 'India pushes for common responsibility', suggestive of a stand that we as a country may shoulder a significant burden. The Chinese piece is entitled 'China expects leadership from rich nations', suggesting that a greater price must be paid by those who have substantially contributed to the global climate problem, while developing at a rapid pace over the last century. The failure of the Kyoto Protocol to make a significant dent in global CO_2 emissions and the general air of pessimism about the outcome at Copenhagen have led to suggestions that it may be worthwhile to reexamine the Montreal Protocol model, which was successful in curbing the use of chemicals that depleted the ozone layer. It is twenty years since the Montreal agreements came into force. *Nature* notes editorially that there was an important difference at that time: '. . . the protocol was ready for signing just two years after the hole was discovered – in part because the threat was immediate, and because chemical alternatives to chlorofluorocarbons had already been developed' (*Nature*, 2009, **460**, 781). The situation with CO_2 emissions is dramatically different. There are no real, economically viable alternatives to fossil fuel and there are no readily applicable technologies for CO_2 sequestration. While energy research is a high priority it may take many years or even decades for 'clean' and 'sustainable' energy strategies to evolve into cost-effective technologies. Although the Montreal Protocol is advanced as a model for international cooperation, a pragmatic assessment may suggest that compliance was possible only because technological solutions were available. The recent special section in *Science* (2009, **325**, September 25) highlights many strategies for carbon capture and storage, all of which appear to be far from practical realization. Steven Chu's editorial appears wistful when he examines the ways in which CO_2 is produced, stored and used in the natural world. 'Can we enhance natural processes ("reforestation plus") or draw inspiration from nature for artificial capture? Similarly, nature provides proof that the energy penalty for releasing adsorbed CO_2 in post-combustion capture can be decreased: Through carbonic anhydrases our blood captures CO_2 created by cell metabolism and releases it in the lungs with no enthalpic energy penalty?' (*Science*, 2009, **325**, 1599).

Global warming and climate change issues appear to be addressable only by reducing the use of fossil fuel and cutting CO_2 emissions. While many scientific issues need to be resolved, including the extent to which doomsday scenarios are reliable, there is no doubt that energy usage must be both sustainable and 'clean'. In the absence of a clear alternative to fossil fuel, international negotiations seem to be the only way to secure global cooperation. For a country like India there are many pressures that need to be understood and neutralized. In an 'Opinion' piece in this issue (pp. 1414–1415), Dilip Ahuja rings the alarm bells. He draws attention to a well orchestrated campaign by the West to force India to accept policies which may not be entirely in the country's interest. His view must be read by all those who influence policy. He points out that high profile Western campaigners for climate change, 'whether in power or retired, come to Delhi wanting to talk to the Prime Minister on climate change. There is no real difference in the message between those from North America or the European Union'. His concerns about the comments of 'many analysts, both Western and of Indian origin' and the role of international NGOs and pliable media in undermining the Indian position merit attention. Copenhagen will soon be upon us. While the science of climate remains uncertain, it is a certainty that climate change negotiations will be contentious and controversial.

P. Balaram

CURRENT SCIENCE

Volume 97 Number 11 10 December 2009

EDITORIAL

Scientific Research: Stupidity and Success

Lectures and committee meetings are often occasions to learn a new phrase. At the annual meeting of the Indian Academy of Sciences, C. N. R. Rao, one of India's most productive and celebrated scientists, spoke about scientific research and the joys and pains of academic pursuits. He observed in passing, that the results of scientific research must be taken to the logical conclusion with investigators publishing their findings in a respectable scientific journal. This 'last mile', a phrase used by Nandan Nilekani in his public lecture on the unique identification number, is sometimes difficult to traverse. Rao noted that many scientists succumb to 'referee fatigue', a wonderfully evocative phrase that immediately conjures up the image of tired and defeated authors, who crumble under the combined onslaught of disagreeable and unkind reviewers and unsympathetic editors. The task of acquiring a formidable list of publications in the best of scientific journals is not an easy one; patience, commitment, resilience and enthusiasm are among the key ingredients for success. The pressures to publish in the best of journals are growing in India and fresh entrants to academia are thrown into a competitive arena, where the obstacles to success loom large at every step. The worldwide competition for journal space in the highest impact publications is intense; referees and editors are often driven by the pressures of current fashion, with the result that careful, scholarly work can often be easily dismissed. The front offices of some journals are sometimes manned by editorial staff, who carry out preliminary review to assess 'importance' of the findings. In such situations current fashion ('nanostructures', 'stem cells' or 'climate change' among others) may score over academic depth and scholarship. Writing styles are important; the best introductory paragraphs often promise a 'paradigm shift' – a term more misused than understood. Editors are under great pressure to drive up impact factors, while university administrators try to drive up institutional rankings. The young (and sometimes the even not so young) academic scientist can thus be subjected to a considerable degree of pressure. In the Indian academic scene the proliferation of awards, academies and incentives add a local flavour to the competitive stress imposed. Stresses must necessarily be transmitted and indeed they are; often influencing even students who are on the verge of beginning a research career. For authors who seek to publish their work the hurdles of the anonymous peer review system must be crossed; rejection letters must be received with the level of equanimity that is necessary to allow the author to take a deep breath and try again. Most often, the visibly successful scientists are those who plough on undeterred by local criticism or difficult referees. The tendency to succumb to 'referee fatigue' must be resisted; cheerful enthusiasm is always an effective antidote.

How does one embark on a career in science? Ambition may be a necessary condition but can, in isolation, never be a sufficient condition for success in science. Even as I wondered whether there could be a recipe for success in research, a young and enthusiastic student sent me an essay, with an eye-catching title: 'The importance of stupidity in scientific research' (Schwartz, M. A., *J. Cell Sci.*, 2008, **121**, 1771). Two helpful colleagues also drew my attention to this piece, although unlike the innocent student, they may have had less than honourable intentions. In Schwartz's words: 'Science makes me feel stupid too. It's just that I've gotten used to it. So used to it, in fact, that I actively seek out new opportunities to feel stupid'. Feeling stupid is a term that Schwartz uses to describe a state of mind that I (and, hopefully, other researchers) often encounter—an inability to understand or solve a problem that appears distressingly simple. At times, subjects and concepts that one has repeatedly encountered in courses and examinations years ago seem incomprehensible and not easily applied to the research problems at hand. Often, research and the background acquired over years of courses and examinations seem strangely disconnected. Problems, even minor ones, encountered in everyday research appear more difficult to solve than those encountered in examinations for which solutions undoubtedly already exist. Schwartz suggests that Ph D programs do not always make students 'understand how hard it is to do research'. He adds: 'We don't do a good enough job of teaching our students to be productively stupid—that is, if we don't feel stupid it means we are not really trying . . . . Productive stupidity means being ignorant by choice. Focusing on important questions puts us in the awkward position of being ignorant.

One of the beautiful things about science is that it allows us to bumble along, getting it wrong time after time and feel perfectly fine as long as we learn something each time'. On reading this I could not help feeling that the 'bumbling' approach to science, that I must confess I love and which Schwartz describes so simply, is fast disappearing. Science today is a highly organized and professional activity; projects must be perfectly conceived, approaches to research must be planned to the last detail and the results are almost always anticipated. The organized, managerial scientist found in academia today has little time for 'bumbling' and certainly no appreciation for 'stupidity'. Ignorance, wide-eyed enthusiasm and wonder, which can be useful attributes in research, are rare qualities, even as research laboratories abound with 'specialists', all of whom seem to have answers to every problem that is posed. Most often, neat hypotheses only need to be proved by experiment; any questions raised by laboratory work are dismissively attributed to poor experimental skills of graduate students, who are often sacrificed on the hard road to scientific success.

How does one become a scientist? Bruce Alberts, a former President of the US National Academy of Sciences and Editor of *Science*, suggests that 'one normally becomes a scientist through a series of apprenticeships, pursuing research in laboratories directed by established scientists' (*Science*, 2009, **326**, 916). It is during these 'apprenticeships' that students learn how to do research. It is in this phase of a career that students must learn to be, in Schwartz's words, 'productively stupid'. Schwartz also notes that 'scientific education might do more to ease what is a very big transition: from learning what other people once discovered to making our own discoveries'. Alberts wisely points out: 'Because so much of one's scientific future is shaped by early experiences, it is critical that beginning scientists select their mentors wisely. Unfortunately, what constitutes a 'good' choice is not always obvious . . . The best research advisers . . . provide their graduate students with enough guidance to prevent them from wasting time on nonproductive pursuits, while giving them freedom to innovate and learn from their own mistakes'. Here there is an echo of the need to be 'stupid' and the importance of mistakes. In research, as indeed in life, the most useful lessons are learnt from mistakes. The launch of an independent scientific career generally requires 'two apprenticeships'. The first, of course, is the period spent as a student acquiring a Ph D degree, while the second is the stint as a postdoctoral fellow in a different laboratory. Alberts advises prospective scientists to choose their areas well so that the 'skills acquired are complementary'. He does, however, sound a cautionary note: 'But success as an independent scientist will require much more than technical skills'.

India has embarked on an ambitious phase of expansion of its institutions of higher education and research. There has never been as much interest in rejuvenating the science and technology enterprise as in the last few years. There are many oft repeated slogans: 'more Ph Ds', 'more laboratories', 'more publications', 'more innovations', 'more patents'; indeed 'more' of everything connected with science and research. A critical problem in this phase of expansion is the need to recruit new faculty and students in large numbers. Existing institutions must also enter a phase of adjustment as the countrywide expansion plans begin to gather momentum. This may be an opportune moment to ask whether the system of graduate (Ph D) training in India can be critically examined and if fledgling post-doctoral programs need strengthening and expansion. The needs of young faculty members, both material and somewhat less tangibly, intellectual, require understanding. I suspect that there is a pervasive feeling in our institutions that financial resources, 'infrastructure' and 'efficient administration' are the only limiting factors in determining research success.

There seems to be little discussion within institutions and departments on the need to promote a collective approach towards building up an intellectual climate that promotes scholarship. An environment which appreciates the 'importance of stupidity' (especially 'productive stupidity') would be wonderfully energizing. Unfortunately, we have gone overboard in 'incentivizing' research, with the result that most young researchers in major laboratories appear to believe that they must achieve recognition milestones (awards and fellowships) at regular intervals. The pressures to publish in the best journals, with the attendant side-effect of referee fatigue, are quickly transmitted from mentors to apprentices leaving little time for being 'stupid'. It is clear that we cannot return to more leisurely times. How does a beginning scientist cope with the pressures of today? Alberts suggests that 'senior scientists have the responsibility of maintaining a system that provides talented young scientists with the opportunity to succeed in whatever career they choose'. Mentoring young faculty is always more difficult than influencing fresh students. As a beginning, we might do well to recognize the importance of 'stupidity'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Microbiology: An Endless Frontier

Over the last four decades the molecular revolution in biology has transformed the life sciences, washing away the bridges between many allied disciplines. Many years ago, Arthur Kornberg lamented that the turbulent advance of 'molecular biology' had washed away the bridges to biochemistry and the traditional approach to the molecules of life. Fortunately, the dramatic rise of structural biology, fuelled by X-ray diffraction, NMR spectroscopy, computational methods and more recently a host of powerful new techniques for investigating molecules, assemblies, cells and tissues, has ensured that chemistry and physics are essential for the solution of important problems in biology. In India, the curriculum for students of the mushrooming courses in 'biotechnology', provides little of the background that is needed to savour the excitement of modern biology. Having never been formally schooled in biological subjects, I have watched with amazement the expansion of the field and the deep inroads made by other disciplines into an area that was once the preserve of those who studied botany and zoology. The most exciting and challenging problems of chemistry undoubtedly lie in biology. The question of what constitutes biology has always intrigued me, since I have many times been advised by molecular biologists that the subjects that seem of great interest to an outsider like me are hardly worthy of attention. Years of listening to dismissive judgements have taught me to read the literature, searching for arguments to bolster my case that biological research needs to be enthusiastically pursued on a much broader front in India. Thus, when the latest volume of the *Annual Review of Microbiology* (eds Gottesman, S. and Harwood, C. S., 2009, vol. 63) appeared on my desk for assignment of a reviewer, I spirited the book away hoping to read authoritative perspectives about an area that must surely be viewed as biology's 'endless frontier'. How does one become a microbiologist? This question seemed to find an interesting answer in Lars Ljungdahl's prefatory chapter entitled 'A life with acetogens, thermophiles and cellulolytic anaerobes' (pp. 1–25). The author describes his entry into a four year technical secondary school, on the day World War II broke out, and adds that 'mathematics, physics, chemistry and mechanical engineering' were emphasized; excellent training for an entry into biochemistry and microbiology. Even a cursory glance at prefatory chapters in most related annual reviews (biochemistry, genetics, immunology, medicine, phytopathology and plant biology) will reveal that many immensely productive scientists in these areas have received training in disciplines other than biology or a specialized sub-discipline.

Idly browsing through the volume of *Annual Reviews* my attention was drawn to an article entitled 'Anaerobic oxidation of methane: progress with an unknown process' (Knittel, K. and Boetius, A., *Annu. Rev. Microbiol.*, 2009, **63**, 311). Could the chemistry of the oxidation of as simple a molecule as methane still be unknown? The abstract provided an equation that surely has no parallel in the laboratory: $CH_4 + SO_4^{2-} \rightarrow HCO_3^- + HS^- + H_2O$. While an editorial column is not the place for a chemical equation, I cannot resist the temptation to wonder at nature's amazing chemical virtuosity. Here is an example of the largely unreactive hydrocarbon, that has acquired notoriety as a 'greenhouse gas, which has so far contributed an estimated 20% of postindustrial global warming', being smoothly converted to other substances that seem more benign, by a process that is hardly understood in molecular detail. The authors lament: 'A great deal of biogeochemical research has focussed on the causes and effects of the variation of fluxes of methane throughout earth's history, but the underlying microbial processes and their key agents remain poorly understood. This is a disturbing knowledge gap, because 85% of the annual global methane production and about 60% of its consumption are based on microbial processes'. Geology, chemistry and microbiology appear to intersect in any attempt to understand the anaerobic oxidation of methane in the seabed, which is the home for the anaerobic methanotrophs, the microbes that carry out this remarkable chemistry. The review by Knittel and Boetius draws attention to 'consortia' of microorganisms involved in methane oxidation, suggestive of the role of multiple groups of methanogens in the natural processes that determine methane fluxes. The idea that biogeochemistry may be controlled by communities of organisms is increasingly gaining ground. Indeed there is a growing body of literature that suggests that studies of genes extracted from isolates of microbial communities may provide valuable biological insights; a significant departure from a more traditional view that genomics is best applied to 'pure' organisms that can be grown in culture in the laboratory.

The first large scale study 'to explore environmental bacteria in a culture-independent manner by isolating DNA from environmental samples' appeared a little over five years ago. In a paper entitled 'Environmental genome shotgun sequencing of the Sargasso Sea', Craig Venter and his colleagues 'generated, annotated and analysed' as many as '1,045 billion base pairs of non-redundant sequence' (*Science*, 2004, **304**, 66). These authors estimate that their isolate contained as many as '1800 genomic species . . . including 148 previously unknown bacterial phylotypes'. This study threw up as many as '1.2 million previously unknown genes'. I was attracted to this report since the Sargasso Sea has been famously featured in fiction, with Jules Verne's immortal Captain Nemo diving into its waters in his submarine, in *Twenty Thousand Leagues under the Sea*. Verne's description bears retelling: '. . . cold, quiet immovable ocean called the Sargasso Sea, a perfect lake in the Open Atlantic. . . . Such was the region the Nautilus was now visiting, a perfect meadow, a close carpet of seaweed, ficus and tropical berries. So thick and so compact that the stem of a vessel could hardly tear its way through it'.

The oceans cover approximately 70% of the earth's surface. Much of the methane, that I began this column with, originates below the ocean floor. However over 90% of the methane produced is consumed by bacteria that inhabit the 'sulfate-penetrated seafloor zones' by the process of anaerobic oxidation (*Annu. Rev. Microbiol.*, 2009, **63**, 312–313). Clearly, marine microorganisms are a key element in controlling methane fluxes; even more interestingly, the potential for carrying out unusual chemistry by marine organisms is both vast and untapped. The 2004 paper by Venter *et al.* launched a new approach to studying microbial communities and diversity in the oceans, demonstrating that the techniques of shotgun DNA sequencing may allow rapid progress in our understanding of microbial evolution, ecology and chemistry. Indeed the initial report in *Science* in 2004 discusses the 'photobiology of the Sargasso Sea', noting that this global gene-fishing approach yielded 'more than 782 new rhodopsin-like photoreceptors'. Such studies provide an extraordinary wealth of genomic data. When translated into protein sequences they add an entirely new dimension to studies of sequence evolution. The competing imperatives of random mutations and selective pressures for maintaining biochemical function find reflection in this large body of sequence data. 'Data mining' (a particularly infelicitous term) may present great opportunities for gaining useful biochemical and biological insights, although this may require thoughtful analysis, which is not blinded by the brute power of computational methodology.

There is a spirit of romance and adventure in exploring the oceans. Just over two years ago the reports of the *Sorcerer II* expedition appeared in *PLoS Biology*, grouped as a part of the 'Oceanic metagenomics collection'. An engaging commentary provides the background to this fascinating study (Gross, L., 'Untapped bounty: Sampling the seas to survey microbial diversity', *PLoS Biol.*, 2007, **5**, e85). The expedition drew inspiration from the 19th century *Challenger* expedition (1872–76), mounted by the British, which Gross notes, 'discovered hundreds of new genera and nearly 5000 new marine species, its gun stations replaced with research stations. . .'. The 21st century expedition traversed 9000 kilometers, periodically pumping seawater 'through a series of increasingly fine filters' which were then frozen and stored for DNA isolation. The sequences, referred to as Global Ocean Sampling (GOS) sequences, must turn out to be a treasure trove for computational biologists, who revel in handling thousands of sequences, looking for patterns that might reveal the hidden hand of evolution fashioning the relationships between protein structure and biological function. The *Sorcerer II* expedition has added dramatically to the number of predicted sequences of proteins, many of which do not have a known biological function. In describing an 'expanding universe of protein families', Venter and his colleagues note 'that new (protein) families are being discovered at a rate that is linear or almost linear with the addition of new sequences, implying that we are still far from discovering all protein families in nature' (Yooseph, S., *PLoS Biol.*, 2007, **5**, e16).

Microbiology is being transformed as environmental ('meta') genomics gains ground. A great deal of current interest in microbial ecosystems stems from the realization that the fluxes of the two major gases that contribute to global warming, carbon dioxide and methane, may be controlled by microbial metabolism. Microbes which remained hidden from the prying eyes of scientists, until Leeuwenhoek invented the microscope, coexist comfortably with insects, plants and animals; the symbiotic relationships are often mutually beneficial. Microorganisms can provide complementary and essential biochemistry to facilitate survival of hosts; the gut is a comfortable habitat for many bacteria. Biology is full of examples of cooperation and coexistence. 'Metagenomics' has been applied to bacterial communities inhabiting the hindgut of the wood-feeding termite. The ability of termites to degrade wood is 'important both for their roles in carbon turnover in the environment and as sources for biochemical catalysts for efforts aimed at converting wood into biofuels' (Warnecke, F. *et al.*, *Nature*, 2007, **450**, 560). The metagenomic analysis revealed 'a large diverse set of bacterial genes for cellulose and xylan hydrolysis'. Intriguingly, genes for hydrogen (H_2) metabolism, CO_2 conversion and N_2 fixation were also found, emphasizing the versatility of microbial chemistry. The authors note that 'termites may be considered to be rich reservoirs of bacterial enzymes' adding that their 'study illustrates how complex a 1 μl (microliter) environment can be'. The millions of microorganisms that inhabit both terrestrial and marine environments must surely be regarded as an endless frontier for chemical biology.

P. Balaram

CURRENT SCIENCE

Volume 98 Number 1 10 January 2010

EDITORIAL

Scientific Publishing: Eroding Trust

Publishing papers in the best of scientific journals appears to be getting harder. Papers authored by groups of researchers may soon need detailed declarations of the contributions made by each author, a feature already introduced by some journals. In a recent editorial, Bruce Alberts, the editor-in-chief of *Science* outlines changes of policy, designed to discourage 'honorary authors'. He cites a 1994 report of the US National Academy of Sciences, which states that 'just providing the laboratory space for a project or furnishing a sample used in the research is not sufficient to be included as an author' (*Science*, 2010, **327**, 12). Chemists and biologists, among others, who often labour long and hard to produce samples may wonder how they will be rewarded for their efforts. The 'importance of intellectual contributions' is emphasized, presumably making it difficult to have papers with dozens, sometimes hundreds, of authors; a common feature in papers in areas like experimental high energy physics, genome sequencing and multi-centric clinical trials. Interestingly, Alberts announces a new requirement: 'The senior author for each laboratory or group' will be asked to 'confirm that he or she has personally reviewed the original data generated by that unit, ascertaining that the data selected for publication in specific figures and tables have been appropriately presented. Thus, for example, a researcher who prepares a digitally processed figure displaying an assortment of electrophoretic gel separations will need to present all of the original gel data to a specified senior author, who must certify that this has been done when the manuscript is returned for revision.' Figures containing 'gel bands' have, of course, acquired considerable notoriety in many high profile misconduct cases. Alberts' editorial entitled 'Promoting Scientific Standards' appears to have been motivated by a spate of recent retractions of papers published in the 'highest impact' scientific journals and by the unusual step taken by the editor in publishing an 'Editorial Expression of Concern' (Alberts, B., *Sciencexpress*, Dec 17, 2009, 10.1126/science.1186078).

Journals and editors are increasingly under pressure to curb the rising tide of papers which attract adverse attention. A recent paper with an apparently catchy title, 'Reactome Array: Forging a Link Between Metabolome and Genome' (Beloqui, A. *et al.*, *Science*, 2009, **326**, 252), has been widely discussed on the Internet, resulting in the journal's unusual step of 'expressing concern'. The paper describes 'a sensitive metabolite array for genome sequence – independent functional analysis of metabolic phenotypes and networks, the reactomes of cell populations and communities. The array includes 1676 dye-linked substrate compounds collectively representing central metabolic pathways of all forms of life'. The task of chemically synthesizing nearly 1700 dye conjugated substrates for specific cellular enzymes seems forbidding. Unfortunately, the introductory figure in the paper does not appear to have engendered a great deal of confidence in the chemistry, leading to a public discussion on the validity of the 'reactome strategy' (Travis, J., *Science*, 2010, **327**, 22). The rise of 'high throughput' methodologies, systems biology, where the individual reactions of classical biochemistry fade into anonymity under the cloak of networks, and the growing need to develop 'arrays' have driven away the conservatism that characterized biochemistry in its formative phases. Efraim Racker's oft quoted dictum, 'never waste clean thoughts on dirty enzymes' is unlikely to influence a new generation of biological scientists. The need to characterize substrates, especially when synthesized by the hundreds, poses a considerable challenge. Whether the 'reactome' study will stand the test of time remains to be seen. There is more trouble in a field, structural biology, that has long been thought to be largely free of deliberate data fabrication. The recent admission by the University of Alabama, Birmingham that as many as ten papers reporting protein crystal structures may have to be retracted, has been a rude shock to the community of structural biologists (Borrell, B., *Nature*, 2009, **462**, 970). The extensive discussion on these structures on the Internet raises important questions on the review processes at the best of journals. An editorial comment entitled 'Black sheep among the flock of protein structures' notes that while 'mistakes do occur', in the current case 'it appears that the retracted structures were deliberately fabricated and there is no evidence that any experimental data were actually collected' (Dauter, Z. and Baker, E. N., *Acta Crystallogr.*, 2010, **D66**, 1). The papers reporting the rogue structures appeared in *J. Biol. Chem.*, *J. Mol. Biol.*, *Cell*, *Proc. Natl. Acad. Sci. USA*, *Nature*, *Biochemistry*

and *Acta Crystallogr. D*. All of these journals have a high reputation and some are amongst the most sought after journals by ambitious authors. Curiously, data fabrication has also surfaced in the area of small molecule crystallography; long considered as an area made routine by automated data collection and structure determination methods. As many as 70 papers in *Acta Crystallographica Section E*, a journal used as a structure report repository, have been retracted. The papers emanate from two different groups in Jingganshan University in China (Zhang, H. *et al.*, *Acta Crystallogr.*, 2010, **E66**, e11–e12; Liu, T. *et al.*, *ibid*, 2010, **E66**, e13–e14). An editorial in the journal acknowledges that the 70 structures are 'demonstrated to be falsified' (Harrison, W. T. A. *et al.*, *Acta Crystallogr.*, 2010, **E66**, e1–e2).

The spate of problems at high profile journals seems unending. Two papers emanating from the Scripps Research Institute, describing the use of the methods for incorporating non-coded amino acids, for preparing glycosylated proteins have been retracted (Service, R. F., *Science*, 2009, **326**, 1610; Hayden, E. C. and Dalton, R., *Nature*, 2009, **462**, 969). The experiments described in the papers published in 2004 in *Science* and *Journal of the American Chemical Society* appear to have been difficult to replicate, by researchers following up on the initial work, in the same laboratory. Curiously and unhappily, the case has been complicated by an e-mail extortion attempt; clearly a sign of the times. Even classical chemistry has been affected by the virus of doubtful science. A paper with a chemically provocative title, 'Reductive and Transition-Metal-Free Oxidation of Secondary Alcohols by Sodium Hydride' (Wang, X. *et al.*, *J. Am. Chem. Soc.*, doi:10.1021/ja910615z) has been quietly withdrawn after publication, 'on scientific grounds'. For the uninitiated, I might add that sodium hydride is invariably used as a base or reducing agent by synthetic chemists. A claim that the reagent does the opposite – oxidation, was predictably met with skepticism. The expected questions about the process of peer review have been raised in the examples that I have cited. For a long time, misconduct and exaggerated claims seemed more common in the biomedical literature. The widely discussed Jan Hendrik Schon case in physics and the recent examples in structural and chemical biology and chemistry suggest that no discipline can be really free of doubtful science and inappropriate practice. For the last few years there has been a great deal of discussion on plagiarism. This is the easiest form of misconduct to detect. Fabrication and falsification can be much harder to spot.

Are there any lessons to be learnt and are there environmental pressures and influences that promote aberrant behaviour? These are questions that must be considered by scientists in India, even as the pressures to be competitive build up. In the United States, the pressures to obtain research support, which often includes salary support, can be stifling. In countries like China and India the drive to increase the number of publications by offering incentives can be dangerously corrupting. Some institutions offer monetary incentives for publishing papers, scaled by journal impact factors. There cannot be a better catalyst for promoting dubious practices in science. In India, a bloated reward system offers monetary incentives of various kinds, ostensibly for enhancing research performance. The monthly bonus offered to scientists who are elected to fellowships of two academies or those who receive CSIR's Bhatnagar awards are examples of schemes that will promote a scramble for these accolades. The DST's J. C. Bose Fellowship, which should really provide stable research support, now carries an enhanced monthly bonus. This profusion of monetary incentives for 'performing scientists' has distorted the view of many researchers. A long list of publications, preferably in journals with high impact factors, is sometimes desirable in order to enter the 'circle of recognition'. For a privileged few, pedigree and connections may suffice. Some years ago none of this may have really mattered. Unfortunately, attaching a regular monetary bonus to Academy fellowships, the J. C. Bose fellowship and Bhatnagar awards distorts the view of many practitioners of science. Research must be enjoyable, satisfying and intellectually stimulating. Publications must be a measure of the enthusiasm that scientists have for their disciplines. The pursuit of recognition and reward cannot become an end in itself. Genuine mistakes and errors are unavoidable. Disagreements between collaborators are not uncommon. These are a part of the human element in science. Fabrication and falsification erode the base of trust on which science is built.

January is the time of the year when science in India is in the public eye, with the inauguration of the Science Congress by the Prime Minister. As in the past, the need for minimizing bureaucracy and increasing institutional autonomy has been highlighted at the Thiruvananthapuram Congress. These are matters that government can act upon. There are many other issues on which the scientific community alone can have an influence. There needs to be some level of introspection in institutions, academies and government science departments, if the environment for research is to be improved in India. The cursory survey of recent problems in international science that I began with in this column, suggests that the importance of promoting good practices in science cannot be overstated. Mentoring at all levels may be valuable. Alberts notes: 'Effective mentoring is critical to the future success of science. . . . Scientists everywhere can and should do more to promote it'. This is a sentiment that we should all endorse.

P. Balaram

CURRENT SCIENCE

Volume 98 Number 2 25 January 2010

EDITORIAL

Imitating the Bayh-Dole Act: Incremental Innovation

In simpler times academic institutions had two major objectives, teaching and research. Universities were intended to disseminate knowledge and also produce new knowledge. Academia was supposedly a refuge for intellectuals, who often worked and worried about arcane subjects. The rise of the research institutions promoted a pronounced divide between research and teaching, resulting in a rapid decline in the academic ambience of the traditional university. In more recent times there is a third strand that has entered academic campuses, with mounting talk of industry interactions. In the West, the enmeshed troika of university–government–industry has been referred to as the 'triple helix'; a term that must be familiar to some readers as a description of G. N. Ramachandran's model for the structure of the protein collagen, in which three strands are intertwined. Collagen is a robust protein, forming the matrix of connective tissue in animals, and the term 'triple helix' conjures up an image of deeply enmeshed, three-way interactions between academia, government and industry. Are these linkages really robust and healthy? This is a question that has been widely discussed in the West, particularly in the United States. Exactly thirty years ago, the Bayh-Dole Act was passed in the US; a piece of legislation that dramatically altered the way in which universities approached the task of reaping the fruits of the commercial success of innovations and inventions, that had their origins in university research. In reviewing the Bayh-Dole Act, a quarter century after its passage, Donald Kennedy, then editor-in-chief of *Science*, noted in an editorial: '. . . the US government renounced intellectual property claims on research supported by federal funds in universities or other nongovernment institutions. The argument in its favor went this way: Because few patents were being issued on government-funded work, scientists and their institutions needed an incentive to patent their discoveries and then license the new technology for development into useful products' (*Science*, 2005, **307**, 1371). The experience in the United States has been mixed. The 'corporatization' of the university has not been an unmixed blessing. The role of the professor–entrepreneur, the emergence of a strong feeling that universities must obtain returns on research (if not on investment), the presence of proximal industry parks where graduate students may obtain, in Kennedy's words, 'offshore employment in mentor's startups' and the creation of venture funds in universities have all been widely discussed. At the quarter century mark Kennedy's assessment of the impact of the Bayh-Dole Act is cautious: 'Inconsistency and ambivalence prevail. We want technology transfer, but we resent those who take federally supported work add some value, and receive a return on investment. The same National Institutes of Health (NIH) that urges nonprofit publishers to give that value away properly declines to make drug manufacturers to sell drugs cheaply if they were derived from NIH research. Some scientists resent control over material transfer; others insist that they are essential. Critics decry the "corporatization" of the university, yet academic/corporate collaborations flourish.' Kennedy concludes his assessment noting that the Bayh-Dole Act 'has neither a sunset nor a reauthorization requirement, but after a quarter-century it may be time to measure the innovation it has created and to balance that against the costs to universities, their faculties, and public trust in science'. The changing face of US universities in the last two decades has worried many analysts and Kennedy asks rhetorically: 'Has the developing thicket of patents and licenses created what Eisenberg and Heller called a "knowledge anti-commons", stifling communication among scientists?'. In any discussion of the role of universities in catalysing technological revolutions the famous examples of Genentech and the University of California San Francisco and Stanford and Google are oft cited. An interesting recent survey provides 'The Universities Patent Scorecard™'. The trademark on the title is most appropriate (www.patentboard.com). A listing of '2009 University Leaders in Innovation' has the Massachusetts Institute of Technology (MIT) at the top, followed by the University of California, Stanford and the California Institute of Technology (Caltech). The scorecard ranks the top 124 institutions using patents granted in the US as an index. Relatively few non-US institutions appear, although the numbers are growing. Interestingly, universities account only 'for 2% of all US utility patent issuances in the past year'.

Why is a piece of US legislation that is thirty years old of any interest to readers of this journal? I have turned to this topic because of the introduction of a Bill in the Parliament, 'The Protection and Utilisation of Public Funded Intellectual Property Bill, 2008', which may soon come up for consideration. In the run-up to the drafting of the Bill, the intentions of government were clear. The Bill

was intended to be India's version of the Bayh-Dole Act and its purpose was to spur the process of innovation and protection of intellectual property rights (IPRs) in universities and research institutions. Since almost all research in Indian academic institutions is publicly funded, the move to divest 'government' of a share in the IPR generated will afford legal sanction to current practices in many institutions. It will also resolve the differences in the approaches of different departments of government, that support scientific research in academic institutions. Bills are difficult documents to draft. Lawyers (and bureaucrats) revel in the ambiguous phrase, creating fertile ground for future litigation and debate. Since this is a piece of legislation that concerns the practice of scientific research and the institutions where future innovation must be nurtured, there is a need to carefully examine the implications of every section of the Bill. This is not a piece of legislation that needs to be hurried through the parliamentary process. Even a cursory (and I must confess uninformed) reading suggests that the Indian scientific community must ensure that clarifications are obtained and modifications made wherever necessary. The 'Statement of Objects and Reasons' is unambiguous and clearly worded: 'The proposed legislation will enhance awareness about intellectual property issues, especially in universities, academic and research institutions. It will also increase the responsibility of universities, academic and research institutions to encourage students, faculty and scientists to innovate. Such innovations can be utilized for raising financial resources of these establishments through royalties or income. The income from intellectual property will promote self-reliance and will minimize dependence of universities, academic and research institutions and other recipient organizations for Government funding'.

I am certainly apprehensive (and I suspect many ordinary people are) whenever a document drafted by lawyers crosses my desk. The language is forbidding and often ambiguous, leading to the feeling that multiple interpretations are possible. This must benefit lawyers as every contentious issue must then be resolved by litigation, with judicial interpretations sometimes varying at different levels. My attention was drawn to point 17 of the Act which says: 'Nothing in this Act shall apply to any intellectual property generated out of scholarship, fellowship and grant given by the Government, primarily, for education purposes'. Research in our institutions is carried out almost exclusively by Ph D students (the ubiquitous JRFs and SRFs) most of whom are usually supported by publicly funded scholarships. These fellowships are intended to serve an educational purpose, principally training students for research. It is these students who also man the frontlines in the laboratories. Any intellectual property that is generated is highly likely to bear the imprint of this workforce. Would the new Act then place all such work outside its purview? There are other sections that bear careful scrutiny and in places it is not clear if the intention is to facilitate innovation for the public good or to add further hurdles to the process of disseminating and utilizing research carried out in public institutions. Many institutions already have in place programs that facilitate entrepreneurship and have internal IP management cells. The new Act might only appear to be incrementally innovative. Some government departments engaged in funding science seem to accept the fact that IPRs rest with the institution where the research is carried out. Curiously, all funding arms of government do not seem to subscribe to this approach.

Will legislation improve the climate for innovation and original research in our institutions? Is financial incentive the key driver of creativity? In thinking about the new IPR Act, I was drawn to a recent study enticingly titled: 'The Decline of University Patenting and the End of the Bayh-Dole Effect' (Leydesdorff, L. and Meyer, M., *Scientometrics*, 2010, in press). The authors note that while the Bayh-Dole Act did have a dramatic impact on patenting by US universities between 1981 and the late 1990s, the effect of legislation 'has withered away, with a relative decline of university patenting since 2000'. They draw attention to an earlier study which suggests 'that Australian universities emulate . . . the Bayh-Dole success without legislation'. These authors make the interesting suggestion 'that institutional incentives for university patenting have disappeared with the new regime of university ranking. Patents and spin-offs are not counted in university rankings'. Patents cost money to file and maintain, in a global arena. Licensing fees and royalty income must substantially exceed patent expenditure; a condition not often achieved in Indian organizations as yet. The new legislation will undoubtedly bring in its wake new problems that will need to be addressed, especially if the size of patent portfolios becomes an index of apparent institutional achievement. I must confess that I found the new Act hard to read, undoubtedly a reflection of my innocence of the niceties of law. I could not help recalling the unforgettable episode in *Oliver Twist*, almost at the end of Dickens' long novel, when the truth of Oliver's origins is revealed. In discovering the fate of trinkets bequeathed to Oliver by his long dead mother the good Mr Brownlow establishes that Mrs Bumble had sold them to a pawnbroker. He addresses her husband, Mr Bumble as the 'more guilty of the two, in the eyes of the law; for the law supposes that your wife acts under your direction'. Mr Bumble's response has achieved immortality. ' "If the law supposes that", said Mr Bumble squeezing his hat emphatically in both hands, "the law is a ass – a idiot. If that's the eye of the law, the law is a bachelor; and the worst I wish the law is, that his eye may be opened by experience – by experience." ' (Charles Dickens, *Oliver Twist*, 1837–1839, Penguin Popular Classics, 1994, p. 491). There is no doubt that new laws will also be tempered by experience.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Universities and Academic Institutions: Competition and Collaboration

Universities and research institutions worldwide, face intense pressure to move up the 'rankings lists', that are now produced annually by different bodies. The 'Shanghai rankings', first published a few years ago, have become as familiar to university administrators as the '*h*-index' and journal 'impact factors' are to scientists and publishers. In a competitive world it is inevitable that 'merit lists' of individuals, journals and institutions are becoming popular and widely used. Quantitative analysis of academic performance using the methodologies of scientometrics has come to stay, despite the fact that many of those who use the various indices seem to have little or no knowledge of their limitations. The space for personal judgements is shrinking, rapidly yielding ground to the growing acceptance of scientometric indices. For those who hold high administrative or political position the availability of ready available 'rankings' is a blessing. They can now pass judgements with little need to study their subjects. It has become an annual practice in India to bemoan the fact that Indian academic institutions do not seem to do well in the global rankings; with only a couple of entrants in lists which represent the 'top 500' in the world. In almost the same breath the very same administrators announce the need to create many new 'world class' institutions, rarely stopping to ask if they are clear about the strategies to achieve this undoubtedly desirable goal. University administrators, especially from the developed countries in Europe, America, Asia and Australia seem acutely aware of the need for their institutions to project a highly visible international profile. The dramatic rise of research productivity in China, as measured by the number and quality of scientific publications and investment in science, has attracted a great deal of attention in recent years. Many commentators seem to sense an imminent eastward shift in the balance of influence on the world scientific scene. In India, apart from the oft-heard laments, when the annual rankings appear, there is little by way of purposeful action, either at institutional or national level, to increase the visibility of our institutions by enhancing both quality and quantity of research output. There is little hope for a sudden surge unless there is a concerted and cooperative effort to raise scientific productivity and research impact. Over the years many small research institutions have been created, which are restricted to very few areas. These laboratories are staffed by many competent, dedicated and highly talented scientists and are often well funded by local standards. These are, however, too small to be even noticed in a global survey. Their major advantage is that their restricted size enables a high level of 'per capita' funding and insulates them from the problems of larger institutions. There is little scope for growth, and even high quality research output may pass unnoticed in the rapidly expanding scientific world. Efficient 'social networking' with corresponding communities of scientists in the West has helped small institutions to remain visible; a strategy that may prove less effective as competitive pressures increase.

In the last few years there has been a sudden increase in the number of foreign universities that come to India in search of collaborative arrangements. There appear to be two distinctly different motivations. Many foreign universities view arrangements with Indian institutions as an effective way of attracting both undergraduate and graduate students. Some autonomous institutions (deemed universities and others) often advertise these 'collaborative' arrangements as a device to attract students who will end up paying substantial fees. Foreign partners are often quite willing to permit usage of their 'brand names' if the price is right. At an entirely different level, foreign delegations travel around the country seeking to establish research collaborations which may permit exchange of researchers, both faculty and students. In many cases, these arrangements permit access to sources of funding that have been specifically earmarked for bilateral cooperation. The favourite instrument of collaboration is the 'memorandum of understanding' (MOU), usually signed amidst some fanfare, which provides a good photo-opportunity for functionaries and administrators. Most MOUs signed between institutions lie forgotten and unimplemented; there are relatively few examples of major successful collaborations between Indian and foreign institutions. While there has been a reasonable degree of collaborative research involving individuals or small groups of scientists, flourishing inter-institutional arrangements are the exception rather than the rule. An

interesting feature of the many foreign delegations that I have met is their clear sense of purpose and organization. Collaboration, both within countries as well as overseas, seems to be an excellent strategy to raise the level of research output, exploit complementary strengths and gain by cross-cultural practices. Most delegations seem to have members who focus on strategic issues, important for institutional progress. They also appear to have studied the terrain rather well; often aware of the strengths and weaknesses of our best institutions. Indian institutions however appear to be significantly dominated by individual interests, often unable to crystallize a coherent view of approaching collaborative arrangements. It is clear that our institutions will benefit by increasing the number of foreign academics, faculty and students, who work on our campuses. They will add in a significant way to the ambience of our institutions. Indeed the remarkable success of US and British universities over the last century has been in large part due to their ability to attract the best of students and faculty from all over the world. While this is a situation that is unlikely to happen in India in the near future, encouraging international collaboration may be a step in the right direction in energizing our old institutions, and accelerating the growth of new ones.

Collaboration is best catalysed by the availability of resources. There is a significant need to increase the magnitude of internal resources available at institutions to promote international collaboration. The grants system administered centrally is not flexible or nimble enough to respond to rapidly changing needs. At present there is a pronounced asymmetry in the interactions possible between foreign and most Indian institutions. This could, in principle, be addressed by earmarked institutional support for enhancing collaborative research. If the right partners are chosen, after careful assessment of complementary strengths, research outcomes will be significantly enhanced. How does one assess strengths and weaknesses of the scientific enterprise in institutions and countries. We must, of course, turn inevitably to the statistics of performance. The report entitled, *Science and Engineering Indicators 2010* produced by the National Science Board in the United States is an example of the kind of comprehensive study that is needed to guide policy. The biennial exercise carried out by the US National Science Foundation results in an extraordinary volume of data which helps analyse 'key aspects of the scope, quality and vitality of the Nation's science and engineering enterprise in the context of global science and technology'. I have extracted the quote from the memorandum signed by the Chairman to the US President and Congress. It is a clear statement of the intent and purpose of this elaborate study. The report begins with an overview that examines global trends in science and engineering, focusing on national spending, research output and outcomes, clearly providing a platform for basing public policy in an intensely competitive world. The seven chapters provide a detailed analysis of elementary and secondary mathematics and science education in the US, survey higher education, research in the academic and industrial sectors, examine the science and engineering labour force and discuss public attitudes and understanding of science. Almost every useful indicator of the health of science is employed in the analysis. The first page of the introductory overview notes that 'Asia's rapid ascent as a major world S&T center – beyond Japan – is driven by developments in China and several other Asian economies (Asia 9 – India, Indonesia, Malaysia, Philippines, Singapore, South Korea, Taiwan and Thailand)'. The report suggests, somewhat alarmingly for Indian readers, that 'India's high gross domestic product (GDP) growth contrasts with a fledgling overall S&T performance'. The contributions of foreign students to academic research in the US are emphasized by the fact that they 'have earned more than half of US natural science and engineering doctorates since 2006. Half of these students are from East Asia, mostly from China (31%), India (14%) and South Korea (7%)'. If engineering is considered separately, the numbers are even more skewed with foreign nationals accounting for 68% of Ph D recipients in 2007. The report notes, I suspect with a tinge of satisfaction, that 60% of foreign students who received doctorates in 1997 'were gainfully employed in the United States in 2007 – the highest ten year stay rate ever observed'. There is much to mull over in the statistics so well presented in this report, especially for those who worry about 'brain drain and brain gain'. The report addresses the issue with which I began this column, foreign collaboration. The data presented suggests that 'the United States rate of international collaboration is similar to that of Japan and China but lower than that of the European Union (EU), where explicit EU policies coupled with incentives stimulate international, and specifically intra-EU collaboration'. In the competitive world of research, the need to promote collaborative effort must be recognized. In India there is a great need to encourage and facilitate collaboration, within institutions and between institutions in the country and elsewhere, if the goal of raising standards and enhancing research productivity is to be realized.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Climate Science: Drowned in the Noise

Climate change is a term that almost every educated person uses today. Hot days, as summer approaches, are quickly interpreted as tangible signs of global warming. Sudden, unanticipated events like torrential downpours and floods seem to immediately conjure up visions of a rapid change of the global climate cycle. Much of the discussion on climate change centres on policy issues. The recent summit in Copenhagen produced little by way of agreement on the commitments that need to be made by nations to mitigate 'global warming'. There will undoubtedly be more international meetings on the issue in the not too distant future. The public perception of the importance of climate change was suddenly enhanced a couple of years ago by the award of the Nobel Peace Prize in 2007 to former US Vice-President Al Gore and the Intergovernmental Panel on Climate Change (IPCC), a large body of scientists (close to 4000), who have over the years painstakingly prepared reports, that provide the evidence for climate change scenarios. The panel came into being in 1988 as a result of a UN General Assembly resolution, triggered by concerns about the rising levels of greenhouse gases, primarily CO_2 which could produce a warming effect. In seminar after seminar over the years the famous graph correlating global temperature with atmospheric CO_2 levels has been projected, establishing the connection between recent human activity and changing atmospheric composition. In the past few weeks controversy has dogged the IPCC report following the revelation that some alarmist scenarios, most notably the section on Himalayan glaciers, may have been based on somewhat flimsy grounds. The leakage of e-mails exchanged between scientists on issues of climate data has brought discussions about climate science, climate scientists and policy makers to the front pages of daily newspapers.

An interesting feature of a great deal of reporting on climate change issues is the almost complete absence of any discussion on the historical origins of climate science. 2011 will mark the 150th anniversary of the publication of John Tyndall's paper circumspectly titled: 'On the absorption and radiation of heat by gases and vapours, and on the physical connection of radiation, absorption and conduction' (*Philos. Mag.*, 1861, **22**, 169). This anniversary will undoubtedly be less celebrated than the recently concluded 'Darwin year' but is nevertheless an event that should refocus attention on the science of climate. The role of the atmosphere in maintaining global temperature was recognized in the early 1820s by Joseph Fourier. Nearly, four decades after Tyndall, Svante Arrhenius – immortalized in physical chemistry textbooks for his work on chemical kinetics – recognized that CO_2 emitted by human activity may warm the atmosphere. His paper entitled 'On the influence of carbonic acid in the air upon the temperature of the ground' (*Philos. Mag.*, 1896, **41**, 237) marks the formal starting point of the CO_2 theory of climate change. It would have been appropriate to mark the centenary of Arrhenius' initial attempts to connect climate and chemistry in 1996. For purists the Arrhenius paper is flawed; for the outside observer it may be prophetic. Years later, G. S. Callender returned to the issue with a paper succinctly titled 'The artificial production of carbon dioxide and its influence on temperature' (*Quart. J. R. Meteorol. Soc.*, 1938, **64**, 223). An interesting feature of the Tyndall, Arrhenius and Callender papers is the simplicity and clarity of the titles. There is no doubt that all three authors foresaw that global climate would be influenced by atmospheric composition. The CO_2 effect had few adherents even in the 1950s. Atmospheric science was a backwater and an unrewarding area of work. My curiosity about the history of the area was aroused when I read a 1956 article in *American Scientist* which was reprinted in the January–February 2010 issue of the journal. The article entitled 'Carbon dioxide and the climate' notes that 'it seems appropriate to reconsider the variations in the amount of carbon dioxide in the atmosphere and whether it can satisfactorily account for many of the world-wide climatic changes' (Plass, G. N., *Am. Sci.*, 1956).

Gilbert Plass (1920–2004) was a physicist who published extensively in the 1950s on atmospheric infrared radiation. His seminal paper entitled 'Effect of carbon dioxide variations on climate' noted that 'physicists are somewhat partial to the carbon dioxide theory since all the relevant experimental and theoretical work that is needed in order to determine the magnitude of the effect is in their field'. Plass was emphatic in his conclusion: '. . . we can be sure that the atmospheric carbon dioxide amount has changed many times and by large factors during the geologic history of the earth and that these variations have had their influence on the climate'. Plass was clear that in order to relate CO_2 changes to climate variation 'the

carbon dioxide content of past epochs should be known'. He was prescient in noting that 'in order to determine these variations we must collect evidence from diverse fields as geology, oceanography, biology and meteorology' (Plass, G. N., *Am. J. Phys.*, 1956, **24**, 376). Plass based his work on the sound knowledge of the infrared absorption and emission properties of the gaseous constituents of the atmosphere. In a paper entitled 'Infrared radiation in the atmosphere', Plass noted: 'From the surface of the earth up to heights of 80 km or more the temperature is usually in the range from 200 K to 320 K. The blackbody radiation that corresponds to these temperatures is largely in the spectral region from 5 to 100 microns. In order to influence the infrared flux in the atmosphere, a gas must have an absorption band in this range of frequencies'. He then added that the common constituents of air, oxygen and nitrogen do not absorb these frequencies but ozone and carbon dioxide do, clearly identifying the latter as the principal actor in determining infrared flux and consequently global temperature. Although in Plass' time the problem of global warming had not heated up, he observed with remarkable foresight: 'The accumulation of carbon dioxide in the atmosphere from continually expanding industrial activity may become a real problem in several generations. If at the end of the century measurements show that the carbon dioxide content of the atmosphere has risen appreciably and at the same time the temperature has continued to rise throughout the world, it will be firmly established that carbon dioxide is an important factor in causing climate change' (Plass, G. N., *Am. J. Phys.*, 1956, **24**, 376–387). Plass' initial calculations were oversimplified; curiously some of his numbers coincide with those obtained with more realistic models. In his 2010 commentary that accompanies the reprinting of the Plass article in *American Scientist*, Gavin Schmidt points out that more accurate calculations carried out subsequently have 'been all but forgotten' since the author 'incorrectly concluded that CO_2 could not play a role in climate change'. Schmidt adds: 'In 50 years time if someone reviews my work, I would hope to have been as lucky as Gilbert Plass.'

The *American Scientist* issue drew me, inevitably to the internet and there I discovered a treasure; a website 'The discovery of global warming' by Spencer Weart (http:www.aip.org/history/climate, June 2007). The author, a historian of science has created a wonderful collection of essays, an ocean of information for those interested in climate change. He provides a fascinating insight into the 'scientific process, as seen in climate studies'. Weart observes that groups of scientists who work in an area do not 'look like an exploring team aiming into new territory'. Rather, 'it looks more like a crowd of people scurrying about, some huddling together to exchange notes, others straining to hear a distant voice or shouting criticism across the hubbub. Everyone is moving in different directions, and it takes a while to see the overall trend. I believe this is the way things commonly proceed not only in geophysics but in most fields of science'. Weart adds that he attempts to capture this process in climate science in his essays, 'by connecting the dots among roughly a thousand of the most important papers in the science of climate change'. This is clearly a formidable task in a rapidly expanding field; one which Weart accomplishes in compelling fashion. Two of climate sciences major figures emerge in these pages, Roger Revelle (1909–1991) and Charles Keeling (1928–2005). In a 1957 paper that he coauthored with Hans Suess, Revelle argued that the rates of absorption of CO_2 by oceans would be far slower than its production by anthropogenic activity, laying the foundation for much of modern discussion on ocean geochemistry. In retrospect, the bicarbonate chemistry that Revelle introduced to the field of climate science seems both obvious and simple; a clear sign of its importance. Revelle's leadership abilities propelled him to the position of the most articulate spokesman for the field. Charles Keeling, whom I have written about on an earlier occasion (*Curr. Sci.*, 2008, **95**, 291), began his painstaking measurements of atmospheric CO_2 in 1957. Weart's essay 'Money for Keeling: Monitoring CO_2 levels' must be read by all those who discuss ways of funding science. Keeling's persistence in the face of funding difficulties is a lesson in the virtues of committment, dedication and resilience. Weart notes that 'some officials would have preferred to pursue CO_2 monitoring without Keeling himself'. 'Keeling's a peculiar guy', Revelle later remarked. 'He wants to measure CO_2 in his belly. . . And he wants to measure it with the greatest precision and the greatest accuracy he can'. It is these measurements of Keeling, together with the work of Plass and Revelle that provide the foundations of present day climate science. The road to Copenhagen was laid a long time ago and the builders have faded into history. A call to Stockholm when the IPCC was being set up, when all three were still alive, may have attracted many more to the area of the science of climate, rather than the politics of climate change.

Is climate science in danger of being drowned by the noise of climate change controversies? James Lovelock in his latest book, *The Vanishing Face of Gaia: A Final Warning* (Allen Lane, 2009; Penguin, 2010) provides a sombre assessment: 'We seem to have forgotten that science is not wholly based on theory and models: more tiresome and prosaic confirmation by experiment and observation plays just as important a part. Perhaps for social reasons, science has in recent years changed its way of working. Observation in the real world and small-scale experiments on the Earth now take second place to expensive and ever-expanding theoretical models. . . . Our tank is near empty of data and we are running on theoretical vapor'. Climate science must carve an identity distinct from climate change policy issues. Only then will the science of the Earth be heard above the cacophony of controversy.

P. Balaram

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EDITORIAL

Higher Education: Rocky Road to Reform

The reform agenda set by the Ministry of Human Resource Development (MHRD) is a clear sign that the time is ripe for considering a major restructuring of the education system in India. Higher education (college and university education) has long been viewed as an area where major interventions by Parliament and Government may be necessary. The process of reform has been speeded up by the extraordinary haste with dozens of new institutions have been created, the controversies over corruption in the accreditation processes, the growing pressure for creation of private institutions of higher learning, the challenges posed by the impending entry of foreign institutions and the growing realization that the best of India's institutions must compete with the best in the world. Reform is being contemplated within a framework that abounds with constraints. The demands for equity must be balanced by the imperatives of excellence. The rules of the game for public, private and foreign institutions may have to be different, but the playing field must be largely even, if public institutions are to stem the tide of decay. The strongly federal nature of our governing structures dictates that Centre–State relationships must be balanced and cooperative. Both state governments and institutions view excessive ministerial interest with suspicion; reform measures can often be mistaken for attempts to interfere with local autonomy. The heavy, and often clumsy, hand of bureaucracy can ensure that even the best of intentions are misinterpreted by institutions which jealously guard autonomy. Over the last couple of years the National Knowledge Commission (NKC) and the MHRD, using the Yashpal Committee, have produced reports that spell out the contours of a reform agenda. Regulation of the education sector has become a critical area with the rapid growth of private institutions. Higher education is now a commodity to be sold and the pressures of the marketplace build up rapidly. The manner in which institutions are accredited and permitted to award degrees has been the subject of much recent discussion. Inevitably regulatory authorities, which have served admirably for many years after their inception, crumble under modern day pressures. Corrupting existing institutions and hastening their decay has unfortunately been a characteristic of India's turbulent, and at times dramatic, growth over the last three decades. The NKC and Yashpal Committee reports are an attempt to address the issues raised by the crumbling foundations of the higher education enterprise in India, even as the sector is poised for unprecedented growth. These reviews have resulted in the drafting of legislation that intends to provide the foundations for reform and restructuring of higher education in India.

The National Commission for Higher Education and Research Bill 2010 (NCHERB) is being introduced as 'an Act to provide for the determination, coordination, maintenance of standards in, and promotion of, higher education and research, including university education, technical and professional education other than agricultural [and medical] education'. The parentheses that envelop 'medical education' are present in the draft copy downloaded from the MHRD's website. While agriculture is clearly excluded, there remains a hint of ambiguity about medicine. Curiously enough, the revolution in biology over the last half a century has had its most profound impact on agriculture and medicine. Genomics and all that has followed from the technical advances that have taken place over the last three decades have reaffirmed what was intuitively appreciated in the 19th century; the unity of organisms, plants, animals, microbes and man, despite enormous biological diversity. The bill, as drafted, places agriculture [and medicine] on a different pedestal; undoubtedly a recognition of political realities and the zealousness with which ministerial turf is guarded in Delhi, rather than the more esoteric grounds of academic unity. In an Act on 'education' the words 'technical and professional' in the opening paragraph strike a jarring note. Is science non-technical and unprofessional? Is the study of archaeology, history or literature also similarly labelled by implication? Is economics to be consigned to the backwaters of higher education by using the words 'technical and professional' to describe engineering, pharmaceutical science (clearly a misunderstood discipline in India in modern times) and possibly other subjects like veterinary science? It is time that the word 'education' is used and understood in its broadest sense. The distinctions in educating students (and teachers) in different disciplines need to be blurred and indeed removed. In a sense the draft Act seems to suggest that this may happen when it states that its purposes will be

achieved by establishing the 'National Commission for Higher Education and Research (NCHER)'. Three broad areas may be excluded from the ambit of the proposed Commission; agriculture, medicine and law. This is a clear tribute to the power and influence of ministries and councils which control these areas. The curricula and requirements for degrees in these disciplines will lie outside the scope of the Commission to examine. Institutions in these areas will continue to operate as they have always done. Undoubtedly, government appears to have come to the conclusion that all is well with institutions in these areas and it is only science, engineering, humanities and social sciences which require the guiding leash of new legislation to walk along the road to reform.

The Act aims 'to further promote the autonomy of higher educational institutions for the free pursuit of knowledge and innovation, and for facilitating access, inclusion and opportunities to all, and providing for comprehensive and holistic growth of higher education and research in a competitive global environment through reform and renovation; and to provide for an advisory mechanism of eminent peers in academia'. Opening paragraphs of the Act unveil two new bodies; the National Commission and the body of 'eminent peers'. It is in Chapter III that the 'collegium' is unveiled as a body 'consisting of core Fellows and co-opted Fellows, being persons of eminence and integrity in academia in higher education and research'. The collegium will advise and 'recommend to the Commission a vision on the emerging trends in different fields of knowledge'. It will draw up panels of three persons for each position in the National Commission. It will 'recommend for inclusion in the National Registry' names of 'persons eligible and qualified to be appointed as Vice-Chancellor of a university or the head of an institution of national importance'. In the last few years the procedures for appointing institutional heads has degenerated to a level where any person of real or perceived 'eminence' is unlikely to be comfortable advancing a candidature. The 'national registry' seems both undignified and impractical. Surely, well intentioned 'search committees' carefully appointed by the National Commission will serve the purpose. The collegium is an interesting body. Core Fellows are restricted to those who are National Professors, recipients of the Nobel Prize or Fields Medal or Jnanpith award or members of 'an Academy of international standing'. The 'core' then coopts other Fellows. The premise that recipients of high international honours will commit themselves to the cause of reforming and uplifting Indian higher education is debatable. Any high level advisory or executive body must be filled with persons of integrity and commitment to the specific cause, backed by significant professional attainment. Government cannot disown the job of making judgements. The statement of intent in forming a collegium composed of a 'core', restricted by these parameters, reflects a sense of insecurity and a lack of confidence in the vast, chaotic but nevertheless resilient Indian academic system. The collegium as envisaged will consist of two groups; the high priests as the 'core' and the less accomplished as 'coopted' members. It is this body which will recommend the panel of names for the selection of the Chairman and Members of the NCHER. The selections will be made by a committee chaired by the Prime Minister and consisting of the Speaker and Leader of the Opposition in the Lok Sabha, the Minister for HRD and the Health Minister. Once constituted, the Commission will subsume the UGC, the AICTE and the National Council of Teacher Education. Interestingly, there are specific exclusions which specify that nothing in the Act 'shall be construed as restricting the power of the Bar Council of India to specify standards [of higher education] concerning practice in courts'. A similar exclusion is extended to the Council of Architecture. There is no reference in this section to the Medical Council of India.

The Act is intended to reform the regulatory process and improve the governance of institutions by influencing the manner by which institutional heads are chosen. The process of curricular reform and the raising of teaching and research standards across the vast network of institutions will require more than legislation and resources. It will need a broad movement that stirs the faculty in our institutions to collectively appreciate the need for change. The IITs, which we prize today, were the subject of a review in the mid 1980s. This was a troubled time; the 1970s were difficult years which affected many institutions. In a perceptive and thoughtful analysis, Rohit Manchanda notes that P. K. Kelkar, the founder of two IITs at Mumbai and Kanpur, felt that 'senior faculty had allowed themselves to slide into a trough of lassitude'. He quotes a letter to the IIT Review Committee in which Kelkar argued that most senior members of the faculty 'have a tired outlook and have very little enthusiasm for change or new ideas or an inner drive for achievement'. Manchanda notes: 'Perhaps more damningly, he believed them to have been estranged from their very métier, the call of the intellect' (*Curr. Sci.*, 2010, **98**, 570). Many of our public institutions today, once vibrant universities amongst them, may well be described in the words that Kelkar used quarter of a century ago. A reform agenda will require a groundswell of support from faculty, students, staff and the public. Laws and their implementation by the bureaucracy of government and interpretations by the judiciary can sometimes derail even the best intentions for reform. Undoubtedly, the new Act will be refined by constructive debate and discussion. When passed, the new legislation will launch the sector of higher education on the rocky and difficult road to reform.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Losing a Friend

'But, for the unquiet heart and brain,
A use in measured language lies;
The sad mechanic exercise,
Like dull narcotics, numbing pain.

In words, like weeds, I'll wrap me o'er,
Like coarsest clothes against the cold:
But that large grief which these enfold
Is given in outline and no more.'

In Memoriam
—Alfred Lord Tennyson (1849)

Editorial advisory board meetings, like those of most advisory bodies, are full of suggestions that need to be implemented by someone else. The regular editorial column in *Current Science* has its genesis in a meeting held in 1998. Challenged with the task of providing a fortnightly column, I began my career as a columnist in the middle of 1998. Between 1998 and 2005 when I retreated from writing, diverted by administrative responsibility, I produced 165 columns. The hardest part of writing is, of course, to find a topic of some interest and to avoid the trap of becoming repetitive. Those who do not write, but are critical readers, often underestimate the difficulty of composing even moderately readable essays. In the first couple of years, age and enthusiasm were still on my side. This was the period before Internet access became so facile and *Google* was yet to become an indispensable weapon in the search for information. Choosing a topic meant that one had to listen carefully to conversations in the coffee houses and read indiscriminately in the library. The task of collecting reference material was physically demanding, requiring a degree of energy which was becoming increasingly hard to generate. Towards the end of 1999, providence smiled and a young man, Riki Krishnan, arrived in my laboratory looking for something to do. He had acquired a B Sc degree in environmental sciences, a few years earlier, and had spent some time working on the ecology of bats. In advancing his claims for a position, he presented a letter entitled 'Monitoring India's lifescape', which bemoaned the absence of bats on a *Current Science* cover, and a book review entitled 'Bats of the Indian subcontinent' (*Curr. Sci.*, 1997, **72**, 229; 1999, **77**, 188). In my first conversations with him, I was struck by his intense interest in bats, organisms about which I know nothing. This passion for a subject that seemed so esoteric, in one so young and unschooled in research, was striking. In the conversation that followed, I realized he was indeed looking for a position that would develop into a stable career option. For students who choose to avoid conventional paths, careers can sometimes be hard to forge, in an environment where entrance exams of increasing difficulty stand as obstacles in the way of academic advancement. Two qualities stood out in my earliest interactions with Riki Krishnan. He was enormously enthusiastic and friendly and honest to a fault. He completely lacked the ability to promote himself, a trait which must be prized and, as I was to discover later, he was helpful to a degree that is uncommon. He joined this journal as a temporary hand towards the end of 1999. In the decade that followed he was, in many ways, my most important collaborator in writing these columns. When he died on March 19, a day before I was to begin my fortnightly exercise, the staff of this journal lost a valuable colleague and I one of my closest associates and friends.

Riki Krishnan (1971–2010)

Riki Krishnan was indefatigable in his search for topics that might be worth discussing in print. As the years progressed, the Internet became an inexhaustible source of information and he would send me almost every day, unfailingly, envelopes containing hard copies of materials

he felt I ought to read. The range of topics and materials he collected were remarkable in their scope and diversity. His youthful energy (and a powerful motorcycle) provided him with the ability to hunt for books in libraries and bookshops across the city. Even as I write, I am surrounded by envelopes, many containing material as yet unread; his success in collecting background material far exceeded my limited capacity to read and absorb. There have been many times over the years when the task of meeting printing deadlines and word limits has seemed an unnecessary burden to bear. Riki's childlike enthusiasm for the task of producing this column proved energizing; transforming a tedious chore into a worthwhile activity. In July 2005, when I laid down the pen, Riki posed a question: 'What shall we do now?'. The 75th year of *Current Science* was approaching in 2007 and the Centenary Year of the Indian Institute of Science (IISc) was to be kicked off in mid-2008. Somewhat casually I suggested to Riki that we begin to collect historical material on the journal and the Institute which might eventually form the basis of articles that could be published. Little did I realize then that Riki's boundless enthusiasm, coupled with the advantages of my administrative position, would launch us on the road to setting up an institutional archive. His disarming nature quickly served to nucleate a group of colleagues who have enthusiastically carried this task forward. He was untiring in his efforts to search for archival material. He located the son of the first editor of *Current Science*, C. R. Narayana Rao, discovering materials and facts about the early years of the journal which have been described ('*Current Science*: Some Early History', Krishnan, R. and Balaram, P., *Curr. Sci.*, 2007, **92**, 129). In his unceasing efforts at unearthing historically interesting facts about the journal and the institution, Riki chanced upon the articles by Kim Sebaly on the Tatas and Higher Education in India. This led quickly to a sudden surge of interest in institutional origins. His enthusiasm drove me to find Kim Sebaly on the Internet and eventually have him visit the Institute in the centenary year. In 2007, I began writing again and Riki renewed his efforts to ensure that columns appeared with unfailing regularity. On the eve of IISc's Centenary Conference in December 2008, Riki was diagnosed as suffering from cancer. He continued to work, as always, with very brief breaks to undergo radiation and endless rounds of chemotherapy. During this ordeal of 15 months his spirits never flagged. His energies continued to be directed towards the task of throwing up topics for me to read, working at a pace which scarcely betrayed his worsening condition. He would call me everyday, unfailingly in the night, to ask 'Anything to collect, sir?'. This is a question which will remain with me, always. It is with Riki that I explored the early history of IISc. Ignorant as we were, there was a genuine sense of joy and achievement when we learnt about the genesis of the institution (*Curr. Sci.*, 2008, **94**, 5) and discovered Burjorji Padshah (*ibid*, 2008, **95**, 1651) and Morris Travers (*ibid*, 2008, **94**, 1109). He urged me to write a longer article reflecting on the Institute as it turned a century (*ibid*, 2009, **96**, 1404).

In 2005, when I decided to stop writing, the last column reflected on my experiences: 'This column has also, albeit infrequently, been used to mark the passing of men who greatly influenced the environment, where I have worked all these years. . . . I have been sharply reminded of their committment, dedication and influence. Reflecting on their lives, I have shed a tear, in the solitude of the night, sometimes from a sense of personal loss, at others from the realization that even the most admirable men are only mortal.' Riki once asked me, with the innocence that was truly his: 'Did you really cry, sir?'. I have never shed tears more copiously than when I write this. How does one say goodbye to one so young? I can only turn to the poets.

'I hold it true, whate'er befall;
I feel it, when I sorrow most;
'Tis better to have loved and lost
Than never to have loved at all.'

—Tennyson

Riki was not celebrated or famous. *The Epitaph* in Gray's Elegy comes back to haunt me:

'Large was his bounty, and his soul sincere;
Heaven did a recompense as largely send:
He gave to Misery all he had, a tear,
He gain'd from Heaven, 'twas all he wish'd, a friend.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Libraries, Journals and Publishers

The library of the Indian Institute of Science (IISc) was the centrepiece of the institution, when I began to frequent it in the mid-1970s. The shelves were full of treasures; it was a time when books seemed out of reach of academic salaries. The weekly arrival of *Current Contents*, much sought after by those interested in the contents pages of journals that were unavailable or issues that would appear by surface mail months after publication, was always eagerly anticipated. There was a special pleasure in 'feeling' newly arrived journals. A sense of tranquility prevailed in the large building, attracting readers to spend a lazy afternoon, browsing. The extensive collection of back volumes, including journals printed in the 19th century, provided a resource that few researchers, old or young, could afford to ignore. The journal collections grew in size and cost throughout the last two decades of the 20th century. This was the period when private publishers became the principal drivers of an explosion in the number of science journals, whose appearance and quality seemed to place traditional journals at a disadvantage. Staid professional societies began to change their style, often compelled to start new, specialized journals to cope with competition. A perceptible transformation was evident in the new century. The digital age had arrived and journals were suddenly 'electronic'. As the campus network expanded, journal access no longer required the pleasant and leisurely walk to the library, often accompanied by diversions to the campus coffee house. Journals which sometimes arrived months after the date of publication were now instantly available on computer screens. A new generation of students (and faculty) now emerged, scarcely attracted by the 'feel' of a newly minted journal. The joy of savouring the illustration on the cover and the pleasure of turning the pages randomly, has been replaced by efficient and purposeful searches of collections of accepted articles in advance of print publication. 'Links' allow readers to instantly access cited references. The task of striding around the library, from stack to stack, is now a rapidly fading memory. The digitization of books and the rapid development of portable reading devices, with extraordinary storage capacities, may even pose a threat to the traditional task of librarians; the acquisition, cataloguing, lending and retrieval of books. Will the traditional library eventually become extinct? What will the 'librarians' of the future do?

An interesting phenomenon over the past decade in India is the metamorphosis of librarians into 'information scientists'. The name appears to signify a perception of a new role, as a handler of information in an organization. In the early days of the Internet many databases were not available online; local data centres needed to be created, maintained and updated. The dramatic upsurge in the use of technology in handling information requires skills which the traditional librarian hardly needed. Unfortunately, the scholarship associated with the librarians of yesteryear, their understanding of the reading preferences of their clientele and a genuine love for books and journals has seemingly evaporated. Digitization and electronic archiving has made many sections of our libraries redundant. Binderies, where one often had to go to retrieve a badly needed journal for quick reference, are almost extinct. The musty smell of old journals is hard to recall. Librarians in India seem to be reinventing themselves; donning the role of analysts of science, dabbling in the arena of scientometrics. This is not an easy, or even desirable transformation with many 'information scientists' often being ill equipped to deal with the new, ever changing and, invariably, controversial metrics of science.

Libraries (and librarians) today need to confront new and sometimes difficult issues associated with journal acquisition. The costs of science journals have been rising steeply over the last decade, even while the number of journals has proliferated. Journal pricing has been strongly influenced by the growth of monopolies in the publishing industry. Journals in the area of science, technology, engineering and medicine (STEM) are being taken over by a handful of commercial publishing houses. Even professional societies and academies have found it difficult to maintain independent production, dissemination and pricing policies. The transition to electronic publishing has provided commercial publishers an excellent opportunity to minimize production costs, maximize journal numbers in their stables and to promote 'bundling' strategies, where librarians purchase electronic access to large numbers of journals, bundled into a single package. The issues of 'perpetual access' and 'electronic backfiles' are important in considering library budgeting today. Two publishing houses, Elsevier and Springer-Verlag, together account for the most significant share of the library budget for science journals. In an effort to

meet the need for journal access and to combat the problem of rising costs, government agencies have put together consortia of public institutions, which are charged with the collective responsibility of negotiating with monopoly publishers, for electronic access to journals. The multiplicity of consortia and diversity of participating institutions in India has resulted in wide variations in pricing for individual libraries. The days when journals had fixed prices for library subscriptions may be receding into the past. Publishing cartels control the arena of science journals in a fashion that does not lend itself to fair and reasonable pricing policies. The boom in institutes of science and technology in India has resulted in an unexpected windfall for publishing cartels, as they set prices for 'bundles' which may rarely be used and whose cost will inexorably rise in later years. With institutions increasingly preferring electronic access and dispensing with print subscriptions, new libraries may inevitably acquire a new form. The days of the large, solemn reading rooms, which even seemed to frown on whispering, are clearly numbered.

Journal costs have rarely been discussed in India. The government has promoted measures to enhance access to science journals by centrally supporting consortia, which have been ill prepared to address the issue of overpricing. The unreasonableness of many publishers has, however, been a matter of concern in Western institutions for many years. Over twenty years ago, Philip Abelson noted that librarians in the United States had 'few allies' and 'were not well organized' to fight mounting journal costs in the early days of the rise of publishing monopolies. He added that at the time of writing 'librarians are now more effectively united and are finding allies among scientists and chancellors of universities. Perhaps most important they have identified major contributors to their financial problems – the international commercial publishers'. Abelson went on to point out that 'another factor influencing a revolt against monopolistic pricing is growing recognition that some high-priced journals have little impact' (*Science*, 1989, **244**, 1125). An early study of the 'cost effectiveness' of physics journals revealed that 'the group of journals published by the American Physical Society had on average a "cost effectiveness" more than 12 times that of the best group averages for any of several major publishing houses' (Barschall, H. H., *Phys. Today*, 1988, **41**, 56). Two decades later the publishing monopolies have grown, consolidated and have begun hunting for new markets in countries like India and China, where a rapid expansion of the research enterprise is underway. Library economics has also been the focus of more elaborate studies including issues of violation of American anti-trust laws. An analysis of 'journal pricing and mergers' suggests that 'the prospect of bundling and price discrimination, of course, will inevitably raise anti-trust issues (McCabe, M. J., *Am. Econ. Rev.*, 2002, **92**, 259). Interestingly, it is precisely the same issues of 'bundling and price discrimination' that might be of concern to those charged with making decisions on acquisitions for the various library consortia in India. It is clear that a more organized, collective approach by institutions is needed to ensure that unreasonable subscription rates are not paid to commercial publishers.

The authors in science journals are scientists, so are the readers. The processes of editing and peer review are also managed by academics, who are almost never paid for their efforts. This is community service and works very well in journals managed by societies and academies. Commercial publishers add value to journals by many marketing strategies including concerted efforts to raise impact factors by purposefully recruiting influential and visible scientists to their cause. In the world of electronic publishing many costs associated with traditional print journals have been replaced with other issues. Authors and journals no longer pay large postal costs for transporting manuscripts across the world. As print volumes decline, paper and printing costs will diminish. Online submissions, using formatted templates, reduce the routine work of editorial staff, permitting journals to cut salary costs. The large price differentials (and of course impact differences) between groups of commercial journals and those produced by bodies of scientists suggest that another hard look at monopoly pricing by large publishers is required, especially in a country like India where many new 'libraries' are likely to be created. There have been several instances where 'rebel journals' have emerged as an answer to excessive overpricing. A report in *Nature* (Declan Butler, www.nature.com/news/2007/070122) notes that most rebel journals produced with community support quickly achieve 'scholarly success' but 'often get poor support from libraries'. Enhancing subscription levels requires a marketing muscle which is hard to achieve for small enterprises. Unsurprisingly, many large and successful scientific societies, with substantial portfolios of journals, have now begun to acquire some of the characteristics of commercial publishers and at times have even entered into collaborative agreements in order to enhance the reach of their journals. In composing this column in my mind, I asked a colleague how many journals did he read on a regular basis. His answer was revealing. He thought about the question for a while and then ruefully remarked: 'Nowadays, I only seem to read papers online and almost never browse through the journal'. The march of the Internet, online collections and public databases may alter the face of publishing. The scientific journal as a carefully chosen aggregate of papers, appearing with a predictable periodicity, may give way to a continuously expanding electronic archive. When that happens, the libraries and journals of the past will only be memories. Commercial publishers will, however, sense new opportunities and challenges.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Human Genome: Dividends After a Decade

Ten years ago, when Bill Clinton, then the US President and Tony Blair, then the British Prime Minister jointly announced the completion of the first rough draft of the human genome, it seemed that the new century's first decade would be dominated by the blinding pace of advance of medicine. Genomes promised much. The human genome appeared to hold the key to a successful attack on cancer, diabetes, cardiac and neurodegenerative disease. The smaller genomes of bacterial and viral pathogens promised to reveal selective protein targets which would open the door to a new generation of therapeutics to combat infectious disease. There was a great sense of impending success on many fronts as the revolutions in biology and information technology suddenly seemed to converge. The determination of the sequence of the approximately three billion letters (bases) in the interminably long human genome was the high point of the development of DNA sequencing technology and the computational tools to assemble and interpret the vast volume of data. The human genome project (HGP) was biology's first megaproject which involved large scale international collaboration and required managerial skills that transcended the individual laboratory. From conception in the mid-1980s to completion at the turn of the century, the project took far less time than its detractors (and indeed, even its supporters) envisaged. The HGP heralded the era of consortia in biology, spawning the 'omics' proliferation. Biologists today speak confidently, and at times innocently, of proteomics, metabolomics, lipidomics and metagenomics. There is an air of confidence with which the complexities of cellular chemistry are viewed by the modern day biologist. The conquest of the human genome sequence suggests that technical solutions to almost any conceivable problem of chemical analysis in biology can indeed be found. The pace of advance of analytical methodology has been breathtaking; the area of gene sequencing providing the most visible example.

What has the human genome sequencing project achieved? In the intense debate that marked the beginning of the project, opponents argued – often eloquently and forcefully – that investments in mega projects in biology would significantly affect funding and support for the traditional and highly successful approach of promoting 'small science'. The criticism that large scale sequencing programs would only generate large volumes of uninterpretable data was advanced by the proponents of 'hypothesis driven research'. The key issue was always one of the sharing of budgets allocated for scientific research. A large fraction of influential biologists in the United States and Britain were ambivalent about the virtues of investing in the HGP. What then is the verdict a decade after the dramatic public announcement of the draft of the human genome? Francis Collins who led the publicly funded consortium that sequenced the genome, along with his colleagues, summarized expectations in 2003: 'If research support continues at vigorous levels we imagine that genome science will soon begin revealing the mysteries of hereditary factors in heart disease, cancer, diabetes, schizophrenia and a host of other conditions. Genomics holds the promise of "individualized medicine", tailoring prescribing practices and management to each person's genetic profile. These revelations should also lead us to develop and target drugs in a rational fashion to genes, protein pathways and networks shown to be involved in primary disease pathogenesis. Furthermore, a better understanding of the genetic factors that influence susceptibility and/or response to various infectious diseases could have an enormous impact on health in the developing world. Genomics also has the potential to help agricultural researchers develop better crops and livestock, environmental scientists create better methods of cleaning up toxic material, production experts streamline industrial processes and energy researchers work toward sustainable, non-polluting energy sources' (Collins, F. S. *et al.*, *Science*, 2003, **300**, 286). This 'grand vision' seemed to project genomics, with the human genome success as its centrepiece, as the panacea for most problems that seemed to await a scientific solution. At this highpoint of genomics anything appeared possible and Collins *et al.* noted eloquently: 'If the past 50 years of biology is any indication of the future, the best is certainly yet to come'. In assessing progress in the 'post-genome decade', the 1 April 2010 issue of *Nature* presents an extremely interesting overview of the field, with personalized accounts by key participants in the human genome project. In asking if 'human health truly benefited from the sequencing of the human genome', *Nature* notes in an editorial that a 'startlingly honest response' is obtained from 'the leaders of the public and private efforts, Francis Collins and Craig Venter', both of whom

say 'not much'. Clearly, post-genome progress has become mired in the complexities of biology. The exuberant optimism of the early years of the decade has given way to a more realistic and tempered assessment of future prospects. As the *Nature* editorial points out, biotechnology companies 'have had to rethink their optimistic assumption that selling human genetic information could turn a profit. Excitement over start-up companies offering personal genetic testing has withered just as fast, as it has become clear that their predictions have little actionable value'. Craig Venter, who so successfully demonstrated that private initiatives can quickly exploit technical innovation, reviews the spectacular progress made in the pace of sequencing with dramatic cost reductions. New instruments have arrived that can generate between 25 and 100 billion base pairs per day, with even greater speeds promised in the near future. Companies now offer a human genome sequence 'in a day for less than $6000'. Venter provides a note of caution in noting that 'these impressive increases have come with a big penalty'. Data obtained from short stretches of DNA need to be assembled, requiring 'layering results on one of the already published human genome sequences'. This procedure 'gives a distorted view of any single genome and, despite advances in processing, the resulting data quality is still well below diagnostic standards'. Venter raises an issue that must always have been obvious when he points out: 'The experiments that will change medicine, revealing the relationship between human genetic variation and biological outcomes such as physiology and disease, will require the complete genomes of tens of thousands of humans together with comprehensive digitized phenotype data . . . . Even if we had all this information today, we wouldn't be able to make use of it because we don't have the computational infrastructure to compare even thousands of genotypes and phenotypes with each other. The need for such an analysis could be the best justification for building a proposed 'exascale' supercomputer which would run 1000 times faster than today's fastest computers' (Venter, J. C., *Nature*, 2010, **464**, 676).

The human genome project has always had its detractors. The fear that 'big science', often driven by the political and managerial abilities of persuasive scientists, will result in a reduced share of the funding cake for 'small science' has been widely articulated in the global scientific community. There is a heightened sense of alarm when science funding is under threat from economic downturns. But, there is another fear. Will the focus on the large data gathering projects threaten the long favoured route of 'hypothesis driven science'? Drawing on examples from the field of cancer genomics, arguably the most visible area of intersection between genome sequences and human disease, R. A. Weinberg presents a forceful critique: 'These massive data-generating projects have yet to yield a clear consensus about how many somatic mutations are required to create a human tumour, and have given us few major breakthroughs in our understanding of how tumours develop. The cost of each conceptual insight has been very high indeed – although this is likely to change as technology costs tumble' (*Nature*, 2010, **464**, 678). The genomics programs have even come under criticism for their choice of words. Arguing for the correct use of language Horace Judson wrote nearly a decade ago: 'Look at the phrase – or marketing slogan – "the human genome project". In reality, of course, we have not just one genome but billions. At the level of genes, the project promises a useful consensus, but at the level of sequences of nucleotides variability is great and important, and not just for uniquely indentifying rapists and murderers. Clues to disorders, to talents and even to human origins may be buried there' (Judson, H. F., *Nature*, 2001, **409**, 709). Judson points out that imprecise language leads to 'public misinformation' which 'is largely and in origin the fault of scientists themselves'. He quotes Bacon: 'Words turn back and reflect their power upon the understanding'.

Did the human genome project attract funding at a level that suggest factors other than hard-headed evaluation of the outcomes were at work? In a provocative commentary entitled 'Bursting the genomics bubble', Phillip Ball (*Nature News*, 31 March 2010) draws attention to the work of a group of Swiss researchers who suggest 'that the HGP was a 'social bubble' (Gisler, M. *et al.*, preprint http://www.arxiv.org/abs/1003.2882), analogous to the notorious economic bubbles in which investment far outstrips any rational cost-benefit analysis of the likely returns'. Ball likens economic bubbles 'to what John Maynard Keynes called "animal spirits" whereby the instability stems from "the characteristic of human nature that a large proportion of our positive activities depend on spontaneous optimism rather than mathematical expectations" '. Ball is brutally critical about other 'omics' programs when he concludes: 'Before animal spirits transform this into the next "revolution in medicine", it might be wise to ask whether the HGP has something to tell us about the wisdom of collecting huge quantities of stamps before we know anything about them'. There is, of course, a counter view which persuasively argues that genomic data must come first in the battle against disease. T. R. Golub notes: 'The battle to decipher the molecular basis of cancer will surely be won over the decade ahead, but without the chemical tools to correct the cellular processes triggered by genetic abnormalities it will be a hollow victory. To make the most of genomic approaches to cancer the research community must be as innovative and systematic about finding these chemical tools as it has been about uncovering the genetics' (*Nature*, 2010, **464**, 679). The human genome project has transformed the face of biological research. Its effects will undoubtedly be felt on other disciplines.

P. Balaram

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EDITORIAL

Irreverence and Advancement

Irreverence is an uncommon word; in India it is also an unusual personal trait. A recent editorial in *Science* immediately attracted my attention when it proclaimed: 'Irreverence and Indian Science'. The author, one of India's most visible, influential and articulate scientists chose the medium of the flagship journal of US science to present a provocative view of the way forward for science in India (Mashelkar, R. A., *Science*, 2010, **328**, 547). Irreverence, in liberal doses, appears to be a key ingredient for science in this country to break free of the shackles of mediocrity and bureaucracy. A cursory glance at dictionaries suggests that 'irreverence' is a term used to describe a disregard for authority and is most often associated with a good humoured disdain for perceived wisdom. The *Science* editorial declares emphatically that the 'Nobel laureate Richard Feynman believed that creative pursuit in science requires irreverence'. A rebellious spirit is often a characteristic that one associates with the most creative people. In his essay Mashelkar draws attention to Freeman Dyson's book *The Scientist as Rebel* (The New York Review of Books, New York, 2006). Dyson's collection of essays, many of which are book reviews, are wide ranging but the title is taken from a piece in which he argues that 'science is an alliance of free spirits in all cultures rebelling against the local tyranny that each culture imposes on its children' (p. 4). Mashelkar quotes Dyson: 'For the great Indian physicists of this century, Raman, Bose and Saha, science was a double rebellion, first against English domination and second against the fatalistic ethic of Hinduism' (p. 4). While the first may be true, I am sure there will be many who will question the second. It is the word 'rebel' that needs the closest attention, for it is indeed used by Dyson in a special context. In his words: 'Science is an art form and not a philosophical method. The great advances in science usually result from new tools rather than new doctrines. If we try to squeeze science into a single philosophical viewpoint such as reductionism, we are like Procrustes chopping off the feet of his guests when they do not fit onto his bed. Science flourishes best when it uses freely all the tools at hand, unconstrained by preconceived notions of what science ought to be' (p. 17). For many readers like me, unfamiliar with Greek mythology, a quick glance at *Wikipedia* would reveal that Procrustes was a malignant figure who 'would amputate the excess length' of his guests in order to make them fit the bed. There is also the helpful explanation of a 'Procrustean solution' which 'is the undesirable practice of tailoring data to fit its container or some other preconceived structure'.

Richard Feynman undoubtedly fitted the public image of a brilliant and irreverent scientist. His student, Dyson, describes him as a 'rebellious spirit who combined a serious dedication to science with joyful adventures in the world outside'. Dyson's analysis of the styles of two of the iconic figures of 20th century physics is penetrating: 'Great scientists come in two varieties, which Isaiah Berlin, quoting the seventh-century-BC poet Archilochus, called foxes and hedgehogs. Foxes know many tricks, hedgehogs only one. Foxes are interested in everything, and move easily from one problem to another. Hedgehogs are interested only in a few problems which they consider fundamental, and stick with the same problems for years or decades. Most of the great discoveries are made by hedgehogs, most of the little discoveries by foxes. Science needs both foxes and hedgehogs for its healthy growth, hedgehogs to dig deep into the nature of things, foxes to explore the complicated details of our marvellous universe. Albert Einstein was a hedgehog; Richard Feynman was a fox' (p. 269). In reviewing a collection of Feynman's letters which were published, Dyson quotes from one written to a former student: 'I have worked on innumerable problems that you would call humble, but which I enjoyed and felt very good about because I sometimes could partly succeed. . . . No problem is too small or too trivial if we can really do something about it' (p. 270). This is a sentiment that should encourage the overwhelming majority of researchers who do not work on the 'grand challenges'. In some sense, Feynman's advice that scientists should enjoy their work is counter to a view held by many famous and successful scientists that 'small' problems are not worth doing. Dyson draws attention to the fact that 'the vision of science as rebellion' was first advanced by J. B. S. Haldane in a lecture at Cambridge in 1923, to the Society of Heretics. Haldane, clearly seemed to be addressing religious conservatism: 'The conservative has but little to fear from the man whose reason is the servant of his passions, but let him beware of him in whom reason has become the greatest and most terrible of the passions'.

Reading Dyson's essay I could not help concluding that 'rebellion' in the context he discusses and 'irreverence' as used in the Mashelkar editorial are different. Irreverence as an ingredient for improving science in India was probably first suggested by J. B. S. Haldane. In his characteristically brusque fashion he stressed the importance of being impolite: 'I have already come to one conclusion as to why science in India is developing with disappointing slowness. It is not because Indians are stupid or lazy. It is because they are too polite' (*Science in Indian Culture*, New Age Publishers, Calcutta, 1991; see also *Current Science*, 2001, **80**, 1245). Haldane, a polymath, was famous for his disdain of authority. In his later years at the Indian Statistical Institute, Haldane expressed his dismay at the frequent travels of P. C. Mahalanobis by remarking: 'The journeying of our Director define a novel random vector'. Haldane was often scathing in his assessments. In savaging the science fiction trilogy by the writer C. S. Lewis, whom he described as a writer of books which are intended to defend Christianity, Haldane pounces upon a Lewis' sentence: 'A wrong sum can never be put right: but only by going back till you find the error and working afresh from that point, never by simply going on'. Haldane points out that he is an 'addict of the kind of "sum" called iteration' and illustrates the virtues of approximation with an equation arising 'in the theory of mosquito breeding' (http://www.marxists.org/archive/haldane). Haldance and Feynman display the irreverence that can only be admired in the most gifted.

I began this column intending to discuss the *Science* editorial on 'Irreverence and Indian Science', but digressed on encountering Freeman Dyson's entertaining collection of essays. In returning to my original theme I must ask: What is the context in which irreverence is advanced as a key ingredient in transforming science in India? Mashelkar suggests that irreverence is sadly 'missing in Indian science today'; a consequence of a 'traditional attitude that condemns irreverence'. He then lists the factors that stifle creativity: a cultural inheritance that inhibits questioning, an educational system that successfully suppresses originality and a bureaucracy that operates in an impenetrable cloak of conservatism. In a telling commentary on a system that he undoubtedly knows well, he quotes from the speeches of two Prime Ministers, Atal Bihari Vajpayee in 2001 and Manmohan Singh in 2010, delivered at Science Congresses separated by a decade, both of whom deplore the stranglehold of bureaucracy. He notes: 'Alas, during the intervening years little had changed'. Mashelkar's 'relentless optimism' surfaces in his last paragraphs when he brings the 'good news', undoubtedly to a Western audience, that 'the environment is about to change'. He highlights the ongoing transformation of higher education, following the dramatic expansion in the number of institutions and suggests that new laboratories 'are fostering a new culture of innovation'. This surge of optimism is also engendered by the fact that 'foreign companies have established 760 R&D centres that employ 160,000 researchers, many of whom are Indian returnees from abroad who bring with them different innovation and work perspectives, whilst at the same time reversing the brain drain'. He then exemplifies the spirit of irreverence that appears to be permeating Indian industry by noting that 'the recent launch of Tata Nano, the world's least expensive automobile' embodies a new 'spirit of adventurism'. Reading Mashelkar's editorial I was left with the feeling that the irreverence he favours may have little to do with Freeman Dyson's 'rebel' or the non-conformism of Haldane and Feynman. It is hard to imagine irreverence as a motivating factor in the many new industrial R&D laboratories being set up by foreign companies. The biggest among them seem to be structured, organized, efficient organizations with clear mandates and directions from their parent companies. The weekly (or sometimes daily) videoconference with a disembodied supervisor can hardly stimulate irreverence. Science is now a highly organized, professional and multi-national activity. The days of the talented amateurs appear to be over. As an aside I must mention that Dyson's collection also contains an essay 'In Praise of Amateurs'. Interestingly, Dyson notes that 'in the nineteenth century professionals such as Michael Faraday and James Clerk Maxwell were the rule and amateurs Charles Darwin and Gregor Mendel were the exceptions' (p. 181). By modern standards we might label all these great figures as 'irreverent amateurs'. Irreverence is not a quality that is easily found in the most celebrated laboratories of science today. The demands of competition, relentless pressures to perform and the need to continually generate research support are not factors that promote 'irreverence'. Fortunately, many problems that need to be solved will undoubtedly succumb to the superior might of technical tools that are employed in the search for solutions.

While promoting 'irreverence' in India may provide a welcome breath of fresh air in our laboratories, there must be other ingredients which may be needed to catalyse transformation. Mashelkar concludes: 'If India can thereby build the spirit of irreverence that Feynman endorsed, then surely Indian science will create many Ramans of the 21st century'. Success will undoubtedly need more than irreverence. It will need professionalism, a clear understanding of the virtues of collaboration and cooperation and an honest and rational system of measuring and assessing performance. It will also require commitment, enthusiasm and resilience. The institutional structure of Indian science is pyramidal. Adapting an idea from the late C. K. Prahlad, renowned in the management arena, there may be riches that need to be uncovered at the bottom of the pyramid.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Social Scientists, Natural Scientists and Sociobiology

Scientists are generally thought to be people who are unafraid to ask questions. After all science seems to progress by the method of asking questions and seeking answers. Reality is somewhat different. Many of us are afraid to ask questions which might appear to be silly and I am no exception. In seminars, one cannot but help having a sneaking feeling of admiration for the person asking the question that many are afraid to raise, for fear of being dismissively brushed aside by a speaker of formidable repute. I was therefore mildly jealous and greatly pleased when a distinguished and perceptive colleague of mine, Raghavendra Gadagkar, sent me a reprint of a lecture that he had delivered at the Institute for Advanced Study in Berlin (Wissenschaftskolleg zu Berlin). In this talk Gadagkar raises questions which have sometimes crossed my mind and I am sure many others have wondered similarly: Why do social scientists and natural scientists behave in contrasting fashion in their lectures and seminars? Gadagkar, a professional ethologist, used his time at an institution, where equal numbers of social and natural scientists mingle, to observe and analyse the behaviour of his colleagues in colloquia. The Wissenschaftskolleg does not seem to have an equivalent in India; a gap that may undoubtedly be filled in the future. In surroundings where biology coexists with sociology, history, philosophy and music, Gadagkar's observational talents were put to good use to raise three questions:

1. Why do social scientists sit while presenting colloquia, while all natural scientists stand? ('The Sit–Stand Dichotomy').
2. Why do social scientists read from a prepared text, while natural scientists speak extempore? ('The Read–Speak Dichotomy').
3. Why do social scientists use 'numerous quotations from other scholars to make their points' while natural scientist rarely turn to quotations? ('Quote–Unquote Syndrome').

Gadagkar seemed, in many ways, eminently suited to address these questions having begun his scientific career as a student of molecular biology, only to realize rather quickly in the late 1970s that other areas of biology seemed poised for major conceptual advances. Evolutionary biology, animal behaviour, ecology and population genetics appeared to be coalescing in the newly developing field of sociobiology, promising to eventually provide a bridge over the yawning chasm that separates the molecular reductionist view of biology and the disciplines that lie well within the borders of the social sciences, human behaviour and psychology amongst them. Darwinism and natural selection, operating at the level of genes that determine individual traits, eventually imposing constraints on groups and populations, seem central to sociobiology. Gadagkar's work on social insects, wasps and bees in particular, eminently qualify him to be an observer of the behaviour of living organisms, human beings amongst them, as individuals and in groups. Having spent much of his career in an environment where the social sciences are excluded, he notes that he found his Wissenschaftskolleg experience even more interesting than he had imagined: 'The reason for this was that I became even more interested in how these "strange" colleagues pursued their craft than in what they actually did' (Gadagkar, R., The evolution of a biologist in an interdisciplinary environment. In *25 Jahre Wissenschaftskolleg zu Berlin 1981–2006*, Akademie Verlag GmBH, Berlin, 2006, pp. 167–180).

In attempting to 'explain' and 'understand' the contrasting behaviour of social and natural scientists in their seminars, Gadagkar reminds readers that behavioural ecologists are fond of three explanations: 'These are (1) random genetic drift, (2) natural selection, and (3) phylogenetic constraints'. At first glance all three explanations seemed to be shrouded in the complexities of a specialized field. But his translation of these terms for the layman bears repetition: 'Some behaviour patterns are neither particularly beneficial nor particularly detrimental and therefore they are neither lost nor do they eliminate the alternative and go to fixation. The laws of statistics govern the dynamics of their spread and persistence. This phenomenon is called random genetic drift, or simply drift. Other behaviours are maintained (do not disappear) because they are significantly beneficial to the actors and are preferentially preserved relative to alternative behaviour patterns. This is called natural selection, or simply selection. Yet other behaviour patterns exist because of historical reasons; changing them is not easy, perhaps too expensive. This explanation is called a phylogenetic constraint, or simply phylogeny or history'.

By this stage my curiosity was roused. Was Gadagkar going to proffer a sociobiological explanation to rationalize the three striking contrasts between social and natural scientists? He did not disappoint. The 'sit–stand dichotomy' was clearly maintained by drift and history. Undoubtedly, effective presentations could be made either sitting or standing. Historical constraints seem to be operating on both groups. For explaining the 'read–speak dichotomy' Gadagkar chooses 'selection', not drift or history. Here he introduces a term that appeared to instantly bridge the gulf between the natural and social sciences – 'behavioural polymorphism'. He notes: 'The reason why the polymorphism is maintained is that while one behaviour pattern is effective for some individuals, a different pattern is effective for others'. In reflecting on why social scientists read from a prepared text, while their colleagues in the natural sciences speak extempore, he notes: 'I would argue that what a natural scientist says is often more important than how he says it. In contrast, how a social scientist says what she says is often at least as important as what she says'. The choice of gender was undoubtedly unintentional. He adds: 'Even within the natural sciences, one often encounters such differences. My favourite example is the contrast between a synthetic chemist, for whom content is far more important than style of presentation, and an evolutionary biologist, for whom style of presentation is at least as important as content.' Having aspired, somewhat unsuccessfully to be a synthetic chemist in the early stages of my career, I was forced to wonder: Were synthetic chemists generally bad seminar speakers, sacrificing form for content? Almost certainly, the writers views would be prejudiced by personal experience. Curiously, synthetic chemists have always associated 'art' with their craft. Was Gadagkar merely echoing a widely held view of chemists and chemistry? Intriguingly, a beautifully produced book entitled *Molecules that Changed the World* (Nicolaou, K. C. and Montagnon, T., Wiley VCH, Weinheim, 2008) is intended 'to enlighten and instill a greater appreciation in society at large about a difficult subject – chemistry'. The authors note in their Preface: 'Many people remember chemistry as one of their most challenging subjects in college or the class in which they struggled. For others, the mere mention of the word chemistry conjures up images of explosives, poisons, and pollution'. Public perception of the discipline would of course be further influenced negatively if chemists, on average, made poor presentations of their work. Gadagkar, having painted chemists as extreme even among natural scientists, adds that unlike natural science, historical or sociological analyses 'often have the unique imprint of the author and would hardly be the same if presented (orally or in writing) by someone else.

In contrasting social and natural scientists, Gadagkar notes that the 'social scientists' love of quotations and the natural scientists' rare use of them is perhaps the most interesting of the three differences'. Here 'selection', he argues, is 'the mechanism that maintains this behavioural polymorphism'. The objective criteria available for validating claims in the natural sciences are not readily extended to the domain of social sciences. The tendency to quote and cite precedence appears to be based on the need to garner support for a point of view or an interpretation. C. P. Snow's two cultures thus seem to be characterized by distinct behavioural traits, locked in position by 'selection' and 'history'.

Gadagkar's entertaining, but scholarly, reminder of the differences between social and natural scientists prompted me to turn to the one area that seemed to bridge the two cultures – sociobiology, a discipline 'defined as the systematic study of the biological basis of all social behaviour' (Wilson, E. O., *Sociobiology*, The Belknap Press, Harvard University Press, 1975). Nearly two decades after writing his book that launched the field, Wilson authored a fascinating account of the controversy that followed the emergence of the area of sociobiology. He notes that 'no scientist before . . . had employed the reasoning of population biology so consistently to account for the evolution of human behaviour by natural selection' (Wilson, E. O., *Naturalist*, Island Press, Washington DC, 1994). But the critics raised the bogey of 'genetic determinism' which Wilson describes as the 'bugbear of the social sciences'. His critics, including some of the best known names on the biology faculty of Harvard, declared human sociobiology to be 'not only unsupported by evidence but also politically dangerous'. Wilson points out that 'in the liberal dovecotes of Harvard University a reactionary professor is like an atheist in a monastery'. What was the goal in attempting to examine the connections between biology and behaviour? Wilson declares that his 'purpose was to celebrate diversity and demonstrate the intellectual power of evolutionary biology. . . . At some point. . . I came to believe that evolutionary biology should serve as the foundation of the social sciences'. What are the role of genes and culture in shaping human behaviour? Wilson has argued in the past that the 'true nature' of the interplay between genes and culture is 'the central problem of the social sciences'. This area, in Wilson's words, awaits 'the slow accretion of knowledge persuasive enough to attract scholars'. I could not help wondering whether the next generation of sociobiologists would sit or stand while describing their work, if they would read from text or speak extempore and if they would quote extensively from the work of their predecessors. Will sociobiology serve to bridge the natural and social sciences or will it eventually cut itself loose from its biological roots or will it get mired in the complexities of molecular reductionism when neuroscience attempts to explain human behaviour?

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Chemical Synthesis and the Synthetic Cell

Analysis and synthesis are terms that have a very special meaning in chemistry. The study of matter has always required analytical tools of increasing sophistication. Over the last two centuries the successes of physics and chemistry have provided remarkable insights into the nature of matter, living and non-living. The distinctions between biology and life on the one hand and chemistry and inanimate matter on the other were first blurred in the 1820s, when Friedrich Wohler famously demonstrated the production of urea, a molecule long associated with living organisms, by heating ammonium cyanate, a common chemical that belongs undoubtedly in the realm of inorganic chemistry. This was indeed the decisive step in demonstrating the unity of matter and chemistry. Vitalism, which endowed living systems with a shadowy 'vital force', was dealt a death blow when Eduard Buchner demonstrated fermentation by the 'juices of cells', in the later years of the 19th century. Biology clearly was controlled by chemistry. In the 20th century, both chemical synthesis and chemical analysis have made astounding progress. Synthesis, especially organic chemical synthesis, has focused on strategies to build, often painfully and laboriously, molecules that are produced with remarkable ease and facility by living organisms. The 'total synthesis' of natural products is an area where the experimental campaigns have been long and hard. The targets when achieved, have permitted the development of closely related 'non-natural' structures, that have often proved immensely valuable as therapeutics. Nature's chemical virtuosity has endowed 'natural products' with a degree of structural variability that never ceases to amaze the connoisseurs of molecular structures. Organic synthesis has for long been pre-occupied with targets, which display a degree of structural intricacy, that provide an intellectual challenge to the practitioner. Stitching groups of atoms together, while exercising total control over their three-dimensional relationships, is not a task for the faint hearted. Imagination, experimental skills, patience and perseverance are key ingredients in the long and sometimes tedious campaigns of total synthesis. The field of organic synthesis, like most other fields of science, is changing with the times. Terms like 'atom economy' and 'green chemistry' may soon dominate the field. A recent essay highlighting the challenges of the field argues that, 'potentially the biggest challenge for twenty first-century synthetic chemistry' will not just be 'preparing a given target molecule, but doing so with a level of efficiency and cost-effectiveness that rivals that of nature itself' (Snyder, S. A., *Nature*, 2010, **465**, 560).

Traditional 'natural products chemistry' has always limited its attention to the diverse 'secondary metabolites' produced by living organisms, most notably microbes and plants. Nature produces these structurally diverse molecules ('biosynthesis') using groups of enzymes that catalyse chemical reactions that are often hard to mimic in the laboratory. Nature's chemistry is truly 'green', displaying a degree of efficiency that can only be envied. The enzymes (invariably proteins) are in turn produced with amazing efficiency as products of a 'DNA blueprint' that is present in the organism's genome. Sophisticated control mechanisms can turn these chemical pathways on and off. Organisms thus develop a sophisticated chemical response to environmental stimuli. Curiously, while secondary metabolites have provided synthetic chemists with many peaks to conquer, the products of primary metabolism have always been of interest only on the fringes of mainstream organic chemistry.

The central actors of biological chemistry, proteins and nucleic acids are polymers in which a limited number of structural units (monomers) are strung together into long monotonous chains. The size and repetitive nature of the chemical steps needed to assemble these polymers has always appeared to pose a technical challenge of formidable proportions. The chemistry of proteins and nucleic acids has its origins in the work of Emil Fischer in the final years of the 19th century and the early years of the 20th century. The most dramatic progress occurred in the period between the 1950s and 1970s when the methodologies for nucleic acid synthesis, DNA in particular, were established by Hargobind Khorana and the principles of 'solid phase synthesis' were developed by Bruce Merrifield. The ability to synthesize, stepwise, long chains of proteins and nucleic acids has been facilitated immeasurably by the technique of holding the growing polymer chain on a solid, insoluble polymer bead, while washing off reagents and reactants in step after step. The repetitiveness of the steps involved lends itself to automation. Automated 'synthesizers' now adorn laboratories;

sleek replacements for trained and tired chemists. Researchers today can order synthetic nucleic acids and proteins over the Internet; the credit card and a research grant being the sole requirements for acquiring material, that was completely unavailable not too long ago. Molecular biology has been revolutionized by the ability to analyse (sequence) genomes with amazing speed. Synthetic genes can also be assembled at relatively low cost and high speed. The abilities to cut, paste, modify and copy DNA have been central to the advance of biotechnology. The announcement of the 'Creation of a bacterial cell controlled by a chemically synthesized genome' seemed almost inevitable when it was first reported a couple of weeks ago (Gibson, D. G. *et al.*, *Science*, doi: 10.1126/science.1190719;2010). A team of authors led by Craig Venter describe the 'design, synthesis and assembly of the DNA corresponding to the genome of the organism *Mycoplasma mycoides*'. The task of putting together 1.08 million bases of the synthetic genome was accomplished by using 1078 'DNA cassettes' each having a length of 1080 base pairs. The synthetic sequence contains 'watermark' sequences which permit distinction from the natural genome. Laboratory 'forgeries' can be set apart from Nature's 'originals' using these cleverly placed identification marks. Venter and his colleagues successfully transplant the synthetic genome into a faster growing organism, *Mycoplasma capricolum*, in their quest to establish the ability of a cell to grow and divide under the control of a chemically synthesized genome. Specialists will undoubtedly dissect the strategies they employ enroute to their goal; but it is clear that this report constitutes a technical *tour de force*, which demonstrates the power of analysis and synthesis in contemporary molecular biology.

The paper by Venter and coworkers is yet another landmark in the triumphant advance of molecular biology. The authors note that 'our ability to rapidly digitize genomic information has increased by more than eight orders of magnitude over the past 25 years'. Their rationale for creating the chemically synthesized genome is simply stated: 'No single cellular system has all of its genes understood in terms of their biological roles. Even in simple bacterial cells, do the chromosomes contain the entire genetic repertoire? If so, can a complete genetic system be reproduced by chemical synthesis starting with only the digitized DNA sequence contained in a computer?' Have Venter and his colleagues successfully synthesized life, as represented by mycoplasma, from the bare chemical components of DNA? The authors note that they 'refer to . . . a cell controlled by a genome from chemically synthesized pieces as a "synthetic cell", even though the cytoplasm of the recipient cell is not synthetic'. They further argue that the 'phenotypic effects of the recipient cytoplasm are diluted with protein turnover and as cells carrying only the transplanted genome replicate'. They also add: 'The properties of the cells controlled by the assembled genome are expected to be the same as if the whole cell had been produced synthetically (the DNA software builds its own hardware)'.

The Venter paper has reopened an old question: 'Can life be synthesized?' The numerous commentaries that have begun to appear after the recent paper in *Science* acknowledge with admiration a major technical achievement, but are quick to dispel the notion that the 'synthesis of life' has been achieved in the laboratory starting from chemical constituents of cells. The opinions of 'eight synthetic biology experts' on the 'synthetic cell' that appear in a recent issue of *Nature* (2010, **465**, 422) make interesting reading. Steven Benner presents a provocative view: 'Synthesis is not a field. Rather, it is a research strategy that can be applied to any field in which technology allows scientists to design new subject matter. Such technology has long been available to chemistry where it allowed theory to develop faster than in fields lacking synthesis, such as planetary science and biology'. Benner notes that the real challenges of synthesis lie not in tinkering with 'natural biological parts' but in new design. He adds: 'Should . . . design strategies be flawed they will fail in ways that cannot be ignored. Thus synthesis drives discovery and technological innovation in ways that observation and analysis cannot' (p. 423). Jim Collins provides the dampener: 'Frankly, scientists do not know enough about biology to create life. Although the Human Genome Project has expanded the parts list for cells, there is no instruction manual for putting them together to produce a living cell. It is like trying to assemble an operational jumbo jet from its parts list – impossible' (p. 424). The 'synthetic cell' has reopened an old question: 'What is life', famously asked by Erwin Schrödinger in a book that drew a generation of physicists to biology. The answer has never been very clear and the debate on the synthetic cell is unlikely to yield a satisfactory resolution.

The technical triumph reported by Venter and coworkers opens up the field of engineered genomes, with many potential applications in biotechnology. Genome analysis and synthesis will undoubtedly be central in the attempt to create engineered organisms capable of performing designed chemistry. Synthetic life may still remain a distant dream. Many years ago, when one of synthetic chemistry's iconic figures, R. B. Woodward, received the Nobel Prize (1965) for his contributions to the 'art of organic synthesis' he is reported to have been asked: 'Will you next synthesize life'. His response (possibly apocryphal) will please many: 'I am quite happy with the way it is presently done.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Bhopal: The Tragedy of Collective Amnesia

Methyl isocyanate (MIC) is a relatively simple chemical; its formula, CH_3–N=C=O, fits conveniently into a line of text. Simplicity can, of course, be deceptive in chemistry with some of the most simple chemicals, hydrogen cyanide and carbon monoxide among them, acquiring great notoriety because of their alarming toxicity. The largest industrial disaster in our history occurred over 25 years ago when MIC leaked from the storage tank in the Union Carbide factory at Bhopal on the night of December 2–3, 1984. The carnage that followed has, over the years, claimed an estimated 20,000 lives and left many many more struggling with the consequences of inhaling MIC and other toxic substances that enveloped the area around the chemical plant. Bhopal's tragedy has slowly faded from public memory; even the observance of the 25th anniversary in December 2009 was muted. The judgement of a local court handing down punishment to those held guilty of negligence, a couple of weeks ago, has sparked waves of indignation at the small quantum of the sentence. A post-mortem has begun on the circumstances which allowed the Union Carbide chairman, Warren Anderson, to leave the country a quarter of a century ago. Discussions now rage on the compensation package that was accepted in an out of court settlement. Sadly, 25 years after the incident we now discuss the costs of clean-up of the site and the contamination of soil and groundwater. The 25th anniversary of the disaster, which passed largely unnoticed last December, was marked by an unusual event; a scientific lecture held in a small lecture room of an institution in Delhi. S. Sriramachari, undoubtedly a legend amongst pathologists and medical scientists in India, gave his last lecture on December 3, 2009, speaking of his long, and sometimes lonely, trail of research to understand the events at Bhopal and the effects of MIC and other toxic substances on the thousands of affected individuals. He died a few days later on December 25, 2009 of chronic pulmonary disease, a condition that he had undoubtedly encountered in the many victims of Bhopal.

Sriramachari (1925–2009) was an unusual man (Indira Nath, *Curr. Sci.*, 2010, **98**, 709; *Indian J. Med. Res.*, 2010, **131**, 463; Ramachandran, R., *Frontline*, Jan 16–29, 2010). In an interview, less than two years before his death, Sriramachari describes the circumstances that took him to Bhopal shortly after the deadly gas leak. The scene, in his words, 'was so dispiriting and horrible – several thousand people had died and mass cremation was done. There was continuing damage to the eyes and lungs of people, and the bodies were scattered round the Medico-Legal Institute (MLI) – one of the most extraordinarily equipped forensic laboratories in the country, of which my friend Dr Heeresh Chandra was the Director; he was an outstandingly dynamic and committed scientist' (Varadarajan, S., *Resonance*, March 2008, pp. 292–306). Heeresh Chandra, by many accounts was an unsung hero of the days and weeks that followed the leak of MIC from the Union Carbide plant in Bhopal. Together with his colleagues at the MLI, he bore the brunt of the enormously difficult task of performing autopsies on the bodies that began to pour into his institution. In Sriramachari's words: 'By 7:00 AM 70 people were dead, by 9 AM 260 were dead and thereafter the figures continued to rise. Though not all dead bodies were brought to the MLI, 311 bodies were received on 3.12.1984. Thereafter, the rate declined. A total of 731 bodies were received in December 1984 alone, 103 in 1985, 90 in 1986 and 44 and 22, respectively, in 1987 and 1988. These figures from the morgue may not account for all the deaths in the city of Bhopal. The MLI continued to perform autopsies on gas-affected victims in subsequent years.' Heeresh Chandra and Sriramachari presented their forensic and pathology findings at the Third Indo-Pacific Congress on Legal Medicine held at Madras, five years after the Bhopal disaster, in December 1989. Sriramachari quoted years later from a review of the proceedings: 'A silent stunned audience listened with awe the terrible story of 1984, when on one tragic day poisonous fumes killed hundreds of people or maimed thousands of them in coming months'. The reviewer 'likened this tragedy to "Pompei suddenly engulfed in the dust of Vesuvius or Hiroshima when the atom bomb was dropped" ' (Sriramachari, S., *Curr. Sci.*, 2004, **86**, 205). In researching for this column, I searched in vain for an account of Heeresh Chandra's life and work, finding only a 'bio-data' posted some years ago on a website. The 17 scientific papers and one technical report that he published in the years following Bhopal tell us nothing of the man and his undoubted and admirable commitment to forensic science.

I first saw Sriramachari sometime in 1985, when he along with the then Director General of the Indian Council

of Medical Research, V. Ramalingaswami, visited the Indian Institute of Science to speak to biochemists about the events at Bhopal. Sriramachari's purpose then, and he was to pursue the problem for the last two decades of his life, was to determine the biochemical and biological consequences of human exposure to MIC. I cannot recall any significant reaction to the problems that he posed from the small audience that had assembled to hear him. The enormity of the tragedy and the potential for long-term consequences of toxic gas exposure did not seem to be appreciated. In the last ten years or so I met Sriramachari several times and heard him describe his work and ideas on the 'biochemical lesions' induced by MIC. In the terrible hours following leakage of about 40 tonnes of MIC, from the now notorious tank 610, a problem that confronted the clinicians and medical staff of Bhopal's hospitals was whether there was an 'antidote' that could be administered. For many extremely hazardous chemicals, protocols for emergency treatment of exposed individuals are usually known. The horrendous toxicity of the 'Bhopal gas' was hardly anticipated by its manufacturers in India. Sriramachari described the immediate observations by the medical team at MLI, Bhopal: 'The autopsies revealed an intense red discoloration of the viscera, especially the lungs, trachea and upper respiratory tract and my friend [Heeresh Chandra] coined the term "cherry red discoloration". . . Death was very sudden and was due to lack of oxygen reaching the respiratory centres' (Varadarajan, S., *Resonance*, March 2008, pp. 292–306). I heard the term 'cherry red discoloration' on many occasions as Sriramachari sought to interest listeners on the subject of the biochemical and pathological investigations on Bhopal victims. There was a very special reason for his interest. 'On the basis of autopsy findings, Heeresh Chandra postulated cyanide toxicity as a causative factor.' Immediately after the disaster a German toxicologist, Max Daunderer, who suspected cyanide toxicity, arrived in Bhopal carrying 'diagnostic kits and ample supplies of sodium thiosulphate (NaTS). These developments spurred vigorous research into the possible causes of the "cherry red discoloration" ' (Sriramachari, S., *Curr. Sci.*, 2004, **86**, 905). The 'cyanide theory' was countered by Union Carbide which 'let loose a campaign of misinformation'. The results of NaTS treatment were puzzling; several survivors of the gas leak appeared to respond well, but many instances of relapse and recurrence of symptoms were noted. Did NaTS administration need to be countered so vigorously that government agencies ensured cessation, effectively discounting the 'cyanide hypothesis'? If Sriramachari were with us he may have raised this question in his characteristically gentle and polite manner.

In March 2003, Sriramachari presented a detailed account of 'The Bhopal Gas Tragedy – An Environmental Disaster' at the Fourth Sir Dorabji Tata symposium on 'Trends in Respiratory Diseases'. He was persuaded to permit reprinting of the text of his paper in this journal (*Curr. Sci.*, 2004, **86**, 905–920). Almost immediately thereafter this journal received a rebuttal entitled 'The Bhopal gas tragedy: Evidence for cyanide poisoning not convincing' (Gassert, T. S. and Ramana Dhara, V., *Curr. Sci.*, 2005, **89**, 923). The correspondents expressed two major concerns: '(a) lack of evidence of cyanide release and (b) reliance in part upon unpublished and incomplete data'. At that time I was struck by the immediacy of the response and the tone of the critics based in US institutions. Sriramachari responded to the criticism beginning with a disclaimer: '. . . the paper based on our scientific studies does not represent any ICMR report'. He argued that his critics 'with pre-conceived notions have tried to weave a criticism based on some unpublished and some irrelevant published reports to ridicule the cyanide hypothesis'. He reiterated the main findings of his studies, pointing to cyanide as one of the many toxic agents in Bhopal, noting that 'elevated blood cyanide and urinary SCN levels and response to NaTS therapy up to 1986, as reported in *Current Science* provide convincing evidence' (Sriramachari, S., *Curr. Sci.*, 2005, **89**, 924).

The ICMR carried out a series of research investigations between 1985 and 1994. Curiously, the 'Report on Health Effects of the Toxic Gas Leak from the Methyl Isocyanate Plant in Bhopal' has a release date of 2004. While 'nearly three-fourths of the deaths occurred within the first 72 hours of the leak . . . a large fraction of the exposed population continues to be chronically ill with diseases of the respiratory, gastro-intestinal, reproductive, musculoskeletal, neurological and other systems' (Mishra, P. K. *et al.*, *Int. J. Occup. Med. Env. Health*, 2009, **22**, 193). Sriramachari completed a report of his work before his death, which should be valuable to future researchers who seek to understand the nature of the substances let loose in Bhopal and the mechanisms by which they might exert both short-term and long-term effects. In his 2004 summary, Sriramachari suggested that 'the moment the Bhopal gas disaster took place the Union Carbide Company adopted a policy of *suppressio vary* and *suggestio falsi*'. In the context of the current debate on compensation for victims and penalties for negligence, the nature of information provided on chemistry, metabolic fate and toxicity may be important. In thinking of Sriramachari, Heeresh Chandra and the many others who committed themselves to help understand the tragedy at Bhopal, I could not help but reflect on the negligence of those communities which should have acted with alacrity and sincerity; politicians, bureaucrats, industry, judiciary and scientists. In the last quarter of a century no Academy of science, no advisory council to governments and Prime Ministers and no agency devoted to science has thought it necessary to push for environmental clean up or speedy publication of scientific studies which may help to understand the cause of death and long-term consequences of exposure. This is the tragedy of collective amnesia.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Research and the Human Element

The communication revolution has dramatically increased the generation gap. The Internet has spawned an ever expanding universe of blogs; a public medium for self-expression, that appears irresistibly attractive to a new generation of communicators. There are wonderful sites where thoughtful bloggers draw attention to articles that may pass unnoticed by many, quickly providing an entry to an unseen and diverse discussion group spread across the world. There are others which appear to be a public forum to vent frustration; often accumulating comments which are harsh and poorly worded. Some sites are exceptionally useful providing a quality of practical technical discussion, which can enrich both the practising researcher and the beginning student, tentatively taking the initial steps in research. Sharing information, experience, knowledge and occasionally, wisdom with an unseen audience seems to have become routine in the bloggers' universe. Having grown up with the traditional printed word, I have been slow to shed my prejudices and inhibitions and learn to browse electronically. 'Surfing' was always a word that I found hard to associate with the world wide web. It was thus almost completely by accident that I stumbled upon a blog site devoted to chemistry, where a discussion seemed to be raging on the travails of graduate students and post-doctoral fellows, who appeared to be toiling, at times with limited success, in the laboratories of ambitious, aggressive and successful professors. Their condition was likened by some imaginative correspondents to that of slaves, unable to break free from the shackles imposed by cruel and unforgiving supervisors. The immediate provocation for this collective outburst by graduate students working in chemistry laboratories, primarily those involved in 'total synthesis' of complex organic molecules, was the posting on a blog site of a letter written in 1996 by a Caltech professor to an apparently errant, young coworker, whose absences from the laboratory must have jarred the sensibilities of a workaholic professor. Having spent sometime in my youth in surroundings that seemed to resemble the environments described by the correspondents on the blog site, I was irresistibly drawn to read the discussion; at times with the guilty feeling of eavesdropping on a conversation between students, enjoying the time honoured practice of mercilessly deconstructing professorial reputations.

The offending letter, curiously enough dug out of oblivion, appears to state the expectations of some of the most competitive and productive laboratories, where long hours and seven day work weeks are the norm. This was and probably continues to be the expected work ethic in some of the high profile experimental groups, especially in the United States. The ability of US laboratories to accommodate large numbers of overseas graduate students, especially from Asia, ensures that the famed American 'weekend' is not necessarily a part of the culture of many laboratories. The work ethic varies widely across the world and many European laboratories probably provide a more relaxed ambience. The primacy, over the last half a century, of American science is in large measure due to a culture of work in a highly competitive environment. The tenure system for young faculty together with the tremendous pressure to raise research grants to support laboratories, postdoctoral fellows and graduate students ensures that only the 'fittest' survive to build long careers at the best institutions. The competitive pressures of research have dramatically increased over the years as the academic research world has expanded. Publishing results in the most prestigious journals is becoming increasingly difficult as the number of manuscripts jostling for limited journal space increases. To reach and maintain a high level of performance is not easy; requiring total committment and an obsessive focus on success. Unsurprisingly, the pressure is felt, sometimes suffocatingly, at the lowest rung of the academic ladder – the graduate student. Some fields are worse than others; synthetic organic chemistry, according to the blog site, and, I suspect, some areas of molecular biology and biomedical science. Traditionally, synthetic chemistry always involved long hours, a great deal of physical labour and continuous spells of work that cannot be easily interrupted. Curiously, unlike many other areas of experimental science, the march of technology has not really reduced the physical burdens of synthetic chemistry. While professors may enjoy the intellectual challenge of designing synthetic routes, plotting strategies to conquer complex molecular structures, the students and postdocs must toil in the laboratory, to achieve experimental realization of the goal. Success demands perseverance and hard work. As in many other areas of research, the

ability to fail repeatedly without getting disheartened is the key to success in difficult problems. Unlike some areas of theoretical science, these fields require teams of workers, with the professor doubling as coach and manager. The sobriquet of 'slave driver' is almost inevitable as the competitive stakes begin to rise. The professors, of course, have won their spurs after passing through the rigours of apprenticeship in laboratories where the 'work ethic' was clearly defined by tradition. Their expectations of those who work under them are undoubtedly a result of their own training, tempered at times by their own personalities. The correspondence on the blog site reminded me of a familiar complaint – the enormous power that professors seem to wield over the academic futures of their coworkers. The power of recommendation letters, especially those written by powerful professors, can make or break academic careers of students and postdocs. The sword of Damocles thus hangs over the heads of the non-conformist and rebellious student. As the academic job market shrinks, these pressures are bound to rise.

The research ambience in institutions and individual laboratories can vary widely. In India Ph D students in the best of our institutions may feel some of the competitive pressures that are more common in US laboratories. However, in most institutions research students may be left to their own devices, often working on problems that lie on the fringes of a field. In the most visible institutions the pressures of competitive research are evident. In some fields, notably chemistry and biology, the pressures to publish in 'high impact' journals constantly increase. Peer review committees which evaluate grant proposals and assess faculty for career advancement are undoubtedly influenced by publication lists. Inevitably, the pressure is transmitted to students and coworkers in the more ambitious laboratories. While in Indian academic laboratories large research groups rarely work coherently towards a single goal, the demands of many contemporary fields require a strongly multidisciplinary approach for success. Thus collaboration between research groups and cooperation within a group become essential ingredients for high productivity. Cooperation within a research group is not always easy to achieve; friendly and fruitful collaboration between groups is relatively rare. Issues of credit sharing – some real, others imaginary – dominate the discourse in research laboratories. How often have we heard the question while evaluating the credentials of a potential candidate for an academic position in a national institution: 'How many "first author" papers does the individual have?' The popularity of this question, admittedly in some fields, ensures that there is a scramble for 'first authorship', a syndrome that afflicted Western laboratories a long time ago. The footnote that appears in many papers stating 'these authors contributed equally' is a sad commentary on the widely held perceptions on credit sharing for a research outcome. In collaborations between researchers who bring complementary skills to bear on addressing a scientific problem, assessment committees often have the distressing habit of apportioning a disproportionate share of the credit to the individual who is not being assessed. There is also another unusual statistic that is often sought: 'How many "single author" papers do you have?' This question appears to signify a degree of disapproval of collaborative work, even when it may involve students working closely with research supervisors. In some fields, mathematics and some areas of theoretical physics are examples, groups and collaborations are less frequent. In many areas of experimental science the lone scientist is a species nearing extinction. Running large experimental laboratories requires professors with management skills, patience and the temperament to handle difficult coworkers; a combination of qualities that are rarely present in ambitious and aggressive scientists. The problems of maintaining common equipment and sharing common resources can be formidable, leading to discord and unhappiness within groups. Often the most hard working members of a group bear the greatest burden for keeping a laboratory functional. Artful dodgers who shirk any common activity are unlikely to be looked upon kindly either by their supervisors or their more industrious colleagues. In many of our institutions the work ethic of large sections of faculty is also a matter of concern. However, this issue is rarely raised or even acknowledged.

Research is performed best in an ambience that encourages the practitioners to enjoy their work. This is possible only in institutions where the collective morale is high and a significant proportion of its members are high performers. There must also be a high degree of respect for those who are scholarly, even when their productivity, as measured by the mundane parameters of scientometrics, is less than stellar. The diversity of temperaments and work habits amongst students and faculty members ensures that there can be no simple formula that promotes a harmonious relationship. Recognition of this fact may sometimes help in minimizing discord and unpleasantness. Modern science is an intensely competitive activity. The long time spans over which Ph D research is completed adds to the pressures and tension felt by young scientists as they begin to search for academic positions to launch their independent careers. Since the work of Ph D students also advances the careers of their supervisors, a failure to obtain results, in difficult and important projects, can result in unpleasant consequences. When I read the blog on synthetic chemistry and the shared experiences of graduate students, I was reminded of generals who sometimes control the troops at the frontlines, from the safety of fortified headquarters. The battle is lost when the generals lose contact with their men.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Physics and Physicists

The public perception of science and scientists is dominated by impressions of physics and physicists. Newton and Einstein are names known to almost every educated individual; the former intruding in the school years, where mechanics and calculus intimidate most students, while the latter's portrait remains the most recognizable in the pantheon of scientists. I was sharply reminded of this fact while lecturing at the Bangalore Science Forum, an admirable organization which has held a weekly event on science and a month long 'science festival' annually, for over four decades. The talks used to be held in an unprepossessing lecture room, fondly called 'H. N. Hall', after H. Narasimhaiah, the Gandhian educationist and committed evangelist for science. He had, over the years, raised the National College and the Science Forum in Bangalore to a position that commanded both loyalty and respect from those interested in the public perception of science. This year, in keeping with changing times, the lecture hall had been completely renovated; shining floors, an air-conditioned ambience, state-of-the-art projection facilities, comfortable chairs and, most importantly for the speaker, a stage whose stability was ensured, greeted me. Even as I surveyed this inevitable inroad of modernity, there was a pang of regret for what has undoubtedly passed into history, to be slowly forgotten by fading collective memory; an austere and ascetic ambience that undoubtedly reflected the styles of two of the men, whose large portraits hung on the newly painted walls of this modern lecture theater, Mahatma Gandhi and H. Narasimhaiah. But, as I lectured, it was the third portrait, the iconic, dishevelled image of Albert Einstein that seemed to pose a question: 'Did not physics, especially theoretical physics, dominate the public perception of science to an unreasonable degree?' In a curious way, such thoughts seemed disloyal to the surroundings. I consoled myself by recalling that Narasimhaiah was fond of questioning conventional wisdom and provoking debate. He was trained as a physicist; so too were some of his closest associates. It seemed natural that Einstein's portrait must adorn the walls. Somewhat guiltily I realized that Einstein looked down benignly from the walls of my own living room; symbolic of the common perceptions about science. Surely, there is no other image that is so closely associated with science; even the photograph of the mushroom cloud after the first atomic explosion or the image of Watson and Crick by the side of their model of the DNA double helix fail to evoke the sense of power that science has wielded in the 20th century. But, the question persisted in my mind: 'Did physics and physicists have a disproportionate influence in shaping the public image of science?'

What of the other disciplines? Mathematics is too abstract, at times esoteric and forbiddingly austere; mathematicians, eccentric, remote, often preferring cold, mathematical 'beauty' over the mundane considerations that seemed to bother physicists and engineers, for whom mathematics is an indispensable tool. In Einstein's assessment: 'As far as the laws of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality' (Einstein, A., *Ideas and Opinions*, The Modern Library, New York, 1954). Chemistry is prosaic, drowning in detail, at times unappealingly utilitarian. Even in an age dominated by economics and commerce, perceptions about science can be coloured by romanticism that can be traced to the extraordinary period in physics in the 1920s and 1930s. Chemists, too often are associated with smelly and accident-prone laboratories. Curiously, the quintessential scientist in most movies is surrounded by flasks and distillation apparatus in a surrounding that appears suspiciously like a chemistry laboratory. Biology seems too familiar and descriptive to a degree that is inhibitory; biologists, and I refer here only to the classicists, seem to be nature lovers practising an easily accessible craft. The new generation molecular biologists closely resemble chemists, drowning in a deluge of data, as genes, genomes, molecules and pathways multiply at an alarming pace. Molecular biology laboratories present a gleaming image but to an outsider the field appears to be shrouded in impenetrable jargon, obscuring the nature of the scientific problems being investigated. Geology bears an uncanny resemblance to classical biology; the former describing our inanimate surroundings, while the latter catalogs living forms. The new description of the science of the earth, 'Earth Systems Science', may hardly serve to shore up the image of an extremely important field, which is often considered a poor relative of the other sciences in India. Opinions about science are invariably shaped by experiences in

school and college. For students who read generally, and this number is shrinking alarmingly, books sometimes provide an important stimulus in shaping their thinking. There are many readable accounts of the lives of physicists and events in physics; far more than in other disciplines. Biology, arguably the dominant discipline of science today, can boast of two widely read books, separated by generations, which may have drawn wide attention: *Microbe Hunters* by Paul de Kruif (1926) and *The Double Helix* by James Watson (1968). Public perception of a field in the past was often influenced to a remarkable degree by a readable book.

In India the birth of modern science coincided to a great extent with the heroic age of physics. While J. C. Bose was the acknowledged pioneer, the Bengal renaissance with C. V. Raman, Meghnad Saha and S. N. Bose in the 1920s and 1930s ensured that the public image of science would be dominated by the successes of physics. Homi Bhabha's remarkable achievements as a builder of institutions and his championing of the power and potential of atomic energy strengthened the case for great investments, in economically difficult times, in areas of science dominated by physics. The mid-20th century revolution in biology traces its roots, not to the established laboratories of chemistry and biochemistry in the West; but, to the Cavendish Laboratory in Cambridge, presided over by W. L. Bragg, one of the founders of crystallography. Molecular biology's origins, both structural and genetic, may be traced to the work of Linus Pauling and Max Delbruck at Caltech. Pauling, an early practitioner of quantum mechanics and crystallography, tasted the atmosphere of physics of the 1920s in Europe; Delbruck began his career with Niels Bohr, Francis Crick began in physics; so too did G. N. Ramachandran. The mid-20th century drift into biology coincided with a feeling that 'physics' as perceived by the romantics of the 1930s was reaching an end; biology might indeed provide the setting for uncovering new laws of nature.

In thinking about physics and physicists, I was blessed by the appearance of two books on my table. The first was an unexpected gift from a friend who brought me a 'used copy' of a book that is now out of print: *The Physicists* by C. P. Snow (Little Brown and Company, Boston, 1981). I found the second while browsing, inevitably in an airport bookstore: *The Strangest Man. The Hidden Life of Paul Dirac, Quantum Genius*, by Graham Farmelo (Faber and Faber, London, 2009). Farmelo's biography of Dirac will undoubtedly interest those who enjoy accounts of the lives of scientists of formidable intellectual achievement. Dirac's 'strangeness', while manifest in his behaviour, was most evident in his work. He was the 'purest' of the theoreticians, who built the edifice of contemporary physics. I cannot resist reproducing two assessments of Dirac's work and style, which Farmelo uses in his eminently readable account. The first is from a talk by Steven Weinberg on the occasion of the Dirac Centenary: 'Dirac told physics students they should not worry about the meaning of equations, only about their beauty. This advice was good only for physicists whose sense of pure mathematical beauty is so keen that they rely on it to see the way ahead. There have not been many such physicists – perhaps only Dirac himself' (Farmelo, p. 428). The second is by Freeman Dyson: 'The great papers of the other quantum pioneers were more ragged, less perfectly formed than Dirac's. His great discoveries were like exquisitely carved marble statues falling out of the sky, one after another. He seemed to be able to conjure laws of nature from pure thought – it was this purity that made him unique.'

Snow's book is a classic. It is a first draft, written 'largely from memory' and published after his death. Snow lived, and for a while worked, amongst the Cambridge physicists led by Rutherford. In his career as a scientist and administrator, Snow witnessed history as it was made. In chronicling the events of his times, Snow's writing is direct and simple. A keen observer of men, he brings 'the physicists' to life. In describing two great contemporaries, Rutherford and Einstein, Snow reflects on 'the disparity in the treatment of the great experimentalists as contrasted with that of the great theoreticians. . . In terms of popular esteem, experimentalists felt, and still feel, as Rutherford did with his usual horsepower, that they got an unfair deal. The names of theoreticians survived in intellectual currency: the names of experimentalists didn't. Einstein provided the most vivid illustration'.

I began this column mildly puzzled by the public perception of physics and physicists. There may be no better way to end than to return to Snow's reflective assessment: 'The most incisive tribute to Einstein was made by Dirac, who doesn't inflate his words. Dirac said first that if Einstein hadn't published the Special Theory of Relativity in 1905, someone else would have done it within an extremely short time, five years or less. . . But, Dirac went on, the General Theory which Einstein published in 1916, is an entirely different matter. Without it, it is likely that we should still be waiting for the theory today. That is one of the most striking things ever said of one great scientist by another. . . There was no injustice in Einstein's transcending fame. Still it is possible that the *mana* of his personality encouraged it. . . When he felt deeply, he was rather like an Old Testament prophet, or else a benign deity being patient with human stupidity and worse – but also like a benign deity who had considerable physical resemblance to a handsome and inspired golliwog. No one who knew him expected to meet anything like that again: and they were right.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Mathematics and Mathematicians

Mathematics can be a formidable and intimidating subject for most people. In the school years, a distinction can be very quickly made between the talented minority who are mathematically gifted and the majority who look forward to the day when they will forever sever their connections to the subject. For students who go on to courses in engineering or some disciplines of science, primarily physics and chemistry, mathematics can continue to cast a long shadow even in the college years. I must confess it was a great relief to drift away into disciplines that required little by way of explicit mathematical skills. The subject returned to haunt me a few weeks ago when a persuasive colleague, involved in organizing the International Congress of Mathematicians (ICM) in Hyderabad, charged me with the task of producing an editorial essay that centres around mathematics. The ICM, like most quadrennial international congresses, is a large affair that moves around the world, much like the Olympic games (and the many more restricted events like the Asian and Commonwealth games). Countries bid to host Congresses, lobby hard, international committees politic and vote; eventually a location emerges. The 2010 ICM in Hyderabad brings the event to India for the first time in its long history, dating back to the first meeting in 1897 in Zurich. The last Congress (Spain 2006) attracted as many as 4000 participants, suggesting that Hyderabad may witness an unusual spectacle when the city hosts such a large congregation of mathematicians. Scientists, unlike sportsmen, require little by way of infrastructure when they assemble for these mega events; the ICM may therefore impose on the organizers a lesser burden than the forthcoming Commonwealth Games in Delhi. The ICM will have its public moments when the Fields medallists and winners of other major awards are announced. In Spain, the drama was provided by the refusal of the reclusive Russian mathematician, Grigori Perelman to accept the Fields Medal. The air of mystery that has so often been associated with the greatest of mathematicians was reinforced when Perelman turned down the million dollar Clay prize. The Perelman phenomenon brought mathematics to the front pages of newspapers, reminding ordinary readers that genius can defy interpretation.

Prodigies and geniuses seem so often associated with mathematics; born with intrinsic gifts that are hard to comprehend. The Ramanujan story, so wonderfully told by Robert Kanigel (*The Man Who Knew Infinity: A Life of the Genius Ramanujan*, Crown, 1991) has formed the basis for a recent novel: *The Indian Clerk* by David Leavitt, Bloomsbury, 2007. This is a fictional account, more about G. H. Hardy and the Cambridge ethos of the early years of the 20th century. The book begins with Hardy on the stage at Harvard in 1936 about to receive an honorary degree 'on the occasion of the university's tercentenary'. In Leavitt's words: 'Unlike most of the visitors however he was not here – nor, he sensed, had he been invited – to speak about his own work or his own life. That would have disappointed his listeners. They wanted to hear about Ramanujan'. In his classic preface to the 1967 edition of Hardy's *A Mathematician's Apology* (Cambridge University Press, 1940), C. P. Snow calls the Ramanujan discovery as 'the one romantic incident' in Hardy's life; 'an admirable story and one, which showers credit on nearly everyone (with two exceptions) in it'. The oft told story of Hardy and Littlewood poring over Ramanujan's 'sheets of paper by no means fresh' never pales in the retelling. Hardy was to later conclude 'that Ramanujan was, in terms of *natural* mathematical genius in the class of Gauss and Euler'. Was this an assessment that was easy to make? Snow says it best: 'It all sounds easy, the kind of judgement great mathematicians should have been able to make. But I mentioned that there were two persons who do not come out of the story with credit. Out of chivalry Hardy concealed this in all that he said or wrote about Ramanujan. . . . It is simple. Hardy was not the first eminent mathematician to be sent the Ramanujan manuscripts. There had been two before him, both English, both of the highest professional standard. They had each returned the manuscripts without comment. I don't think history relates what they said, if anything when Ramanujan became famous. Anyone who has been sent unsolicited material will have a sneaking sympathy with them' (Canto, 2000, pp. 33–34). While Snow's long Foreword (50 pages) is a masterpiece of simple prose and a deeply personal assessment of Hardy, *Apology* is an extraordinary piece of writing; reflective, brutally honest and, at times, hauntingly sad. When the short book appeared in 1940 no greater tribute could have been paid than Graham Greene, a writer of uncommon ability, comparing Hardy's writing to Henry James, praising the work as demonstrating 'with an absence of fuss, the excitement of the creative artist' (*Spectator*, December 1940). The reviews of Hardy's *Apology* were not all laudatory. An

analysis by Steve Whittle compares a variety of reactions to Hardy's reflective essay (http://www.aug.edu/dvskel/Whittle2002.htm). Frederick Soddy, the Nobel Prize winning discoverer of isotopes and a contemporary of Hardy, launched a blistering attack; provoked undoubtedly by Hardy's pacificism and his dismissal of 'applied mathematics' (*Nature*, 1941, **147**, 3). Whittle paraphrases Soddy, noting 'that counting and calculation, like art and music, are capable of giving pleasure to those who have the right kind of mind. And in the words of Soddy such skills are "the bloody masters" in a world that is ruled by counting tables'.

In mathematics, more than in any field of science, the superiority of 'pure' over 'applied' work seems implicit in the behaviour of the practitioners. Hardy is emphatic: 'There are two mathematics. There is the real mathematics of the mathematicians, and, there is what I will call the "trivial" mathematics, for want of a better word. The trivial mathematics may be justified, . . . but there is no such defence for the real mathematics, which must be justified as art if it can be justified at all' (p. 139). Hardy notes that Whitehead has spoken of 'the tremendous effect of mathematical knowledge on the lives of men, on their daily avocations, on the organisation of society'. His dismissal of these arguments is magisterial: 'The mathematics which can be used "for ordinary purposes by ordinary men" is negligible, and that which can be used by economists or sociologists hardly rises to scholarship standards' (p. 138). The results or, more appropriately, constructs of 'pure' mathematics have provided the mathematical framework of physics. Half a century ago Eugene Wigner wrote about 'The unreasonable effectiveness of mathematics in the natural sciences' (*Communications in Pure and Applied Mathematics*, 1960, vol. 13). He defined mathematics as 'the science of skillful operations with concepts and rules invented for this purpose'. Physics in Wigner's words is concerned with 'discovering the laws of inanimate nature'. He was cautious about 'theories' of biological phenomena. Wigner's conclusion may still be true: 'The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve. We should be grateful for it and hope that it will remain valid in future research and that it will extend, for better or for worse, to our pleasure, even though perhaps also to our bafflement, to wide branches of learning. Twenty years after Wigner, Richard Hamming returned to the same theme. His discussion centres on examples closer to engineering. Hamming's conclusions seem appealing: 'Mathematics has been made by man and therefore is apt to be continuously altered by him. Perhaps the original sources of mathematics were forced upon us . . . but we have made choices for the extensions that were only partly controlled by necessity and often, it seems to me, more by aesthetics. We have tried to make mathematics a consistent, beautiful thing, and by so doing we have had an amazing number of successful applications to the real world' (*The American Mathematical Monthly*, 1980, vol. 87). Twenty five years later Sundar Sarukkai revisited the same theme, suggesting that mathematics be viewed as a language that may be applied 'to some descriptions of the world'. He argues that 'the use of a language like English to describe the world is itself "unreasonably effective" and the puzzle with mathematics is just one reflection of this larger mystery of the relation between language and the world' (*Current Science*, 2005, **88**, 415).

I have tried, admittedly with considerable difficulty, to devote this column to general reflections on mathematics, a subject that has always seemed intimidating. In my years in college, now a rapidly receding memory, the names of D'Alembert, Lagrange and Euler seemed to intrude in mathematics classes. As I surfed the internet hoping to find material, relevant to the task at hand, I chanced upon an engaging historical account, with the inviting title, 'Frederick the Great on mathematics and mathematicians' (Cajori, F., *The American Mathematical Monthly*, 1927, **34**, 122). The article begins in compelling fashion: 'Frederick William I of Prussia ordered that his son, later known as Frederick the Great, should "learn no Latin"; "let him learn arithmetic, mathematics, artillery, – economy to the very bottom" '. The old king allowed the Berlin 'Society of Sciences', the favourite child of Leibniz, to languish and almost to pass away. His son, Frederick, on the other hand, secretly acquired some Latin, shunned the study of mathematics beyond its rudiments and brought the Berlin 'Academy' to great splendor. Frederick wrote to Voltaire in 1738: 'As for mathematics, I confess to you that I dislike it; it dries up the mind. We Germans have it only too dry; it is a sterile field which must be cultivated and watered constantly, that it may produce'. Despite this apparent dislike, Frederick cultivated mathematicians bringing to Berlin, among others, Euler and Lagrange. He tried hard to get D'Alembert to leave France, exchanging a great deal of correspondence. Cajori's 1927 article notes: 'D'Alembert once wrote to Frederick the Great: "It is the destiny of your majesty to be always at war; in summer with the Austrians, in winter with mathematics" '. Cajori concludes: 'Frederick the Great was desperately in love with poetry and philosophy, and wholly unsympathetic with mathematics. Yet, by his patronage at the Berlin Academy he contributed nothing substantial to poetry and philosophy, but achieved marvels in the advancement of mathematics. . . Frederick the Great's controlling motive for his academy was splendor. This is expressed in the form of the invitation he is said to have sent to Lagrange at Turin: "The greatest King of Europe" wishes to have "the greatest mathematician" at his court'. Both mathematics and recruitment practices have surely changed over the centuries.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Drug Resistant Bacteria and the Global Economy

Bacteria are amongst the most adaptable organisms on Earth. Long evolutionary timescales, extremely short generation times, exposure to the most diverse and often hostile environments, together with the remarkable power of natural selection have made microorganisms the most resilient of life forms on this planet. In very large measure, many forms of bacteria are benign and beneficial to their animal hosts; most often colonizing the guts of animals, humans amongst them, providing key enzymes for digestion and producing metabolites that are essential for normal biological function. Bacteria and fungi abound in the soil; essential contributors to maintaining an ecological balance in our environment. Microbiology provides the bridge between the study of plants and animals on the one hand and the earth sciences and agriculture on the other. However, bacteria have a very poor public image; most commonly being associated with infectious disease. Human pathogens constitute only a limited fraction of bacterial populations; yet their impact on human health has been devastating over the centuries. Despite the apparent sophistication of animal immune systems, these natural defences can be breached with impunity by pathogens, which have learnt to evade and subvert the immune processes, that identify and destroy 'foreign' organisms. One of the most dramatic developments of the 20th century was the discovery of the antibiotics, with penicillin and streptomycin being the earliest members of an increasingly large battery of pharmaceutical agents used to combat the spread of infection. The structure determination of penicillin using X-ray diffraction by Dorothy Hodgkin, a landmark in the development of X-ray crystallography, revealed the beta-lactam, a ring of four connected atoms, for the first time. Penicillin's successors, and there are many, are generically called beta-lactam antibiotics.

In the last few days the term 'beta-lactam' has appeared repeatedly in the Indian press as a result of a paper entitled, 'Emergence of a new antibiotic resistance mechanism in India, Pakistan and the UK: a molecular, biological and epidemiological study' (Kumarasamy, K. K. *et al.*, *Lancet Infection* online August 11, 2010, doi: 10.1016/S1473-3099(10)70143-2). These authors examine the prevalence of a gene encoding the sequence of an enzyme that breaks down beta-lactams, named as the New Delhi metallo-beta-lactamase-1 (NDM-1), in clinical isolates of bacteria. The presence of this gene (and the production of the enzyme beta-lactamase) permits bacteria to breakdown the beta-lactam antibiotic, rendering them resistant to the molecules designed to kill them. Resistance genes can be transferred through populations posing a public health hazard. There is little that is new or surprising in the *Lancet* paper. It is the conclusion that UK patients travelling to India for elective surgery (an interesting byproduct of globalization, which has added a new term to our vocabulary – medical tourism) are at risk of contracting drug-resistant, hospital-acquired infections, which has raised hackles. In a world dominated by commerce even scientific papers can be viewed with suspicion; especially in the medical arena where researchers, their journals and pharmaceutical companies often confront difficult, conflict of interest situations.

The phenomenon of antibiotic resistance was anticipated by Alexander Fleming, an inevitable consequence of Darwinian selection when bacterial populations are exposed to antibiotics (*Current Science*, 2008, **94**, 697). Several of the most successful agents in the fight against bacterial disease are molecules (secondary metabolites) produced by microorganisms; a curious example where both the enemy and the weapons to be used against them are products of nature. Synthetic molecules, the products of painstaking research in medicinal chemistry that have appeared on pharmacists shelves, have often been modelled on the structures of natural products with antibiotic properties. Why do microorganisms produce antibiotics, investing a great deal of genetic and metabolic effort? Are these secondary metabolites the true offensive weapons to be deployed in a battle for ecological space; elements in the 'survival of the fittest' scenario which so often intrudes into discussions of natural selection. Do bacteria (and other microorganisms) wage a sophisticated form of 'chemical warfare' as they battle over environmental niches? Defensive strategies to overcome an antibiotic onslaught are an inevitable biological consequence. Drugs, antibiotics among them, can be extruded from target cells by 'pumps'. Drug targets, most often proteins, can be altered rendering them impervious to the molecules flung at them. Even more directly, drug molecules can be chemically degraded by an enzyme that is geneti-

cally encoded in resistant bacteria. The beta-lactamase, now indelibly linked to New Delhi, is an example. It is the name which appeared to attract a great deal of adverse reaction in the popular press in India. The first report describing this gene in an isolate of *Klebsiella pneumoniae* from 'a Swedish patient of Indian origin' who had been 'admitted to a hospital in New Delhi' appeared in December last year (Yong, D. *et al.*, *Antimicrobial Agents and Chemotherapy*, 2009, **53**, 5046). These strains were shown to be resistant to the carbapenems, which at present are the last line of defense against many Gram negative, opportunistic bacteria that are unaffected by most commonly used antibiotics. The new antibiotic pipeline is far from being reassuring, raising the serious concern that bacterial resistance may soon approach alarming proportions. This report which, introduced the name New Delhi beta-lactamase, noted that 'the broad resistance carried on these plasmids is a further worrying development for India, which already has high levels of antibiotic resistance'.

Even before this detailed study on NDM-1 appeared, an alert had been issued in the UK, warning of the appearance of bacterial strains which seemed resistant to carbapenems (*Health Protection Report*, vol. 3, No. 26, July 3, 2009). The report stated clearly that 'a further metallo-beta-lactamase type – designated NDM-1 ("New Delhi Metallo-1") – is swiftly emerging. . . . Critically, at least 9/19 affected patients have had recent hospitalization in India or Pakistan'. This report also goes on to note that 'carbapenems are widely available in the Indian subcontinent, are widely used owing to prevalent cephalosporin resistance, and have doubtless exerted selection pressure'. There is also a mention of the fact that 'the population flow between the UK and the Indian subcontinent is larger (than other countries) with some elective medical tourism'. In an ominous note the report warns that 'most isolates with NDM-1 enzyme are resistant to all standard intravenous antibiotics for treatment of severe infections'. The UK report was followed remarkably quickly, in March, by a study from the P.D. Hinduja Hospital, Mumbai entitled: 'New Delhi Metallo-beta-lactamase (NDM-1) in Enterobacteriaceae: Treatment options with carbapenems compromised' (Deshpande *et al.*, *Journal of Association of Physicians of India* (*JAPI*), 2010, **58**, 147). This study reports that of 24 carbapenem-resistant isolates, collected over the period August–November 2009, 22 were indeed NDM-1 producers. The authors draw attention to earlier work which suggests the possibility of using non-carbapenem antimicrobials in many situations, helping to 'preserve the efficacy of carbepenems against increasingly resistant organisms'. The *JAPI* report suggests that the spread of NDM-1 producing organisms 'may endanger patients undergoing major treatment at centres in India and this may have adverse implications for medical tourism'. Indeed the authors of the *Lancet* paper in August 2010 appear to have gone further, presumably due to 'calls in the popular press for UK patients to opt for corrective surgery in India with the aim of saving the NHS money'. They argue that the data presented in the *Lancet* paper show that this 'might ultimately cost the NHS substantially more than the short-term saving' and 'strongly advise against such proposals'. While the authors of the *JAPI* paper worry about the adverse impact of NDM-1 on medical tourism, the *Lancet* authors warn 'tourists' away from undergoing hospitalization in India. Clearly, the furore in the media is a consequence of the greater 'impact' of the journal *Lancet* and its undoubtedly superior ability to ensure that papers likely to engage public attention are quickly released to the media. Despite the sensitivity about the propriety of naming the offending beta-lactamase after the city of Delhi and the suspicion that commercial interests may be involved in promoting studies that adversely affect the medical tourism industry, the problem of antibiotic resistance must be taken seriously by the community of physicians.

In an editorial that appears in the same issue of *JAPI* as the paper from the Hinduja Hospital, K. Abdul Ghafur (*JAPI*, 2010, **58**, 143) highlights the crisis in the management of infectious disease in India. His commentary is strongly worded, undoubtedly hoping to provoke a somnolent medical community into action. He notes: 'There is no restriction on the use of higher end antibiotics in India. Indian doctors need not justify their prescription. Any doctor can prescribe and in some cases even pharmacists can dispense without prescription, meropenem in a situation where ampicillin would have been adequate and at the same time prescribing ampicillin in a case where meropenem would have been the right choice'. Ghafur's may be a cry in the wilderness, since testing for antibiotic sensitivity of clinical isolates may hardly be an option in many situations. Microbiology is a discipline that is fundamental to the understanding of infectious disease. It is hardly an important subject in the medical curriculum in most institutions. Ghafur may touch many a raw nerve when he says: 'Indian medical community has to be ashamed of the NDM-1 ('New Delhi Metallo-1') gene. Even though we have not contributed to carbapenem development, we have contributed a resistance gene with a glamorous name'. The antibiotic pipeline in pharmaceutical laboratories is nearly dry. Bacteria have been quick to adapt and neutralize the molecules thrown indiscriminately at them. Holding the line in the war against infectious disease, in a globalized world, will need renewed focus on public health, hygiene and medical education, in addition to a concentrated effort to discover new antibiotics and beta-lactamase inhibitors. The prospect for an outright victory in the war against bacterial pathogens is very bleak. It will be a continuing struggle, in which nature sides with the enemy.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Trouble With Science

The title of this column is adapted from a book that came my way a few days ago: *The Trouble With Physics*, by Lee Smolin (Houghton Mifflin Co., USA, 2006; Penguin, 2008). My attention was immediately drawn to the Penguin edition, because the title was printed upside-down; undoubtedly a marketing strategy thought up by the publishers. My physics is weak, at best, but the subtitle 'The Rise of String Theory, the Fall of a Science and What Comes Next' seemed irresistible. The very first page did not disappoint. 'There may or may not be a God. Yet there is something ennobling about our search for the divine. And also something humanizing which is reflected in each of the paths people have discovered to take us to deeper levels of truth. Some seek transcendence in meditation or prayer; others seek it in service to their fellow human beings; still others, the ones lucky enough to have talent, seek transcendence in the practice of art. Another way of engaging life's deepest questions is science. Not that every scientist is a seeker; most are not. But within every scientific discipline there are those driven by a passion to know what is most essentially true about their subject. If they are mathematicians, they want to know what numbers are, or what kind of truth mathematics describes. If they are biologists, they want to know what life is, and how it started. If they are physicists, they want to know about space and time, and what brought the world into existence. These fundamental questions are the hardest to answer and progress is seldom direct. Only a handful of scientists have the patience for this kind of work. It is the riskiest kind of work, but the most rewarding: When someone answers a question about the foundations of a subject, it can change everything we know.' Smolin's book is an exceptionally engaging discourse on subjects that require a deep understanding of physics and which have been illuminated in the past by some of the most penetrating insights in human history.

For readers, generally interested in science, Smolin's style is bound to be attractive; although professional theoretical physicists may be less than enthusiastic about his effort to present a strong critique of the way the discipline has moved in the last quarter of a century. He begins by noting: 'The most cherished goal in physics, as in bad romance novels, is unification. To bring together two things previously understood to be different and recognize them as aspects of a single entity – when we can do it – is the biggest thrill in science' (p. 18). He cites two examples. The first is Giordano Bruno's recognition that 'the sun is just another star – and the stars are just suns that happen to be very far away', which led to his burning at the stake as a heretic. The second is Charles Darwin's idea of natural selection which points inexorably towards a common ancestry of living organisms. Smolin notes: 'Biology before Darwin and biology afterward are hardly the same science'. In setting the stage for his survey of the unified theories that have for so long been a goal of physics, Smolin reminds us: 'Great unifications become the founding ideas on which whole new sciences are erected. Sometimes the consequences so threaten our worldview that surprise is quickly followed by disbelief' (p. 19). How do new theories gain acceptance? There must, of course, be 'new insights and hypotheses' which then act as 'the engine that drives progress in understanding'. Most importantly a new unifying theory in science must make firm predictions which must be confirmed by experiment. While observations, both in the natural world and the laboratory, provide the basis for formulating theories, these in turn must point the way towards new experiments that advance our understanding and provide a test of the predictive power of a theory.

Smolin illustrates the power of unification beginning with what he terms as 'the greatest unification in all of science: the *unification of motion and rest*'. The beginnings of modern science are indeed traceable to the revolution, begun by Copernicus and Galileo and decisively completed by Newton, in which our basic Aristotelian instincts are suppressed. Newton's first law, drilled into generations of students, implicitly advances the notion 'that whether a body is moving or not has no absolute meaning. Motion is defined only with respect to an observer, who can be moving or not'. Smolin's assessment is engaging: 'Galileo and Newton achieved here a beautiful intellectual triumph. To others, it was obvious that motion and rest were completely different phenomena, easily distinguished. But the principle of inertia unifies them. To explain how it is that they seem different, Galileo invented the *principle of relativity*.' It is the emphasis on the insights that come from thought alone that is at the core of Smolin's writing. In developing his theme of the foundational role of unification he notes: 'In the history of physics there is one unification that serves more than any other as a model for what physicists have been trying to do for the last thirty years. This is the unification of electricity and magnetism achieved by James

Clerk Maxwell in the 1860s.' Those who are confronted by the concepts of forces and fields, in their brushes with science, may find some reassurance in Smolin's words: 'Thus a field can carry a force from one body to another. There is no need to believe in ghostly action at a distance.' Michael Faraday's work on electricity, magnetism and their interrelatedness stands as one of the major triumphs of the 19th century. Maxwell's unification was the stepping stone to Einstein's special theory of relativity, 'born, as a joining of Galileo's unification of rest and motion with Maxwell's unification of electricity and magnetism'. This achievement in 1905 happened at a time when 'the mechanists had the right equations, they just had the wrong interpretations' (p. 37).

In a short and beautifully written chapter entitled 'The world as geometry', Smolin outlines the remarkable road that Einstein took in unifying 'gravity with relativity' reiterating the importance of the unification of motion and rest traceable to Galileo. At the heart of this remarkable feat of thought that led to general relativity lay other unifications: 'So Einstein succeeded in unifying all kinds of motion. Uniform motion is indistinguishable from rest. And acceleration is no different from being at rest with a gravitational field turned on.' The unification of the gravitational field with the geometry of space and time, the realization that 'the presence of matter affects the geometry of space' was to be one of the defining events of 20th century physics. Smolin turns to the gap between gravity and electromagnetism, a subject that had already begun to attract many physicists in the second decade of the 20th century. He describes Einstein's reaction to a theoretical construct of Herman Weyl: 'Apart from the [lack of] agreement with reality it is in any case a superb intellectual performance'. This is a situation that often surfaces in Smolin's account; mathematically elegant, often compellingly beautiful theories, which seem to be imperfectly grounded in the prevailing view of reality. The unification of quantum mechanics and electromagnetism, a mid-20th century triumph and the construction of a theory to unify electromagnetic and weak nuclear forces are in Smolin's account landmarks in the development of 'unification as a science'. It is here in the years that followed the 'standard model of particle physics' that Smolin introduces us to one of the predictions that 'grand unification' approaches made – that protons must fall 'apart into simpler things'. As Smolin tells the story: 'Funds were raised, and huge tanks were built in mines deep underground. The results were impatiently awaited. After twenty five years we are still waiting. No protons have decayed . . . . It's a beautiful idea, but one that nature seems not to have adopted' (p. 64).

In a chapter titled 'Preparing for a revolution' Smolin is reflective: 'Sometimes scientific progress stalls when we encounter a problem that just cannot be solved in the way we understand it. There is a missing element, a different sort of trick involved. No matter how hard we work, we won't find the answer until someone somehow stumbles on this missing link'. These simple words seem to capture the romance and adventure of science in a way that almost seems at odds with the frenetic and apparently purposeful march of modern science. He illustrates the 'missing link' using the explanation of eclipses. He goes on to add: 'The idea that elementary particles are not pointlike particles but vibrations of strings may be another of these rare insights. If right, it is as profound a realization as the ancient discovery that the circles the planets travel on are themselves moving' (p. 102). He traces the origins of string theory in the late 1960s and early 1970s and its relatively slow acceptance until the mid 1980s. In his words: 'Few of us realized that we were living in the last days of physics as we had always known it'. To the average reader this may seem an overstatement of his case. But for Smolin it is the foundational aspects of the discipline that are of concern; not the more mundane areas where physics has made deep inroads, primarily in areas that border chemistry and biology. He describes two string theory revolutions, noting that 'string theory initially proposed to unify all the particles and forces in nature'. It is here that he begins a forceful critique: 'But as it was studied in the decade following the 1984 revolution something unexpected happened. The alleged unified theory fractured into many different theories: the five consistent superstring theories in ten-dimensional spacetime, plus millions of variants . . . . As time went on, it became clear that string theory itself was in need of unification.' The attempts to provide the unifying frameworks are 'conjectures' not 'theories' for Smolin. As he describes it, the acceptance by a community of 'the existence of a landscape containing a vast number of theories based on much less evidence than we needed twenty years ago to convince ourselves that a single theory existed' (p. 160), is a matter that merits discussion. The development of string theory, in Smolin's analysis, seems to have led to a situation where theories that make no falsifiable predictions are legitimate. Its most visible proponents, Steven Weinberg among them, argue that 'we may be at a new turning point, a radical change in what we accept as a legitimate foundation for a physical theory' (p. 168).

Smolin describes recent theoretical and experimental work which he believes has 'already inaugurated the post-string era in fundamental physics. In all the frontier fields – quantum gravity, foundations of quantum mechanics, elementary-particle physics and cosmology – bold new ideas are evolving in tandem with experiments. These initiatives have to be nourished or they'll die on the vine, but they show great promise'. It is in his final section that Smolin draws attention to the growing stranglehold of string theorists over academic theoretical physics in a chapter titled 'How do you fight sociology?'. Smolin raises questions relevant for many other fields of science, where communities in specialized areas grow to exercise a disproportionate influence on a broad discipline. Fashions can sometimes dominate the development of science. That, indeed, may be the trouble with modern science.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science: Pressures of a Changing Age

The global economy is driven by competitive pressures. A recent editorial in *Science*, that addresses the issue of 'Nurturing Young Scientists' in the United States, proclaims: 'It is imperative to grow our economy through global leadership in science'. The editorial highlights the need to nurture and mentor early career scientists and the importance of recognizing the need to 'affirm and reward cross-boundary collaborations'. The American research university system has long been envied in the developing world. It is almost impossible to attend any gathering of science policy makers in India without hearing of the role models in graduate education and research – Harvard, MIT, Caltech, Berkeley, Columbia and the list goes on. In the drive to stay ahead of the competition, the US National Academies have begun to examine 'ways to keep research universities healthy'. There is a real concern that the future demands change. In their editorial essay, J. Gentile and S. Boehlert (*Science*, 2010, **329**, 884) argue that frameworks must be created 'in which interdisciplinary research and synergistic teaching can thrive'. Even in the best of places, interdisciplinary work can sometimes be difficult for young researchers, constrained by conservative and tradition-bound mechanisms of peer assessment. The *Science* editorial recognizes the need to redefine 'what constitutes original and independent contributions to a research agenda that involves multiple partners and rewarding those who forge robust interactions among groups'. In words that need to be read carefully in our own institutions, the authors note: 'Most universities have a department-centric organization. Each department's faculty judges its peers according to the norms of their own discipline, a perspective that makes it difficult for interdisciplinary faculty to receive a fair hearing'. While issues of tenure are not a matter of concern in the overwhelming majority of Indian institutions, the importance of promoting cross-disciplinary faculty is still to be recognized, during the process of recruitment and in the decision making process on research grants. Departmental and disciplinary boundaries are rarely violated in India, placing our institutions in a poor position to exploit research opportunities that arise at the interfaces of traditional subjects.

The *Science* editorial points out that in the United States 'early career professors often seek to integrate their research and teaching, eager to bring the vitality of their own science into the classroom to engage students'. They add, undoubtedly, with a tinge of regret: 'Unfortunately, great teaching skills are still considered to be of secondary value to career success at research universities'. To bolster the case for teaching as a critical component for ensuring the health of academic institutions, they quote from Carl Wieman's testimony to the US Senate: 'To maintain US economic competitiveness and leadership in innovation, we need to also have leadership in STEM [science, technology, engineering and mathematics] education'. Are these issues relevant to the Indian situation? They must be, if we are to ensure that the ongoing expansion of science education and research is to catalyse a real process of change. A very substantial part of India's investments in scientific research focuses on national laboratories, highly specialized institutions sometimes mandated to work in areas whose current relevance may be less than optimal. The intersection of these institutions with the educational process is negligible. The creation of many small research institutions, with a limited range of subjects of study appeared to be attractive some years ago. In the present context it might be wise to reexamine the virtues of size. Should the science and technology system in India consider a strategy of mergers and acquisitions; an approach that seems to have been so successful in the corporate world?

In addressing the issue of a transformational agenda for science, Gentile and Boehlert talk of the need for 'building a critical mass of individuals for long-term cultural change' within science departments in universities. Interestingly, this analysis was followed by yet another editorial in *Science*, which reflected on 'China's research culture'. The advance of China in recent years is a phenomenon that is widely admired. Chinese science, as measured by the conventional metrics of publication statistics, is moving rapidly forward, threatening to overshadow the advanced countries of the West. Yigong Shi and Yi Rao, two senior academics in Beijing note that, 'government research funds in China have been growing at an annual rate of more than 20%, exceeding even the expectations of China's most enthusiastic scientists. In theory, this could allow China to make truly outstanding progress in science and research. . . . In reality, however, rampant problems in research funding – some attributable

to the system and others cultural – are slowing down China's potential pace of innovation' (*Science*, 2010, **329**, 1128). This frank assessment will find a sympathetic Indian readership, which grapples with similar 'cultural problems'. In discussing funding for large projects, the authors note: 'The key is the application guidelines that are issued each year to specify research areas and projects. Their ostensible purpose is to outline "national needs". But the guidelines are often so narrowly described that they leave little doubt that the "needs" are anything but national; instead the intended recipients are obvious. Committees appointed by the bureaucrats in the funding agencies determine these annual guidelines. For obvious reasons, the chairs of the committees often listen to and usually cooperate with the bureaucrats. "Expert opinions" simply reflect a mutual understanding between a very small group of bureaucrats and their favourite scientists. This top-down approach stifles innovation and makes clear to everyone that the connections with bureaucrats and a few powerful scientists are paramount. . . To obtain major grants in China, it is an open secret that doing good research is not as important as schmoozing with powerful bureaucrats and their favourite experts.' The 'culture', described in this frankly worded critique, 'even permeates the minds of those who are new returnees from abroad'. In words that might be equally applicable to many of India's best funded institutions, Yigong Shi and Yi Rao describe the situation in China: 'A significant proportion of researchers in China spend too much time on building connections and not enough time attending seminars, discussing science, doing research, or training students (instead, using them as laborers in their laboratories). Most are too busy to be found in their own institutions'. Their assessment of China's research culture is brutal: 'It wastes resources, corrupts the spirit, and stymies innovation'. Their call is worth heeding: 'The time for China to build a healthy research culture is now, riding the momentum of increased funding and a growing strong will to break away from damaging conventions'.

Asia's drive to catch up with the West in scientific research, by building new institutions, refurbishing old ones and by dramatically expanding investments in research and development, has been a subject of great interest, worldwide. In a commentary entitled 'Achieving scientific eminence within Asia', A. S. Huang and C. Y. H. Tan return to a, now, familiar theme: 'We sense that Asian countries share some aspects of mindset and culture that limit their aspirational reach. . . . After considerable investment in buildings and equipment, Asian countries have come to appreciate that more is needed to build a vibrant research culture that is capable of world-class competitive research' (*Science*, 2010, **329**, 1471). In sentences that need to be carefully read in India, the authors emphasize a regrettable aspect of the culture of science in Asia. 'Unfortunately, throughout Asia, the respect for scholarship has translated to a virtual worship of the Nobel Prize and branding opportunities offered by top universities. Nobelists and professors from well-known Western universities represent the ultimate intellectual authority. Huge sums of money have been and are being spent in the hope of fast-tracking scientific institutions in Asia by associating with famous universities abroad. . . . How can Asia nurture its own scientific leadership with such a lack of confidence in its own talent? If not corrected, intellectual colonialism can become a serious barrier to Asia's quest to build capacity in science.'

Coincidentally, the same issue of *Science* carries an editorial 'Young leaders for biology in India', which highlights government programs that facilitate 'repatriation' of Indian scientists trained abroad. India has not followed 'China's and Singapore's strategy of offering lavish resources to established US or European scientists to either move to or create outpost laboratories in Asia'. A mentoring program ('Young Investigators Meeting'), organized for Indian postdoctoral fellows in biosciences, who are considering returning to India is described. The concluding sentence of this editorial returns to the theme of 'culture': 'The best guarantee for future success is to imbue young scientists with a sense of mission, to nurture not just the science but a culture that fuels the will to succeed in countries long thought of as second choice options for research careers' (Tole, S. and Vale, R. D., *Science*, 2010, **329**, 1441). Many new schemes being thought up, especially in biology, seem to cast adrift the large numbers of young scientists trained in Indian laboratories, placing an undue emphasis on areas and problems that are approved by Western scientists. 'Social networking', increasingly facilitated by excessive travel to foreign laboratories, appears to influence the framing of research agenda's in India. The bogey of 'intellectual colonialism' raised by Huang and Tan may well prove inhibitory to progress.

Asia is not alone in worrying about a competitive future. Bruce Alberts, the former President of the US National Academy of Sciences and the Editor-in-Chief of *Science* has raised a concern about 'overbuilding research capacity' in the United States, in the area of biomedical research. In his words: 'The current trajectory is unsustainable, threatening to produce a glut of laboratory facilities reminiscent of the real estate bust of 2008 and, worse, a host of exhausted scientists with no means of support' (*Science*, 2010, **329**, 1257). Science, in Asia and elsewhere, is clearly facing the pressures of a changing age.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Citations, Impact Indices and the Fabric of Science

In the mid-1970s, when I began an independent research career at the Indian Institute of Science (IISc), the most formidable problem that confronted me was to find a topic to study and carry out, presumably original, research. For those trained in Western laboratories continuing a trail of research, that was exposed during doctoral or post-doctoral days, seemed formidably difficult. Finding problems that would uncover new trails required indiscriminate reading of the available literature in a broadly defined discipline. Journals would arrive after a delay time of about six months, having made the stately journey across the seas. Under these circumstances the best way to keep abreast of the literature was to carefully scan the contents pages of journals reproduced in *Current Contents*. Reprint requests, mailed to authors on cards that were decorated with colourful stamps, usually elicited a warm response; many authors would send along related papers that they had published. Both reprint and stamp collections grew; as did the bond with the postman who delivered these treasures. In these aggressive and frenetic times nostalgia is not an emotion which is wise to display, especially in academia by aging professors. But, I must succumb to the temptation having been reminded of Eugene Garfield's first visit to Bangalore in the mid-1970s, by an editorial that appeared in a Festschrift on his 85th birthday (Arunachalam, S., *Ann. Lib. Inf. Stud.*, 2010, **58**, 173). When I heard him in the Faculty Hall of IISc many of us knew Garfield as the founder of *Current Contents* and the author of the most readable essays on science and scientists that appeared in each issue. Those were the early years of 'scientometrics' and Garfield seemed to be ploughing a lonely furrow, talking about citations and impact factors. Information science seemed to be the province of professional librarians; most practising scientists (most certainly in India) still lived in the age of innocence, where they were unconcerned with the metrics of science.

Five years ago in a talk entitled 'The Agony and Ecstacy – The History and Meaning of the Journal Impact Factor', Garfield noted that he might have considered an alternative title: 'Citation Sanity and Insanity. . . The Obsession and Paranoia of Citations and Impact Factors' (*Int. Cong. Peer Review and Biomedical Publications*, Chicago, 16 September 2005). His 1955 paper introduces the term 'impact factor' for the first time and 'is considered the primordial reference for the concept of the *Science Citation Index*' (*SCI*; Garfield, E., *Science*, 1955, **122**, 108). Half a century later he notes: 'In 1955 it did not occur to me that "impact" would one day become so controversial. Like nuclear energy, the impact factor is a mixed blessing. I expected it to be used constructively while recognizing that in the wrong hands it might be abused. Since *Current Contents*, no less *SCI*, did not exist, it would have been precocious indeed to contemplate the influence of the nascent impact factor'. In the first 25 years after Garfield's seminal paper in *Science*, citations and impact factors seemed most valuable in identifying core journals in a field; a tool that librarians must use in devising the most cost effective strategy in building journal collections to serve their readers. The *SCI* seemed to be a tool of great interest to analysts of science (sociologists and historians amongst them) who would use graphs, networks and nodes that emerged from connecting scientific papers through citations and co-citations. Indeed in an early review of the *Science Citation Index*, J. D. Bernal noted: 'The publications of science effectively form a network of mutual reference which can be traced out from any particular point from which one chooses to start. It is a graph in the mathematical sense. The *Citation Index* is constructed so as to produce an almost infinite number of such graphs (Bernal, J. D., *Science Progress*, 1965, **53**, 455). In the early days the *SCI* was not widely available in India; the *Web of Science*, the internet and Thomson's promotional skills were still to arrive on the scene. Avid readers of Garfield's essays in *Current Contents* were beginning to be exposed to the many uses (and inevitable misuses) of citation data. He ranked journals and identified the most productive and well cited scientists in specific fields, displaying insights and judgements that were truly engaging. He spotted scientists of Nobel class aided by citation counts, networks and a growing knowledge of the scientific community. In 1998 he reflected on a 1965 study in which he noted that 'the average Nobel prize winner published five times the average author and were cited 30 times the average'. He draws attention to the fact that 'over 50% of the 1000 most cited scientists are members of the US National Academy of Sciences'. He quotes 'a former Academy president who told me "for every scientist elected to the Academy, there is another equally qualified who is not elected" '

(Garfield, E., *Scientometrics*, 1998, **43**, 69). Garfield's essays seemed to bring the world of science to life; at times unearthing information that allowed resurrection of scientific reputations. His discovery, in the mid-1980s, of Sambhu Nath De's work on cholera toxin led me to spend two years trying to produce a special section of this journal devoted to cholera research, a subject far from my own interests (*Curr. Sci.*, 1990, **59**, 623–716).

Scientometrics has transformed over the last two decades as the use of citation counts and impact indices has exploded. All of a sudden it seems there is little need to assess the work of scientists by reading what they write (and at times, listening to what they say). It appears sufficient to obtain an 'average impact factor' for a list of publications or to calculate an '*h*-index' or one of its variants. Indices permit the ready ranking of individuals, journals and institution. The accessibility of the *Web of Science*, *Scopus* and *Google Scholar* has led to an increase in the number of science analysts in India; many of whom seem to be unaware of the limitations of databases or the nature of the disciplines that they compare. Government agencies and academies now use citation counts in a manner that Garfield could scarcely have envisaged. Most often, comparisons are made at the low end of the spectrum of citation counts, leading to meaningless conclusions. Scientists seem to be succumbing to an obsession with the *h*-index and it is not uncommon to see various factors and indices listed on CVs. Discussions on methodologies for increasing journal impact factors and citation counts for individual papers abound in the literature of science. A *Nature News* report (Zoë Corbyn, 13 August 2010) describes an apparently 'easy way to boost a paper's citation'. The prescription is due to a study by Gregory Webster, a psychologist at the University of Florida, who suggests a correlation between the number of cited references in a paper and the number of citations received. Using a dataset of 53,894 articles published in *Science* between 1901 and 2000, Webster suggests that bloating the reference list in a paper, boosts the number of citations that it may receive. There is the provocative suggestion of 'reciprocal altruism'. Critics, and there are many, have been quick to point out that correlations can often be found between unrelated variables. While Webster's conclusion may sink into oblivion, other ideas for boosting citation counts will undoubtedly surface.

In a commentary on the 'state of scholarly publishing', Douglas Arnold, president of the Society for Industrial and Applied Mathematics draws a damaging conclusion: '. . . common bibliometrics – such as the impact factor for journals and citation counts for authors – are easily manipulated' (*Siam News*, 2009, **42**, 1). He argues that 'their use in ranking and judging should be curtailed'. Arnold's concerns are based on cases of both editor and journal misconduct. He cites the case of the journals *Chaos, Solitons and Fractals* (Elsevier) and *International Journal of Nonlinear Science and Numerical Simulation* (Freund Publishing), in which the editors 'publish copiously, not only in their own journals but also in each other's, and they cite each other frequently'. While author misconduct is not an uncommon phenomenon nowadays, it is likely that journals (and publishers) may also be tempted to promote dubious practices, in the drive to boost impact indices. The ever increasing emphasis on quantitative parameters in assessing individuals and institutions has been termed as a 'bad idea whose time has come'. In a critique of the UK Research Assessment Exercises, Colin Macilwain argues that we now see the 'social scientists' equivalent of the uncertainty principle'. He notes that 'such exercises influence the behaviour of the observed in unforeseen ways. Whatever is measured becomes emphasized, probably at the expense of whatever is not. And as metrics change – as they must to stop institutions gaming the system – the process loses simplicity, transparency and credibility' (*Nature*, 2010, **463**, 291). The tremendous influence of scientometrics on the conduct of science has been emphasized in a collection of articles in the 17 June 2010 issue of *Nature*. I was particularly struck by a short opinion piece by Tibor Braun, editor-in-chief of the journal *Scientometrics*. He says: 'Thanks in part to easy access to big, interdisciplinary publication and citation databases, . . . evaluative metrics can seem very easy to use. Because it is so easy to produce a number, people can be deluded into thinking that they have a thorough understanding of what those numbers mean. All too often, they don't know which database to use, how to clean raw data, which indicator to use or how to use it for the task at hand' (*Nature*, 2010, **465**, 870). Scientists, as a community, often worry about bad science; they might do well to ask hard questions about bad scientometrics.

I began this column with Garfield and I must end with him. What is Garfield's impact on science? Can it be measured in quantitative terms using citations and impact indices? P. Jasco sets out to answer these very questions, prefacing his study by describing Garfield as 'a contemporary renaissance man, a visionary, an indefatigable researcher, and a fabulous story teller'. Garfield's prodigious output of 'essays, commentaries and other editorials' account for about 70% of all his publications. Jasco concludes that 'Eugene Garfield has an *h*-index of 35. . . This is by far the highest *h*-index in the field of information and library science' (*Ann. Lib. Inf. Stud.*, 2010, **58**, 222). Can a single number measure Garfield's impact on the practice of science? Has not the impact of citation counting dramatically changed the way scientists practice their craft? Does not the bland statement of the *h*-index conceal more than it reveals? Garfield's creations have transformed the fabric of science. If not used wisely, they may indeed shred the fabric.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

In Vitro Fertilization: Success, Controversy and Tragedy

Reproduction is essential for survival of all biological species. The processes of fertilization, embryonic growth, differentiation and development are amongst the most dramatic phenomena in nature. They are also so common that they are hardly noticed. The fusion of a sperm cell with an egg can set off an amazing chain of events, that can lead to the birth of a wonderfully formed baby. The remarkable ease with which reproductive functions are fulfilled in nature and the widespread familiarity with the processes of conception and birth have obscured the importance of the field of reproductive biology. The award of the Nobel Prize in Physiology or Medicine to R. G. Edwards, has turned the spotlight, albeit briefly, onto a field that appeared to have faded from centrestage, a long time ago. Ever since the famous and ominous prophecies of Malthus, in the 19th century, contraception research has dominated the field. Indeed, when Edwards began his work on fertilization under laboratory conditions, in test tubes or more appropriately petri dishes (*in vitro* in the parlance of the field), there was little to suggest that human life might indeed be conceived in prosaic, laboratory conditions by sperm–egg fusion. In a paper published 45 years ago, Edwards noted that 'the investigation of early development in many mammalian species is restricted by the difficulty of obtaining sufficient numbers of oocytes and embryos at particular stages of development'. In introducing a technique to facilitate *in vitro* maturation of 'mouse, sheep, cow, pig, rhesus monkey and human ovarian oocytes', he looked ahead: 'With this technique it should eventually be possible to study pre-ovulatory development and perhaps fertilization and pre-implantation development in various species including man' (Edwards, R. G., *Nature*, 1965, **208**, 349). The same year in reporting 'maturation *in vitro* of human ovarian oocytes' he pointed out, that 'perhaps the greatest challenges of the present work lie, however, in the prospect of obtaining fertilized human eggs'. The concluding sentences of this report look into the future: 'Nevertheless, the plentiful supply of oocytes from one ovary could ultimately allow us to grow human embryos *in vitro*, and even control the genetic disorders of man' (Edwards, R. G., *Lancet*, 1965, 926–929). Four years later, in reporting on the 'early stages of fertilization *in vitro* of human oocytes matured *in vitro*', Edwards *et al.* clearly outline a clinical use of human embryos: 'Fertilized human eggs could be useful in treating some forms of infertility, and many infertile patients will probably be older women' (Edwards, R. G., Bavister, B. D. and Steptoe, P. C., *Nature*, 1969, **221**, 632). In August 1978 a very brief note entitled 'Birth after the reimplantation of a human embryo' appeared in *Lancet*. The text was technical, matter of fact and barely more than half a column. The procedure for producing the world's first 'test-tube baby' was summarized in one sentence: 'Pregnancy was established after laparoscopic recovery of an oocyte on 10 November 1977, *in vitro* fertilization and normal cleavage in culture media, and the reimplantation of the 8-cell embryo into the uterus 2½ days later'. The much celebrated baby 'was safely delivered by caesarean section on 25 July 1978' as 'a normal healthy infant girl weighing 2700 g' (Steptoe, P. C. and Edwards, R. G., *Lancet*, 1978, 366). Reading the stark, economical prose of the Steptoe–Edwards note, over three decades and four million babies later, one cannot help wondering how many lives have been touched by this scientific advance.

When the Nobel Prize was announced, Patrick Steptoe (1913–1988) had already passed on over two decades earlier. As the gynaecologist and surgeon, Steptoe was a key contributor to the success of the *in vitro* fertilization (IVF) procedure. Obtaining human eggs would have proved a major obstacle if Steptoe had not pioneered the use of laparoscopy, a technique invented by Raoul Palmer in France and Hans Frangenheim in Germany in the 1950s. Highlighting Steptoe's work, a decade after his death, Grzegorz Litynski relates an anecdote on laparoscopic sterilization: 'I can't do it (sterilization) often enough in France as it's a Catholic country', confided Raoul Palmer to Steptoe in private conversation. 'And Hans Frangenheim also has difficulties because he's working in a Catholic region of Germany. But you Patrick, live in England, and you could work unimpeded' (*JSLS: Journal of the Society of Laparoendoscopic Surgeons*, 1958, **2**, 99). In 1967 Steptoe published the first textbook, *Laparoscopy in Gynaecology* which was to influence medical practice. In Litynski's words: 'When laparoscopic sterilization was introduced a few years later on a large scale in the United States, Steptoe's book became a standard text. In a sense, his work served as a bridge between the European scientists and the expanding market in North America' (p. 100). Soon after, the Steptoe–

Edwards collaboration began in 1968, leading a year later to the first demonstration of fertilization of human oocytes *in vitro* in 1969. The birth of the first 'test-tube baby' was to happen nine years later. During this period Steptoe and Edwards were resolute in their effort to achieve 'human birth after *in vitro* fertilization' despite the absence of funding from public sources. The historical account, 'Why the Medical Research Council refused Robert Edwards and Patrick Steptoe support for research on human conception in 1971', must be read by all those who are interested in the processes of decision making, when uncharted territories are explored and when troubling ethical concerns surface in public debate (Johnson, M. H. *et al.*, *Human Reproduction*, 2010, **25**, 2157). The authors adopt a tempered view in their retrospective. They note that many accounts of the controversies of the 1970s 'contain important elements of truth: Edwards and Steptoe were outsiders and did pioneer—against prevailing wisdom—new ideas, therapies, values, public discourses and ethical thinking. But the standard histories also risk promoting an unduly simple view of how such decisions work'.

Discussions of ethical issues were always central to any consideration of *in vitro* fertilization in the 1970s. There were also major criticisms about the use of laparoscopic techniques. In 1989, Edwards described the trials of the 1970s in a memorial tribute to Steptoe. His essay entitled 'Tribute to Patrick Steptoe: beginnings of laparoscopy' is probably one of the most readable accounts of the road travelled to produce the first 'test-tube baby'. Theirs appears to have been a perfect collaboration, cemented by shared goals, complementary skills, total committment and similar views on the difficult ethical questions raised by their work. Edwards describes Steptoe: 'He never was a man to publish vast numbers of papers, and I soon learned how I would have to pick up my pen and write our joint articles throughout our time together!'. In his moving tribute Edwards asks: 'Who else I wonder, would have helped to conceive Louise Brown in the circumstances existing in Oldham in the 1970s?' (*Human Reproduction*, 1989, **4** (Suppl.), 1–9). Edwards willingly engaged in public discussion on ethical issues raised by the move towards *in vitro* fertilization. In an article published in 1971, Edwards and D. J. Sharpe, a lawyer, discuss 'social values and research in human embryology' (*Nature*, 1971, **231**, 87). Reading this article, nearly four decades after its publication, I could not but marvel at its direct relevance to many issues that we confront in areas where scientific advances 'catch unprepared a society that lacks either ready made attitudes or the institutional means of forming new ones'.

In vitro fertilization and 'test-tube babies' are no longer subjects that attract debate. In being reminded by the Nobel award of another time and another age, I cannot but return to the tragic story of the circumstances that surrounded the birth of India's first 'test-tube baby' on 3 October 1978, a few months after the success at Oldham. The procedure used by Subhas Mukherjee (1931–1981) and his collaborators used protocols distinct from those employed by Edwards and Steptoe. Mukherjee's report of his successful use of '*in vitro* fertilized frozen-thawed human embryo' to achieve successful implantation and uneventful pregnancy did not bring him any accolades. Instead, his experiment invited the unwelcome attention of an inquiry committee appointed by the Government of West Bengal. His subsequent harassment by bureaucratic agencies and his transfer to an institute of ophthalmology, a subject far from his area of specialization led to his suicide in July 1981. The story of Subhas Mukherjee has been kept alive in Kolkata by his colleagues and admirers, but it is not a story that is widely known. In 1997, T. C. Anand Kumar, published in this journal, a remarkable account of the 'architect of India's first test-tube baby' (*Current Science*, 1997, **72**, 526). Anand Kumar (1936–2010) who was widely credited with being responsible for the birth of India's first test-tube baby in 1986, took the unusual step of studying Mukherjee's notes and papers to reach the conclusion that this achievement must be recognized, albeit posthumously. Anand Kumar died a few months ago and an obituary notes that 'such generosity and honesty is a rare attribute' (Mehta, R. H., *Current Science*, 2010, **98**, 569). The critical step was 'the successful cryopreservation of an 8-cell embryo, storing it for 53 days, thawing and replacing it into the mother's womb, resulting in a successful and live birth as early as 1978 – a full five years before anyone else had done so'. He published his work 'in an obscure journal' (Mukherjee, Subash, Mukherjee, Sujit and Bhattacharya, S. K., *Indian Journal of Cryogenics*, 1978, **3**, 80). As Anand Kumar notes, quoting a WHO report: 'Embryo cryopreservation has now become a routine adjunct to IVF procedures and various methods of freezing are employed'.

Mukherjee did his work under difficult conditions in an unresponsive and indifferent environment, which turned hostile when he reported success. His work was done when contraception research was strongly supported. 'Family planning' was a popular slogan; 'family welfare' was to be invented after the forced sterilization excesses of the Emergency years (1975–1977). Mukherjee's work was carried out in the backdrop of those difficult years. Ironically, the apparent freedom from an oppressive government that followed the lifting of the Emergency, had little or no effect on the heavy hand of officialdom, that eventually drove Mukherjee to take his own life. This year's Nobel award in medicine is a tribute to resolute experimenters who have achieved difficult goals in the face of great public opposition. The story of *in vitro* fertilization is one of great drama, controversy and sadly, tragedy.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science and Medicine: Building an Interface

'Translational research' is a popular term, used with a stamp of authority by those who formulate research policy. In simple terms, the phrase appears to describe the process needed to transform the research output of academic research laboratories into useful products, procedures and services. In the sphere of research 'translation' can be both difficult and uncertain, often facilitated by individuals with uncommon attributes. While translation is a word that can, in principle, be applied in any branch of science, it is most often used in the context of modern biological research. No field of science has grown as rapidly, spreading its tentacles in all directions, as biology. Indeed the common public perception of biology, fostered during years of school, as a boring, descriptive subject may change rapidly as there is greater dissemination of the remarkable progress made over the past fifty years. The advances in biology directly impact two major areas of broad public concern, medicine and agriculture. Over the past twenty years or so, the loose use of the term 'biotechnology' has obscured the view of the broad discipline of biology in India. The growth of departments of biotechnology in colleges and universities, alongside departments of botany, zoology, life sciences and microbiology must surely puzzle outsiders, who attempt to grasp the meaning of biology. 'Translation' when applied to biological research undoubtedly refers to exploitation of results in medicine and agriculture. Biomedical research in the West has been a booming enterprise for over three decades. The budgets of the National Institutes of Health (NIH) in the United States have grown dramatically in this period and scientists in other disciplines, chemistry, physics and engineering among them, have drifted in substantial numbers to work on interdisciplinary problems, that successfully attract NIH support. The growth of the biomedical enterprise in the West has also been fuelled by the progressive increase in funding for these areas by private foundations, the Howard Hughes Medical Institute and the Wellcome Trust amongst them. Western institutions, most notably in the United States, have built a strong tradition of clinically oriented research, often spearheaded by physician-scientists, who sometimes sport both MD and PhD degrees. The biomedical enterprise in the United States has undoubtedly benefitted by the association and, often, colocation of medical schools and clinical research facilities within the environment of major research universities. Medical school entrants have already received a broad undergraduate education, setting them apart from medical graduates in India, whose exposure to other disciplines is limited to the years in high school. The US National Institutes of Health, which viewed from afar, has a mind-boggling $31 billion budget boosted, curiously enough in the recession, by an additional injection of $10.4 billion provided under legislation entitled the 'American Recovery and Reinvestment Act' (Wadman, M., *Nature*, 2010, **466**, 808). NIH has played a central role in the expansion and successes of biomedical research over the last few decades. In sharp contrast to the West, the gulf between science and medicine has been almost unbridgeable in India; 'two cultures', a phrase made famous by C. P. Snow, which appear to have evolved over the years with little in common.

In thinking about medical education, I was drawn to a marvellous account of the life of William Harvey (1578–1637), who discovered the circulation of blood. The author, Andrew Cunnigham, writes: 'This was, and still is, simply the most important discovery about the anatomy and physiology of the human and animal body that has ever been made' (*Cambridge Scientific Minds* (eds Harman, P. and Mitton, S.), Cambridge University Press, 2002, pp. 21–35). In this essay, written for a volume celebrating Cambridge University's long and distinguished history, Cunnigham asks whether Harvey's early education at Cambridge over a period of six years (1593–1599) contributed in any manner to his development 'as the great anatomical researcher he turned out to be'. He refers to a speech after Harvey's death, which pays a great tribute to Caius College where Harvey 'drank in philosophy and medicine from the purest and richest spring of all, if there be such another dedicated to Apollo in the British Isles'. From Cambridge, Harvey journeyed to Padua, 'the greatest nursery of medicine in its day', where he was influenced by the anatomical research and teaching of Fabricius. What are the factors that shaped Harvey; Cambridge or Padua? Cunnigham answers: 'For these, [Harvey's ground breaking researches] Padua was exclusively responsible. However, by the chances of his education – first The King's School and then Caius College – Harvey was steered towards the priceless innovative teaching of Fabricius in Padua'. Over the centuries

inspirational teachers have sown the seeds of discovery in the minds of gifted students. In medicine and in science, teaching and research merge seamlessly in environments that promote 'translation' and 'innovation'. This is a conclusion that is not often appreciated and, indeed, sometimes vigorously contested in the many discussions of the future of education and research in science and medicine in India today.

The contributions of basic scientific research to the advance of medicine throughout the 20th century are evident. Unfortunately, there seems to be little appreciation of the need to integrate science and medicine in India. The turf battles between the Ministries of Health and Human Resource Development will ensure that the emphasis on regulation and control dominate discussions of medical education and research. In thinking of examples of advances and discoveries in unrelated fields that have impacted medicine, I realized that the earliest Nobel prizes in physics recognized serendipitous discoveries that have revolutionized medical practice. In 1895, Wilhelm Röntgen observed that the 'impact of cathode rays on the glass vacuum tube was generating a new kind of invisible ray'; rays which made a distant fluorescent screen glow. Röntgen rays had 'extraordinary penetrative power'; a property that led to the birth of imaging in medicine. Röntgen famously despatched 'X-ray photographs of his wife's hand which produced an almost ghoulish image that clearly showed her bones and wedding ring' to several prominent physicists in Europe. This action soon resulted in a situation where 'all hell broke loose.' (The Nobel Prize in Physics 1901; Nobelprize.org). Henri Becquerel followed up on this discovery: 'At the beginning of 1896, on the day that news reached Paris of the experiments of Röntgen... . I thought of carrying out research to see whether all phosphorescent material emitted similar rays. The results of the experiments did not justify this idea but in this research I encountered an unexpected phenomenon... . Of all the phosphorescent materials, uranium salts seemed particularly suitable for this investigation... . I found that the uranium salts had emitted rays which produced the silhouettes of the crystalline sheets through the black paper and various screens of metal or thin glass laid on the plates.' (A. H. Becquerel Nobel Lecture 1903). This, of course, was the discovery of radioactivity. The Nobel prizes in Physics in 1901 and 1903, two of the earliest awards, recognized discoveries that have had an immeasurable impact on medicine and many other areas of science. Researches in physics and chemistry that have transformed medical practice can also be illustrated by more recent examples. Three dimensional imaging in medicine became a reality with the invention of computed tomography (CT), which permitted image reconstruction, resulting in the award of the 1979 Nobel Prize in Medicine to G. H. Hounsfield, an electrical engineer, and A. H. Cormack, a physicist. In his Nobel lecture Hounsfield had begun to realize that NMR imaging, then in its infancy, would indeed prove to be even more versatile in its application to medical problems. A dramatic illustration of the enormous dividends that may accrue to medicine from research in apparently esoteric fields is provided by the development of magnetic resonance imaging (MRI). Paul Lauterbur's Nobel lecture in 2003 is entitled: 'All Science is Interdisciplinary – from Magnetic Moments to Molecules to Men'. There cannot be a more succinct summary of the progression of scientific advance across disciplines. Indeed in the area of magnetic resonance the earliest Nobel Prizes were in physics, followed decades later by two in chemistry, progressing almost inevitably to the 2003 award in medicine. At the very beginning of his work in the early 1970s, Lauterbur clearly recognized the potential of NMR imaging in studying the human body. However, his first experiments resulted in an image of two 1 mm glass melting point capillaries filled with ordinary water (H_2O, which provided the signal from hydrogen nuclei), placed inside a 5 mm glass tube filled with heavy water (D_2O). This technique of imaging first named as 'zeugmatography' (a word with classical Greek origins), was to grow very rapidly into MRI, an enormously sophisticated, but nevertheless indispensable, diagnostic tool in hospitals. The seeds of basic science can indeed yield succulent fruit in the field of medicine. The history of MRI (and indeed many areas of successful science) is best summarized in an old American saying: 'From little acorns, tall oaks grow'.

Are there any lessons to be learnt from experiences elsewhere for the biomedical research enterprise in India? The rise of the corporate hospitals and private clinical facilities (euphemistically called 'nursing homes') has substantially eroded the influence of 'teaching hospitals', which indeed are best suited to create an ambience of research. The limitations in the number of medical students pursuing MD degrees, automatically restrict the flow of clinically trained researchers into fields which lie at the borders of science and medicine. The 'stand alone' nature of the best of our clinical research centres provides little scope for interactions among scientists, engineers and clinicians. The efforts at translation, well intended and undoubtedly required, need more than financial resources and new institutions. They need concerted attempts to transform undergraduate medical education and to create opportunities for students and young clinicians to be exposed to the broad sweep of science that has transformed medicine over the past century. The physician-scientist is an extremely rare species in India. The task ahead in medical research and education in India will be to create a new generation of clinical researchers, who will taste the excitement of science. One can only hope that they will effect a transformation that will place biomedical research in India, centrally, at the confluence of science, engineering and medicine.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Chemicals, Chemistry and Molecules

Judicial pronouncements have an air of finality. In India, we now turn to an overburdened judicial system to resolve failures of governance in everyday life; ranging from major national issues to minor and often, petty local controversies. Lawyers thrive in an intensely litigious environment. Courts oversee investigations into an ever increasing number of scams and misdeeds, by those who have been charged with the responsibility to govern. If one adds the vast number of criminal and civil cases that require judicial attention, we cannot but marvel at the diversity of problems that must confront judges. Judgements on matters of great public interest are awaited with considerable expectation, with press coverage that heightens interest. Judgements on highly arcane matters rarely evoke any mention. I was therefore, nonplussed to read a news item announcing that the Bombay High Court had ruled that 'steam was not a chemical'. The status of steam was a contentious issue, with a company manufacturing sulfuric acid, selling steam as a by-product. The issue before the court was not a judgement on chemistry, but one of interpreting tax laws. There is nothing more bewildering than regulations, periodically promulgated and frequently modified, which govern the imposition of taxes on materials and services. The interpretation of laws can be debated, but in this instance the judges were in no doubt: 'In common sense, the steam is treated as a by-product of water and for preparation of steam the process is just to boil water. Therefore, the common man always treats steam as part and parcel of water. It is a fact that in taxing statute the words which are not of technical expressions or words of art but are words of everyday use, must be understood and given a meaning, not in their scientific sense, but in a sense as understood in common parlance' (Shibu Thomas, *Times of India*, April 17, 2010). The judgement meant a great deal to the company, which was then required to pay 10% sales tax instead of 4%, for the years 1988–1991. One cannot but help feel a tinge of regret that after two decades an appeal to chemistry would be overturned by a vote in favour of common sense. An alert colleague of mine drew the attention of the news magazine of the American Chemical Society to this striking (at least for chemists) judgement of the Bombay High Court leading to an article that begins provocatively: 'Steam is just water. Right? And water is the most abundant chemical on Earth. Right? So when is steam not a chemical? When the Bombay High Court says so' (Ritter, S. K., *Chem. Eng. News*, 2010, **88**, 56).

The court verdict, delivered in the special context of a tax dispute, may have been of little general interest, except to those concerned with chemicals and chemistry. Having been drawn into a career in science by the promise of chemistry, resisting the apparent stability of the administrative services or the uncertain world of journalism, the High Court's pronouncement turned my attention to a discipline that has rarely been viewed as glamorous, in public perception. Chemistry is a common word. Journalists wax eloquent about the 'chemistry' between Prime Minister Manmohan Singh and American Presidents as diverse as George Bush and Barack Obama. Reviews of cinema and theater often highlight favourable or, sometimes, unfavourable chemistry between actors and actresses. In common usage 'chemistry' is a word used, more often, to describe human interactions, than to refer to the science itself. 'Chemicals' is a term that instantly conjures up visions of poisonous, harmful substances that are hazardous to human health and damaging to the environment. Toxic wastes, pollutants of every kind, gaseous, liquid and solid seem to be invariably associated with chemistry. In agriculture, so central to our existence, the dramatic increase in productivity following the industrial synthesis of ammonia and its conversion to the fertilizer, urea, has faded from memory. The story of the rise of DDT as a pesticide has long been supplanted by the chronicle of its fall from grace, as a toxic despoiler of the environment. While the first half of the 20th century has been hailed by historians of science as the age of physics, the second half has been heralded as the age of biology. The glamorous and widely appreciated triumphs of these sister disciplines, often inextricably linked to chemistry, have undoubtedly relegated the subject to the position of the Cinderella of the sciences. It is indeed hard to conjure up an image of chemistry that would enhance its public standing. Even when the Nobel prizes are announced annually, chemistry arouses a heightened public interest only when the award recognizes an advance that makes deep inroads into biology. This year's awards provide an example, with the public interest in 'test tube babies' and the promise of graphene in 'nanotechnology' clearly scoring over the subject of making carbon–carbon bonds using 'palladium catalyzed cross-coupling in organic syn-

thesis'. Carbon is central to organic chemistry, but its elemental forms are increasingly associated with physics and the seductive world of nanoscience. Graphene, a single layer of carbon, a truly two-dimensional material, is not referred to in the press as a chemical; it is a wondrous 'material' that may be a harbinger of many technological advances. The build up of expectation is remarkable: 'Imagine a machine that can test the same physics that scientists test, say, CERN, but small enough to stand on top of your table. Graphene allows this to happen. . .' (Andrew Geim, 2006 interview, quoted in *Frontline*, November 19, 2010, p. 105).

A recent editorial in *Nature Chemistry*, a relatively new addition to the constantly expanding portfolio of *Nature* journals, raised a question: 'Where are the champions?' The essay argues that 'chemistry lacks the easily articulated grand challenges associated with physics or biology and it generally gets a rough ride in mainstream media' (*Nature Chemistry*, 2010, **2**, 599). Indeed, I have heard the phrase 'grand challenge' only from physicists (invariably, theoretical) and biologists. Understanding quantum gravity or dark matter or the tantalising prospect of a theory that unifies all particles and forces is clearly in the realm of 'grand challenges'. So too are problems that address the understanding of brain function, the task of winning the never ending war against infectious and metabolic disease and the translation of genome sequences into predictors of biological fate. While chemistry and chemicals are central to all of biology and a great deal of physics, there is little to suggest that this fact is commonly known. The *Nature Chemistry* editorial notes that the 'negative perception of chemicals permeates through large sections of society and seems to influence how some consumer products are marketed. Perhaps the most stark example of this is a particular brand of compost that boldly proclaims to be "100% chemical free". That certainly would be some kind of miracle. Examples such as these have even prompted the Royal Society of Chemistry to offer a £ 1-million prize to anyone who can provide them with a sample of a material that is chemical free'. Curiously, the word 'material' is used even in science in a manner that places it apart from its poor cousin, the humble chemical. In naming journals, *Materials Chemistry* or *Chemistry of Materials* sound both scholarly and sensible. *Chemicals Chemistry* or *Chemistry of Chemicals* offend the ear and our sensibilities. Simply put, in popular perception, even amongst scientists, the term 'materials' seems to refer to substances that are 'good and useful'. 'Chemicals' appear to be best avoided. The *Nature Chemistry* editorial notes that the most common misconceptions about chemicals, 'include the idea that an individual can lead a chemical free life, and the notion that synthetic chemicals are dangerous, whereas natural ones are not'.

Even as I wondered about the public perception of chemicals and chemistry I received, as an unexpected gift, a beautifully illustrated, coffee table book: *Molecules that Changed the World* (Nicolaou, K. C. and Montagnon, T., Wiley-VCH, Weinheim, 2008). The book is subtitled, 'A Brief History of the Art and Science of Synthesis and its Impact on Society'. The authors state their intentions clearly in the preface: 'The inspiration for this book arose from the desire to enlighten and instill a greater appreciation in society at large about a difficult subject – chemistry. . . Many people remember chemistry as one of the most challenging subjects in college, or the class in which they struggled. For others, the mere mention of the word chemistry conjures up images of explosions, poisons, and pollution (toxic waste and dangerous fumes). The reality of chemistry, however, is far more exciting and rewarding, once these unfortunate and distressing images are dispelled'. In presenting a view of the heart of chemistry, the authors attempt to dispell common prejudices: 'Indeed, beyond this curtain, there lies a beautiful world of molecules with a glorious history and myriad wondrous applications'. Chemical synthesis, a creative activity which over two centuries has produced most of the chemicals that we need and use, but love to hate, is an underappreciated discipline. Nicolaou and Montagnon present a superbly illustrated tour through a world of molecules as familiar as glucose and aspirin, as feared as strychnine, as valuable as penicillin, cyclosporin, erythromycin, taxol and vancomycin and as exotic and unfamiliar as thiostrepton and the monstrous marine natural product, palytoxin. While structural complexity has provided the intellectual challenge for organic synthesis, modern drug discovery has remained rooted in the more mundane world of medicinal chemistry. Most widely used pharmaceuticals are small molecules, despite the oft repeated claims of biotechnology. In a chapter entitled 'Small Molecule Drugs', the authors bring to life the history of omeprazole and the drugs used to treat gastric ulcers, sedatives like valium and the barbiturates, antidepressants of which prozac has become widely known, antivirals and antibiotics that provide the main line of attack against invading viruses and bacteria. The complexities of chemistry are lightened by the authors' foray into history, bringing chemists and their discipline to life. Chemistry, biology and medicine are intertwined in ways that are not commonly appreciated. In attempting to take readers on a guided tour through a world of molecules, that have transformed our existence, Nicolaou and Montagnon may have indeed provided the justification for an old slogan that used to proclaim, 'better living through chemistry'. The coming year, 2011, has been declared by UNESCO as the 'International Year of Chemistry'. In celebrating chemistry as a discipline we might take heart in the fact that in stripping stream of its status as a chemical, the Bombay High Court did not address the issue of what constitutes a molecule. Truly, chemicals have changed the world.

P. Balaram

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EDITORIAL

Being Busy

While listening to an impassioned speaker extolling the glorious past of his discipline and predicting an exciting future, I was struck by something he said. He outlined many wonderfully challenging areas that appeared to be opening up, lamented the fact that he could not turn the clock back to begin research all over again (here I must confess feeling a tinge of regret), and finally burst out: 'Most young people are too busy these days to think about new problems'. Here too, he struck a chord. How often have we all heard the familiar, 'I am too busy' excuse, when something, however minor, needs to be done. As an administrator, of sorts, I have often had to recruit colleagues to the task of carrying out the many (and sometimes tiresome) chores that are so necessary in academic institutions. Not infrequently, accomplished and capable colleagues tell me, 'I am too busy. Find someone else'. Coincidentally, even as I wondered about scientists too busy even to sense the changing contours of their fields, I received an e-mail offering a ray of hope. The senders were marketing a product, which was a publication entitled *Making Every Minute Count: How PIs Can Fit 28 Hours Into a 24 Hour Day*. This 'executive report' produced by an organization called the 'Principal Investigators Association' was described as a 'management survival guide with over 50 pages of valuable methods and expert tips'. The promotional mail noted: 'For most busy scientists it seems there is never enough time in the day. Time management is critical to the success of any investigator'. Why are academics, scientists and professors, so busy? According to the e-mail promoting the time management guide, scientists need to teach classes, mentor research students, write grant proposals, prepare manuscripts for publication and attend scientific meetings, an activity critically important for networking with peer groups, which decide the fate of grants and publications. In addition, there are the committee meetings at institutions which can be an imposition on time available. Reading this I almost began to feel extremely sorry for hard pressed academics, so busy that they can hardly find the time to think – presumably an activity of some importance in the environment of academia. The Principal Investigators Association seemed to be addressing American academics and I could not help but wonder, 'is the situation different in India?'

The situation in India has changed dramatically since I began my independent scientific career over 35 years ago. There was a time when faculty at academic institutions seemed to work at a pace all their own; unruffled by happenings elsewhere. We were unaware of competitive pressures and the need to publish research outcomes quickly. We were also ignorant of the importance of publishing frequently in visible journals. The impact factor was an idea that was hardly known and the *h*-index was to come many, many years later. Research grants were very few and most scientists did what they could with limited funding. Air travel was prohibitively expensive and foreign exchange scarce. The result, of course, was that most scientists stayed at home. There seemed a great deal of time available for reading, books and journals, which seemed so necessary for understanding the sometimes difficult subjects needed to push research forward. Instruments were scarce and immensely valued. Looking after them well and trouble shooting when the need arose were activities that were deemed important. Institutions also seemed to always have a large pool of people to carry out the multitude of tasks that needed to be done; albeit routine chores that could scarcely be exciting to academic faculty. Institutions in India fall into three distinct classes. There are stand-alone research laboratories where scientists do no teaching. There are universities where faculty teach but do not always run large research laboratories. Then there are institutions where faculty are expected to do both research and teaching; the emphasis varying depending on the mandate of the institution. Is everyone really hard pressed for time; overworked and struggling to find the four extra hours promised by the promotional e-mail from the Principal Investigators Association?

Over the last twenty years science funding has steadily grown in India. Old institutions have expanded; new ones have been established. The expansion has gathered pace in recent years, although many old university departments seem to have remained untouched. Even as the scientific enterprise has grown, so too have the demands on scientists. There are more committees to be manned and run; grants need to be reviewed and awarded. More honours and recognitions to both young and old scientists need to be distributed. There are more seminars and meetings to

attend. The demands to advise and mentor new institutions are on the rise. As government departments work with larger budgets than even before, there is a sudden proliferation of advisory committees, whose advice is often discarded unnoticed and unsung. This flurry of activity requires considerably more travel than before, with the result that the more visible and sought after scientists are often on the move. International networking requires considerable investments of time; sometimes dividends are forthcoming, but most often the returns are not always obvious. An interesting feature of the new sociology of science in India is that the importance of academics is often directly correlated to the extent of travel performed. Globalization is advanced as the reason for the excessively large number of delegations that head overseas, ostensibly to learn how other countries run their science and to attract young Indians to return. Ironically, the many available technologies of the information age are rarely used in gathering or disseminating information. In this scenario it is hardly surprising that many scientists find it easier to acquire information on their fields by attending conference after conference, rather than by reading the literature. The multitude of distractions in academia have begun to relegate teaching and research to a lowly status; a situation that must be a matter of concern.

In thinking about the issue of 'being busy' I turned to a book lent to me by a scholarly colleague, but which had remained unopened on my desk for weeks. *Cognitive Surplus: Creativity and Generosity in a Connected Age* by Clay Shirky (The Penguin Press, New York, 2010) is the kind of book I would normally avoid. Everyone has their own pet prejudices and I am no exception. I do not like books about the Internet and give a wide berth to the flood of books on managerial successes, which seem to occupy a great deal of space in bookshops. But I was intrigued by the term 'cognitive surplus'. This is just the kind of surplus that seemed needed in science today; in a world awash with information and too little time available to absorb it. The blurb on the jacket of the book described the author as an 'internet guru', adding that he 'may be the finest thinker we have on the internet revolution'. Shirky's book is easy to read and I was drawn into the book very quickly after the first sentences: 'In the 1720s London was busy getting drunk. Really drunk. The city was in the grip of a gin-drinking binge, largely driven by new arrivals from the countryside in search of work.' Unaware of the 'Gin Craze', I read on to discover how and why Londoners sobered up. According to Shirky: 'What made the craze subside wasn't any set of laws. Gin consumption was treated as the problem to be solved, when it was in fact a reaction to the real problem—dramatic social change and the inability of older civic models to adapt. What helped the Gin Craze to subside was the restructuring of society around the new urban realities created by London's incredible social density, a restructuring that turned London into what we'd recognize as a modern city, one of the first.' In Shirky's analysis social transitions, of which the industrial revolution was the first major upheaval of modern times, need 'lubricants' that ease and mitigate the trauma of change, until new structures provide the needed stability of a new order. Shirky goes on to argue that in the postwar years American society has been in the midst of a major historical transition. He notes that 'postwar trends of emptying rural populations, urban growth, and increased suburban density, accompanied by rising educational attainment across almost all demographic groups, have marked a huge increase in the number of people paid to think or talk, rather than to produce or transport objects'. Observers of the ongoing transformation of the Indian economy may see early signs of an impending transition.

Shirky moves on to ask a curious question. 'During this transition, what has been our gin, the critical lubricant that eased our transition from one kind of society to another?' His answer – television. He argues that 'the host of . . . amusements offered by TV has absorbed the lion's share of the free time available to the citizens of the developed world'. TV viewing, most often done alone results, in Shirky's words, in 'underinvesting in relational activities'. He suggests 'treating the free time of the world's educated citizenry as an aggregate, a kind of cognitive surplus' and asks: 'How big would that surplus be?'. He estimates that 'Americans watch roughly two hundred *billion* hours of TV every year'. Shirky points out that time spent on editing Wikipedia, in all languages, represents 'something like 100 million hours of human thought'. Modern internet technologies make collective effort so much easier, leading Shirky to suggest that what 'makes the current age remarkable is that we can now treat free time as a general social asset that can be harnessed for large communally created projects, rather than as a set of individual minutes to be whiled away one person at a time'. Shirky's book is fascinating in its treatment of 'online' spaces which permit communal, collaborative activity.

I have moved far afield from my initial concern of scientists with too little free time, to Shirky's analysis of the 'cognitive surplus' arising from an unproductive use of free time. Is academia in India in the midst of a social transition and are conferences and excessive travel the 'gin' that is lubricating a transformation? The social fabric of 21st century science may be vastly different from that of the century gone by. In thinking of an appropriate way to end these musings I turned to *Google* and typed in 'being busy'. I was instantly rewarded with an interesting quote: 'A man who is very busy seldom changes his opinions'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Arsenic, Bacteria and Mendeleev's Legacy

Gifts, especially when unexpected, are always a source of pleasure. A mischievous and much travelled colleague returned, a few days ago, from a 'working holiday' abroad and presented me with a tablemat, confidently asserting: 'This should help you learn even while you eat and drink'. The tablemat, marketed as part of a 'painless learning' programme had a beautifully produced version of the Periodic Table, laminated so that tea and coffee stains could be readily wiped away. There is something captivating about the Periodic Table, so central to chemistry and indeed, the study of all materials. Biology and life seem to need very few of the over one hundred elements so colourfully depicted in modern versions of the Periodic Table. A handful is all that is necessary to wade through a biochemistry course; hydrogen, carbon, nitrogen, oxygen, phosphorus and sulphur are the major actors, with a few other elements playing limited, albeit essential, roles. Even as I admired my new possession, I could not resist returning to a favourite essay, 'Mendeleev's Garden', in which Oliver Sacks describes his childhood fascination with the elements. In Sacks' words: 'I was convinced that the Periodic Table was neither arbitrary nor superficial, but a representation of truths which would never be overturned, but would on the contrary, continually be confirmed, show new depths with new knowledge, because it was as deep and simple as nature itself. And the perception of this produced in my twelve year old self a sort of ecstasy, the sense (in Einstein's words) that "a corner of the great veil had been lifted" '. (*Uncle Tungsten: Memories of a Chemical Boyhood*, Vintage Books, New York, 2001, p. 211). Having drifted away from the diversity of chemistry over the years, the Periodic Table began to evoke memories of long forgotten courses, where the neatly arranged rows and columns of elements seemed so important. Even as I was musing on the past, my attention was drawn to a report that announced the discovery of 'A bacterium that can grow by using arsenic instead of phosphorus' (Wolfe-Simon, F. *et al.*, *Science*, 2 December 2010; doi:10.1126/science.1197258). This report appeared to dramatically overturn many long held ideas about the chemistry of life. Although we learn that 'six elements make up... the bulk of living matter', the authors note that, 'it is theoretically possible that some other elements in the periodic table could serve the same functions'. I was immediately reminded of a formidable teacher, in the years that I learnt a little chemistry, who would demand aggressively that students answer the question: 'why is silicon not widespread in nature's chemistry although it lies directly below carbon in the periodic table?' I heard this question at a time when science fiction formed the staple of my reading. It seemed so tempting to think of aliens on a far removed planet surviving happily on a silicon enriched and carbon depleted environment. The elements, at first glance, seem so replaceable in Mendeleev's columns, especially if the, often uncomfortable, niceties of chemistry are ignored. The report by Wolfe-Simon *et al.* seemed to raise an interesting question: can arsenic substitute for phosphorus in the chemistry of life? The paper describes a bacterium, somewhat mysteriously called GFAJ-1, which was isolated from a lake in California, which 'is a hypersaline and alkaline water body with high dissolved arsenic concentrations (200 μM on average)'. While in its natural habitat GFAJ-1 undoubtedly has adequate access to phosphate as a nutrient, Wolfe-Simon *et al.* report growing the bacteria in arsenate enriched media, apparently devoid of phosphate. Using the device of increasing arsenate enrichment in the medium, through successive transfers, these authors suggest that the bacterium can 'exceptionally ... vary the elemental composition of its biomolecules by substituting As for P'.

Arsenic is not a popular element. In recent times arsenic toxicity has been a subject of great concern following the contamination of groundwater in Bangladesh, West Bengal and several other locations. In the world of fiction and theater, arsenic was often a poison of choice, with the Broadway play 'Arsenic and Old Lace' being a great hit, converted into a movie in the 1940s. The question of whether Napolean was murdered on St. Helena by arsenic poison has intrigued forensic researchers since the 1950s. Most recently, neutron activation analysis has weighed in against the theory of poisoning. Arsenic, ironically has also been central to the development of chemotherapy and medicinal chemistry. Paul Ehrlich's Nobel lecture in 1908 describes the successful use of arsenic compounds as trypanocidal agents; the first triumph in developing molecules that kill trypanasoma, the parasites responsible for African 'sleeping sickness'. Salvarsan, the first drug for syphilis was another of Ehrlich's discoveries; the result of years of painstaking studies of arsenicals. Despite ushering in the dawn of chemotherapy, arsenic chemistry has not been a glamorous area of research. As I read the

Wolfe-Simon paper, I wondered if this was about to change. If there was compelling evidence for arsenic incorporation into DNA, RNA and ATP would not the tide turn in favour of this relative of phosphorus? Were we about to see a surge of interest in the chemical biology of arsenic? At first glance the paper had all the trappings of modern chemical analysis. The list of techniques used to estimate arsenic in cellular fractions is impressive: inductively coupled plasma mass spectrometry (ICP-MS), nano-secondary ion mass spectrometry (nano SIMS) and synchrotron X-ray using micro X-ray absorption near edge spectroscopy (μXANES). To the average reader, and I must count myself as one, the apparent sophistication of the analytical methodology used must seem reassuring. However, no individual, isolated class of metabolites is analysed in this report. In a surge of optimism, the authors conclude that arsenic atoms have indeed been substituted into the DNA backbone, replacing phosphorus. They go on to add that the arsenylated analogs of a whole host of intracellular molecules are indeed present. They conclude in intriguing fashion: 'How arsenic insinuates itself into the structure of biomolecules is unclear, and the mechanisms by which such molecules operate are unknown'.

The 'arsenic bacteria' paper might have taken some time to attract attention. However, a press release from NASA served to catalyse a remarkable flurry of interest. The space agency, somewhat unwisely, proclaimed: 'NASA-funded astrobiology research has changed the fundamental knowledge about what comprises all known life on earth'. The press release was emphatic: 'The finding of an alternative biochemistry makeup will alter biology textbooks and expand the scope of the search for life beyond Earth'. If I had been forty years younger I might have conjured up visions of arsenic eating Martians, whose very breath was toxic to humans. Unfortunately, age blunts the imagination. The seeds of doubt that must have germinated in the minds of many readers of the *Science* paper quickly found expression in the ever expanding universe of blogs on the internet. A storm of criticism began to appear, with every piece of experimental data being publicly dissected. From bacterial growth curves to X-ray absorption near edge spectroscopy, almost no piece of reported data was exempt from the process of deconstruction. The blogs, and there are many, are both instructive and entertaining. It is here that I was reminded of Frank Westheimer's thoughtful and reflective analysis on 'Why nature chose phosphates' (*Science*, 1987, **235**, 1173). Could nature have used silicates or arsenates? Westheimer noted that arsenate esters breakdown so rapidly in aqueous media that arsenic could hardly provide the requisite stability for the essential molecules of biochemistry. In the concluding section of his commentary Westheimer returns to an oft asked question: 'Why have the choices made by chemists in the laboratory been so strikingly different from those made by nature in living cells?' His answer: 'We can understand the choices made by chemists and by natural selection. They are both correct'.

Ignorant as I am of Twitter and the joys of tweeting and as a bemused entrant to the world of blogs, I found the rapidity and scale of the attack on the Wolfe-Simon paper astonishing. The power of the new media for communication and the rapid nucleation of networks of interested readers are indeed remarkable. Less than a week after the online appearance of the paper, the evidence for biomolecules with arsenic in their backbones seemed to be evaporating, under the heat of technical criticism. NASA blundered in a manner characteristic of governmental agencies worldwide, when it declined to engage the critics in discussion. The reason cited was that it would be inappropriate to debate the results in a paper published in one of the 'most prestigious scientific journals', with bloggers who could hardly qualify as peers. The response was overwhelmingly critical of NASA's 'we know best' attitude. I found the argument, that pre-publication refereeing is really 'just a quality filter' while post-publication reaction is 'true peer review', compelling. All the responses to the Wolfe-Simon paper are not negative. GFAJ-1 may turn out to be another wonderful example of adaptation to an extreme environment, revealing new ways by which the microbe handles arsenic. The focus on arsenic may also revive interest in chemotherapy using arsenic trioxide and the biochemical mechanisms involved (Kroll, D., 4 December 2010, cenblog.org/terra-sigillata). The Wolfe-Simon paper appears to be a case where a seductive hypothesis has enthralled a group of enthusiastic researchers. In biology, as practiced in India, I often hear that all good research must be hypothesis driven. There is sometimes a danger in this approach. Researchers can fall prey to the temptation of designing and interpreting experiments to prove a pet hypothesis. Critical control experiments can be overlooked, often overwhelmed by the desire to sustain an engaging hypothesis. This indeed seems to be the trap into which the authors of the 'arsenic eating bacteria' paper have fallen. They may yet resurrect the story they have presented, if the presence of arsenic in place of phosphorus is convincingly demonstrated in DNA isolated from the bacterium.

In thinking of hypothesis driven research and the oft reviled, observational approach to science, I was struck by the fact that two great intellectual syntheses of the 19th century were indeed based on painstaking observation and intense reflection; Darwin's principle of natural selection, as the driver of biological evolution and, Mendeleev's conception of the Periodic Table, as the central organising structure of chemistry, both of which are valuable in thinking of the Wolfe-Simon paper. Arsenic is an element that has never been at the centre stage of modern chemical research. The bacterium GFAJ-1 has provoked a surge of interest in the chemistry of arsenic. Looking down at my tablemat, which so concisely displays Mendeleev's legacy, I could not but help wonder if other elements will also emerge from the shadows.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Chemists and Chemistry

The J. N. Tata auditorium at the Indian Institute of Science is an imposing lecture theater. On New Year's Day 2011, the hall was packed with nearly 1000 high school children herded in, accompanied by watchful teachers, to witness the Chemical Research Society of India launch the International Year of Chemistry. India's most visible and accomplished scientist, C. N. R. Rao spoke forcefully about the contributions made by chemistry and chemists over the past two centuries. The roll call of honour began with Faraday, now claimed by both physics and chemistry and moved on, inevitably, to Linus Pauling, arguably the dominant presence in 20th century chemistry. This overview of a discipline's historical progress was followed by a lecture-cum-practical demonstration of chemical experiments that produce light, sound and, sometimes, fire. Cold and hot flames, invisible writing that appeared colourfully intense, upon spraying a chemical, and wonderfully luminescent solutions glowing in the dark engaged the young audience. Chemistry seemed a part of magic; an entertaining visual spectacle that hid the image of a subject viewed as boring or intimidating in its detail by most school students. A cursory glance at the chemistry textbooks used by students appearing for the all-India school board examinations (CBSE or ISC) reveals impossibly large volumes, densely packed with information. A mere look at the textbooks must fill the average student with a great sense of foreboding; turning the pages will undoubtedly turn dread into despair. I suspect physics and biology textbooks are hardly more attractive as the volume of information considered necessary at school stage seems to increase in a frightening manner. With both teachers and students struggling to cope with bloated syllabi, it is hardly surprising that 'science' and, particularly, chemistry, are subjects that do not attract many students. In the midst of the captivating demonstration of chemistry's entertainment value, the lecturer paused and asked of the audience: 'How many of you dislike chemistry?' There was little reaction. He then goaded them on: 'Don't worry, your teachers are not looking'. The response was now spontaneous as a sea of hands went up; a resounding vote for the commonly held perception that chemistry is an unpopular subject. The lecturer moved on to play a short video recording of Linus Pauling, iconic and inspirational, speaking of science, at times reflectively. Pauling painted on a very large canvas, moving over the course of a remarkable career from quantum mechanics and crystallography to molecular biology. Chemistry was always his central theme; atoms, molecules and structures were at the core of his forays into immunology and medicine. I am not sure how many of the children were moved by listening to one of the all-time greats of science; but I could not help thinking somewhat sadly, that Pauling's example of viewing the discipline in its broadest sense has never been followed in the development of chemistry teaching and research in India.

The new year provides an opportunity to look back at major events in science in the year gone by. The major journals provide retrospectives and, in following current fashion, rankings of the most significant advances. Just over twenty years ago the journal *Science* introduced a feature that appeared in the last issue of the year, intriguingly entitled 'The Molecule of the Year'. The editorial by the then Editor, Daniel Koshland, explained in captivating fashion the reason for the new feature: 'Historians tend to personalize history. They use political leaders to symbolize war or peace, freedom or slavery, abundance or starvation. Political systems represented by these individuals may be essential but political leaders cannot cure disease without medicine, cannot improve crops without fertilizer, and cannot encourage the literacy on which democracy depends without communications technology. The great advances of the past have been profoundly influenced by science and technology, and our present standard of living depends on them. Political systems can be designed to encourage advances in science and are essential to the fair distribution of its products. Wealth must be created before it can be distributed.' Koshland went on to note that 'in the rush of daily events and the ease of describing personalities instead of analyzing issues, the fundamental causes of progress can be obscured'. He then unveiled the new feature by stating: 'To symbolize . . . scientific progress and to honor the structure that creates it, *Science* has decided to name a Molecule of the Year. The molecule will symbolize a discovery or technique that may actually involve many molecules, but the award will be singular to force us to choose one such discovery each year that is likely to have the greatest influence on history' (Koshland Jr, D. E., *Science*, 1989, **246**, 1541). The choice of the term

'molecule', so central to chemistry, seemed an implicit acknowledgement of the pivotal position occupied by the discipline in science. The first choice in 1989 was the DNA polymerase molecule, the engine that drives the polymerase chain reaction (PCR). This reaction which allows the 'amplification' or copying of DNA has revolutionized molecular biology. However, then and possibly even now, DNA polymerase seemed distant from 'chemistry'. In the period between 1990 and 1996 the term 'molecule' was used to symbolize scientific achievement and progress. Synthetic diamond (1990), the protein p53 so important in cancer research (1993), DNA repair enzymes (1994) and Bose–Einstein condensates (1995) were accorded the honour of being recognized as *Science*'s 'molecule of the year'. Biology, materials science and physics seemed to dominate. There were two exceptions. Buckminsterfullerene (C_{60}, 1991), the 'celestial sphere that fell to Earth' in the words of one of its discoverers, H. Kroto, captured the imagination by its sheer symmetry and beauty. Chemistry reigned briefly. The following year, 1992, saw a surprising choice, nitric oxide (NO), a chemical so simple and common that chemists may hardly give it a second thought, outside an undergraduate classroom. Once again it was a nod to biology, with the recognition of its production and role in signalling within living cells. It was increasingly evident that the term 'molecule' with its strong association with chemistry may be inadequate to symbolize the broad advance of science on many fronts. From 1996, *Science* began to title its annual year end feature as 'Breakthrough of the Year'. Over the past fifteen years many fields and topics ranging from 'understanding HIV' (1996) to the 'first quantum machine' (2010) have been featured. None appear to directly relate to the discipline of conventional chemistry. From 2002 a scientific body with an improbably long acronym, the International Society for Molecular and Cell Biology and Biotechnology Protocols and Researches (ISMCBBPR), based in Berlin has begun announcing a 'molecule of the year'. The 2009 choice of the jumping gene or 'sleeping beauty' transposon is hardly like to enthuse chemists practising what is often termed 'mainstream chemistry'.

In India chemistry is highly visible in both industry and academia. Whenever statistics on publications and scientometric indices are compared with those of the advanced countries, chemistry does better than many other disciplines. Among the national laboratories, those devoted to chemistry are extremely well regarded. Despite the apparent health of the subject in our institutions there appears to be a sense of disquiet; a feeling that the discipline seems less attractive to students than biology or physics. Chemistry in the West, particularly the United States, has transformed over the last quarter of a century, with traditional academic departments broadening their scope to encompass newly emerging areas of chemical biology and materials science. The dramatic advances in chemical analysis over the past few years have really been catalysed by the needs of biology and the requirement to address complex molecules and assemblies in atomic detail. Mass spectrometry and imaging technologies of diverse kinds are commonplace in biology. The impact of the transformation that has swept chemistry elsewhere is yet to be really felt in India. Biological problems have served to unite the traditional sub-disciplines of chemistry in the best of institutions world-wide. Unfortunately in India tradition still seems to rule. Organic, inorganic and physical chemists continue to go their separate ways. Biochemistry, despite the name, seems to have little to do with chemistry. This lack of unity limits the appeal of the discipline. Sharply defined boundaries are irrelevant in today's science. Nevertheless, high and, sometimes, impenetrable barriers ('tunneling' seems improbable) exist between the various sub-disciplines of chemistry in our institutions. Natural products chemistry, once a thriving area of research in many university departments, has virtually become extinct, even as advances in chromatography and analysis have revolutionized the field. Biology offers a fertile hunting ground for natural products chemists, but our institutions lack the technical expertise to exploit the explosive growth of structural analysis. Chemical ecology, a subject that unites wondrous biological phenomena with marvellously sophisticated chemistry, needs practitioners who speak an interdisciplinary language. We can hardly exploit biological diversity to discover new pharmaceutical leads if there are almost no trained chemists comfortable with the analysis of natural products. In teaching, a great disservice is done to students when 'secondary metabolites' are divorced from proteins and nucleic acids, so central to all of biochemistry.

Chemistry, worldwide, has indeed had a crisis of identity even as biology and materials science have exploded. A feature in a prominent chemistry journal raised an interesting question: 'Is the Nobel Prize in Chemistry Still Relevant?' (Mukhopadhyay, R., *Analytical Chemistry*, 2009, **81**, 7866). This is an oft discussed issue, especially when advances in what is perceived as 'biochemistry' or 'biology' are recognized. The commentary quotes the Nobelist, Roald Hoffmann: 'Academia is stodgy and stuck in disciplines'; a judgement that undoubtedly applies to chemistry in India. In attempting to convey the excitement of chemistry on New Year's day the enthusiastic lecturer told his large audience: 'Virtually all human activity requires chemistry'. He was understating his case. Indeed, all human activity requires chemistry.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Ph Ds: Uncomfortable Questions

'India must produce more Ph Ds', is a statement that is constantly heard in bodies that discuss science policy. As research funding grows, the easiest quantifiable measures of progress in science appear to be the number of papers published in journals covered by the *Science Citation Index*, and the number of Ph Ds awarded annually by our academic institutions in science and engineering. Comparative statistics are frequently projected, with China rapidly forging ahead in the race to increase the production of researchers with Ph D degrees. A favourite and well worn quote that I have often heard is: 'MIT produces more Ph Ds annually in computer science than all of India's institutions combined'. As the custodian of a highly visible academic institution in science and engineering, I have begun to dread the question: 'What was your score of Ph Ds last year?'. This is quickly followed by the supplementary questions: 'Did you cross the figure of two hundred? When will you touch three hundred?' Even as I struggle for an answer, I am reminded of the anxiety with which Indian cricket fans waited for Sachin Tendulkar to score a double century in Tests, having always managed to fall short of the magic figure, despite many sterling performances. These questions also remind me of one that I confront frequently, from aggressive and critical colleagues, when I don my editor's hat: 'Why does the impact factor of *Current Science* not improve more rapidly?' Quantitative measures are often useful in judging progress and achievement but, at times, comparisons of chosen indices can be extremely disconcerting, when the true import of the metrics are poorly understood by those who use them. The quality of theses which are approved for the award of doctoral degrees varies dramatically across the wide spectrum of institutions in India. In the best places, students registered for doctoral degrees are the main drivers of research activities. When the best of students are associated with knowledgeable and scholarly supervisors, the quality of theses produced can match the best in the world. Unfortunately, there are also many institutions where research students receive little attention and formal review mechanisms are deficient. The ongoing expansion of higher education in science and engineering will significantly increase the number of Ph D producing institutions in India. Most of the new institutions, IISERs and IITs amongst them will undoubtedly be well funded, with the expectation that high quality research will flow, in addition to the training of the best of undergraduate students. Research students at centrally funded institutions invariably receive financial support in the form of scholarships, a factor that eases to some extent the uncertainties of a course that may take several years to complete. Are our Ph Ds in science and engineering employable in India? Do academic institutions and national laboratories have job openings for the newly minted Ph Ds or even those who enter the market after a brief post-doctoral period? Are industrial R&D laboratories on the look out for Ph D scientists? Can the widely discussed 'faculty shortage' be alleviated by increasing the production of Ph Ds in science and engineering? These are all questions that are often raised. There is little consensus on the answers. In thinking about Ph Ds I was drawn to an article in *The Economist* entitled 'The disposable academic'. The sub-title was even more provocative, declaring: 'Why doing a Ph D is often a waste of time' (Dec. 16, 2010).

The Economist article begins by tracing the origin of the word 'thesis' to the beginning of the Reformation, which led to the split in the Christian church and the rise of Protestantism. Martin Luther is credited with nailing 95 theses 'to the door of a church' in Wittenberg in 1517. *The Economist* points out that 'in those days a thesis was simply a position one wanted to argue'. For good measure, the article pointedly notes that Luther's thesis 'asserted that Christians could not buy their way to heaven'. Momentarily diverted, I could not help marvelling at the soundness of this thesis, nearly six centuries later. But the *Economist* was not addressing the history of the reformist movement within the Christian clergy; the opening sentences were intended only to highlight the transformation of 'theses' over the centuries. In Luther's time 'a thesis was simply a position one wanted to argue'. The modern thesis which is required to obtain a Ph D degree 'is both an idea and an account of a period of original research'. Indeed, writing a thesis constitutes the most difficult period for all those who wish to call themselves a 'doctor of philosophy'. A Ph D is almost the minimum qualification need to enter a career in academia. It is the starting point of a research career in science. It is also a degree sought by many who believe that the honorific title 'doctor', confers an aura of scholarship and distinction. Administrators and bureaucrats write theses

and obtain Ph Ds in disciplines, which often have little to do with their daily work. Politicians find that acquiring honorary doctorates may be an easier option, raising their status as scholars in the eyes of an uninformed public. While Ph Ds can be acquired in almost any discipline, it is really science and engineering that are the areas where there is a thrust to increase the number of doctoral degrees awarded in India. *The Economist*'s thesis that 'doing a Ph D is a waste of time' seemed to state a point of view that is diametrically opposite to the prevailing consensus in Indian higher education, where the focus is clearly on increasing the number of Ph Ds produced. Undoubtedly, the article's focus was on the situation in America or Britain, where newly minted Ph Ds face a declining job market in both industry and academia. The dramatic expansion of the American research university system over the past few decades has contributed to the glut of Ph Ds. In many areas of science, most notably the disciplines that contribute to biomedical research, the size of research groups has grown dramatically over the years. Large teams of Ph D students and post-doctoral fellows work under the supervision of principal investigators (PIs), who generally are tenured members of the faculty. The number of Ph Ds produced has grown very much faster than the available positions in academic faculties and in industrial R&D laboratories. Low salaries, the uncertainties of the job market and diminishing opportunities to begin independent research careers are beginning to raise serious questions about the Ph D programs in many disciplines. *The Economist* notes that 'the fiercest critics compare research doctorates to Ponzi or pyramid schemes'.

I was struck by this reference to an investment scam, made famous in recent times by the disgraced financier, Bernard Madoff. Can the vast edifice of graduate research be likened to the dubious stratagem of paying dividends to the first generation of investors by using capital raised from the next generation, without any productive activity? Is academic science a self perpetuating exercise in which expanding Ph D programs serve only the interests of an existing professoriate, with little regard for career opportunities for those who spend years in long drawn out doctoral programs? These questions are being increasingly asked in the United States and Britain as a sluggish economy accentuates the problems created by an over-production of Ph Ds. The situation in India appears dramatically different. The ongoing expansion of the sector of higher education is creating a new demand for faculty with Ph D degrees. Private sector research also seemed poised for growth, although projections are harder to make. Corporate captains often publicly bemoan the shortage of well trained Ph Ds; stating their intention to recruit, but implying that their specific requirements are not addressed in the training that students receive in academic institutions. India produced about 8500 Ph Ds in science and engineering according to statistics for 2005–2006. The numbers are unlikely to be significantly different today. A recent assessment of the Indian academic science noted: 'the status of doctoral education in India is disturbing. Its numbers are not increasing to meet the growing demand from the public sector research labs and higher education institutions. There are a small number of university level institutions that produce a decent number of doctorates. Even among them, there is a suspicion about the quality of doctoral education, especially in the institutions that are not known to be reputed yet contribute to a significant number of doctorates' (Agarwal, P., *Indian Higher Education: Envisioning the Future*, Sage, New Delhi, 2009, p. 258).

One of the most serious concerns about doctoral education in India is the wide variation in the quality of the Ph Ds produced. In a few of the top tier institutions, students undergo a program of courses, cross the hurdle of a qualifying examination and produce research that is usually published in peer-reviewed journals, before a doctoral thesis is written. The dissertation is then subjected to external review, with the reports being subjected to further internal scrutiny before a formal oral defense is conducted. This elaborate procedure can be subverted, at times, but the existence of a formal supervisory academic structure generally acts as a deterrent. In the best of institutions, seminars, group meetings and work presentations contribute substantially to a student's training. Unfortunately, many institutions have few formal requirements to be met and theses are almost entirely the responsibility of a student. Ph D training is a form of apprenticeship with students interacting closely with a mentor. Sadly, in a significant number of institutions mentorship is often lacking and the overall ambience is not conducive to academic research. In such situations the student often ploughs a lonely furrow. A major concern in our institutions today is the poor communication and writing skills of Ph D students. These deficiencies are hard to correct, but there is a pressing need to devise remedial courses that can address this problem, which is assuming epidemic proportions. The next generation of faculty in our expanding higher education system need to be able to teach, and teach well. Producing well trained technicians who will serve Western laboratories may hardly be a desirable goal.

Ph D students are the lifeblood of academic research. Ensuring their future must be a central concern of science policy discussions, which cannot ignore uncomfortable questions.

P. Balaram

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EDITORIAL

Books, Bronowski and the Ethics of Science

Book lovers can often be easily seduced by the appearance and feel of a new book. Today's bookshops have well stocked shelves of both new titles and old classics, reprinted in a most attractive manner. A recent discussion on TV focussed, somewhat surprisingly, in this age of scams and controversies, on the topic of e-books and electronic 'readers'. The discussion raised an interesting question: Would the digital revolution eventually consume the printed volume? Would books, as we know them now, cease to exist? In the field of scientific publishing scholarly journals are increasingly being accessed only on computers. The traditional journal may be close to extinction, as even conservative scientific societies begin to turn towards a business model that would eventually phase out the conventional printed journal. Science textbooks seem to be less endangered, although a new generation of students may hasten the end, when reading on computer (and tablet) screens becomes a way of life. The situation for books in general seems quite different, as bookshops overflow with new arrivals. As a compulsive book buyer, my thoughts turned to the future of books, as I tried to dust and rearrange my untidily stacked collection. Books unread, half read and read and long forgotten surfaced, as I struggled through the piles. In such tidying operations the best of intentions can be derailed by the sighting of an old favourite. My moment of distraction from the task at hand, came when I discovered an old, fraying and bedraggled copy of Jacob Bronowski's, *The Ascent of Man* (British Broadcasting Corporation, 1973). This is a book that I have mentioned in these columns in the years gone by (Science and the ascent of man, *Curr. Sci*., 2001, **81**, 1273; Science, technology and war, *Curr. Sci*., 2003, **84**, 859). I have used material from this book in lectures. Coincidentally I rediscovered old slides with quotations from Bronowski's book, even as I struggled to prepare a presentation to the Indian Women Scientists' Association, which met a few days ago in Mumbai, to address the complex issue of Science and Ethics. This is an area that Bronowski addresses, especially poignantly in the aftermath of the Second World War, illustrating the many moral dilemmas that confront the best of scientists in difficult times.

Invitations to lecture are often hard to turn down, especially when the request arrives many months in advance. Having accepted an invitation from the Women Scientists' Association, I made the cardinal error of not reading a long letter, which outlined the theme of the conference. To my vast discomfiture, I realized a day before the conference that the meeting was intended to address 'Science and Technology: Ethical Issues'. Reading and rereading the draft program, it became apparent that the conference would discuss both the 'ethics of science' and also 'ethics in science', which may be more appropriately cast as 'ethics of scientists'. Even as I contemplated these closely related, but undoubtedly distinct phrases, the power of the preposition in the English language became apparent. A consideration of the 'ethics *of* science' must presumably address the broad questions of the use of scientific advances in human affairs, most dramatically illustrated by the moral dilemmas faced in the late 1940s by the best of scientists, in the Manhattan project. There are the well known stories of Fritz Haber and Werner Heisenberg, remarkable scientists caught in the tangled web of historical circumstances. In more recent times, ethical questions have been debated not when science is involved in destruction, but ironically when the march of science has facilitated creation. In the years immediately preceding the successful creation of the world's first test tube baby, R. G. Edwards the recipient of last year's Nobel Prize in medicine or physiology had initiated a public discourse involving both clinicians and clergymen on the ethical issues that surround the entire area of assisted human reproduction.

As the time for delivering the lecture drew perilously close, the temptation to consider the alternative topic of 'ethics in science' seemed irresistible. This is indeed a simpler topic, easily understood, and of considerable recent interest. The current literature of science is replete with instances of misconduct, sometimes outrageous, at other times mildly transgressing well established requirements for reporting scientific data. Fabrication of data and plagiarism are eventually detected and the papers and authors discredited by their peers. The current debates on misconduct appear to centre more on institutional responses and punishment of offenders, both of which are difficult and uncomfortable issues for the scientific community. There are many other issues of ethics *in* science. These include 'honorary authorships', violation

of conflict of interest norms, especially in clinical research involving corporate pharmaceutical interests and misappropriation of intellectual contributions. Ethical issues also involve referees, editors and publishers. The process of 'anonymous peer review' is the foundation on which scholarly journals in science have been raised. Reviewers can sometimes be competitors, with the result that detachment is often a virtue in short supply. Editors are under constant pressure to raise impact factors of journals; a relatively new phenomenon in recent times, which has led to interesting examples of directed citations. Science publishing is a robust commercial enterprise, with monopoly publishers having a clear financial interest in the proliferation and aggregation of scientific journals. Recent cases of journals, notably in the area of biomedical research, have provided evidence for a new phenomenon which can only be labelled as 'publisher misconduct'.

A quick survey of the program for the symposium on 'Science and Technology: Ethical Issues' reassured me that the questions of ethics *in* science were well addressed. A day long session on 'Ethics *in* Medical Sciences' seemed to confront more complex issues. The topics suggested that no resolution would be possible, without an understanding of the more general issues of the ethics *of* science. Biomedical advance has raised ethical questions which are difficult to confront, in areas like surrogacy in assisted reproduction, prenatal diagnostic tests, genetic analysis which may provide foreknowledge of disease without a cure and the oft discussed right to 'die with dignity', in an age where medical technology can often prolong life under the most distressing circumstances. The contentious debate on the use of embryonic stem cells in research in the United States has really hinged on perceptions of when life begins.

In preparing for my lecture, I realized that any discussion of ethics in science must necessarily begin with a consideration of the meaning of 'ethics'. The dictionary provides a largely unhelpful interpretation, terming ethics as 'a system of moral principles'. In addressing abstraction, I turned to Einstein: 'Ethical axioms are found and tested not very differently from the axioms of science. Truth is what stands the test of experience'. These reflections seemed closer to the realm of philosophy, a subject far from my sphere of understanding. To return to firmer ground, I turned to a quote attributed, at times, to Richard Feynman: 'Philosophers of science are about as helpful to scientists as ornithologists are to birds'. Unable to proceed further to discuss the issues that formed the theme of the seminar, I was drawn to a famous statement made by J. Robert Oppenheimer in 1956: 'We have done the devil's work. Now we have come back to our real job, which is to devote ourselves exclusively to research.' There is a chilling quality to this view; an assessment of science and research that is unlikely to be shared by most scientists, over half a century later. Even as I struggled to put together a presentation it dawned slowly on me that I must turn to Bronowski. As one tempered in the cauldron of the run up to the Second World War, the war itself and its immediate aftermath, Bronowski is often sadly reflective. He quotes Leo Szilard after Hiroshima: 'It is not the tragedy of scientists. It is the tragedy of mankind.' There is something haunting when he describes Auschwitz: 'This is where people were turned into numbers. Into this pond were flushed the ashes of four million people. And that was not done by gas. It was done by arrogance. It was done by dogma. It was done by ignorance.' In writing this column, I searched the internet and quickly found a short clipping, where Bronowski stands in a slushy pond in the stark desolation of the long abandoned concentration camp, tracing what can only be termed the 'descent of man'. But there is one word that Bronowski uses which must strike a chord; that is the word 'arrogance'. In his compelling narrative Bronowski returns to this theme at the very end, in his assessment of John von Neumann. In Bronowski's words, Neumann believed in the 'aristocracy of the intellect'; 'a belief which can only destroy the civilization that we know. If we are anything, we must be a democracy of the intellect. We must not perish by the distance between people and government, between people and power. . . And that distance can only be conflated, can only be closed, if knowledge sits in the homes and heads of people with no ambition to control others, and not up in the isolated seats of power'. Bronowski's views on science evoke images that can only be uplifting: 'Science is a very human form of knowledge. We are always at the brink of what is known, we always feel forward for what is to be hoped. Every judgement in science stands on the edge of error, and is personal. Science is a tribute to what we can know although we are fallible.' At the very end he returns to ethics. 'We devise ethical strategies or systems of values to ensure that what is attractive in the short term is weighed in the balance of the ultimate long term satisfactions.'

Many public debates, some already underway, on science and society loom ahead in India. The use of genetically modified crops as the driver of food security, nuclear power as a key component to satisfy energy needs and climate change and conservation issues versus development strategies are already widely discussed. Protagonists on all sides of these debates may find echoes of their positions in Bronowski's reflective comments on science and ethics. Even as I left the podium after the lecture, a young lady walked up and took issue with my choice of Bronowski: 'He ignores India and her contributions'. This is indeed true, in his Eurocentric view of human progress with science as the catalyst of change at every turning point. But, we must remember that discussions of ethics and science arise largely in the context of the breathtaking advances of science over the past century.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Gathering Storm: A Case for Science

Memorable phrases, often found in the titles of books, reports, lectures and speeches attain immortality, being used and reused in varying contexts. Two of my favourites, that spring immediately to mind are both related to science. Vannevar Bush's famous characterization of science as an 'endless frontier' is one. The Bush report, produced in mid-1945, set the tone for the public engagement with science in the United States and in many ways transformed the way in which governments view science and technology. In his letter of transmittal Bush noted that 'science offers a largely unexplored hinterland for the pioneer who has the tools for his task'. Bush coined the phrase almost immediately after the power of nuclear fission was recognized. DNA was not yet established as the 'genetic principle'; Oswald Avery's work had been published, but largely ignored. The double helix and the molecular biology revolution lay far in the future. The antibiotic era was in its infancy. The transistor and the laser were still to be invented. Computers were on the distant horizon. The phrase, 'endless frontier' evoked an image of a subject whose horizons continually receded into a misty distance, even as the pace of scientific progress quickened. In a recent editorial Bruce Alberts asks a provocative question: 'Is the frontier really endless?' (*Science*, 2010, **330**, 1587). In reflecting on the vast amount of genetic information present in human cells, whose function we do not yet understand, Alberts notes that 'our "dark genome" – is needed for processes whose nature mostly remains a mystery'. He goes on to ask: 'Was Vannevar Bush right in 1945 when he described science as the "endless frontier"? Can we assume that if we solve the mysteries of the dark genome, new puzzles will arise?' Alberts is optimistic: 'One can certainly hope so, because life would lose much of its grandeur and joy if we ever reach the finish line, with no further frontiers to challenge us'. The 'endless frontier' must indeed be a descriptor of a horizon that forever remains elusive. The other phrase that I have always been attracted to is C. P. Snow's characterization of the apparently unbridgeable chasm, that existed at the dinner tables in Cambridge, between the humanities and the sciences as 'the two cultures'. Snow launched his now immortal phrase when he titled his Rede Lectures at Cambridge as 'The Two Cultures and the Scientific Revolution', in 1959. The 50th anniversary of the birth of this phrase was widely observed by many commentators in 2009. One observer noted that 'few literary phrases have had as enduring an afterlife as "the two cultures" coined by C. P. Snow', but added that 'few people actually seemed to read Snow's book bearing that title' (Dizikes, P., *New York Times*, Sunday Book Review, March 19, 2009). He went on to argue that 'the deeper point of "The Two Cultures" is not that we have two cultures. It is that science above all, will keep us prosperous and secure'. Curiously, the concluding sentence of Vannevar Bush's letter of transmittal accompanying the 'endless frontier' report anticipates Snow: 'Scientific progress is one essential key to our security as a nation, to our better health, to more jobs, to a higher standard of living, and to our cultural progress'.

In 2005, the National Academies of the United States produced a report on the challenges faced by the science and technology enterprise in America, as a result of growing global competition and diminished national interest in science education and research. The report ominously titled *Rising Above the Gathering Storm* used a phrase borrowed from Winston Churchill's first volume, in his epic six part account of the Second World War. Churchill was a master of rhetoric and the captivating turn of phrase. His critics, and there were many, often accused him of letting his penchant for theatrics cloud his view of facts. *The Gathering Storm* is a volume that details the political and social developments in Europe in the period between 1919 and 1939; a time when the author appeared to have been put out to pasture, in the political wilderness. Churchill constantly warned an unheeding public of a dark future, as Germany rearmed under the Nazi leadership, while Britain clung to the comfortable vision of a 'world without war'. Churchill's famous phrase, resonant in its grim air of foreboding, has been used in many contexts. The report of the National Academies uses the Churchillian phrase to focus attention on the apparent perception of a future that threatens the preeminence of American science and technology. The 2005 report, published as a book in 2007 (*Rising Above the Gathering Storm: Energizing and Employing America for a Better Future*, National Academies Press, Washington, DC, 2007) appeared at a time when the Chinese surge in scientific activity was beginning to be perceived as a competitive threat. Although the economic downturn and the current recession had not yet set in, worrying trends in

the sphere of science education and research seemed evident. Five years later, in the shadow of the ongoing economic crisis a second report appeared recently. *Rising Above the Gathering Storm, Revisited: Rapidly Approaching Category 5* (National Academies Press, 2010) reviews progress made in the five years since the original call to action. Both reports must be required reading (and subsequent reflection) for policy makers in India where the science, technology and education enterprise may have already entered 'the storm'.

The 2005 report by the US National Academies was a direct outcome of questions posed to the science bodies by members of the US Congress. Two questions were asked: (1) 'What are the top 10 actions, in priority order, that federal policy makers could take to enhance the science and technology enterprise so that the United States can successfully compete, prosper, and be secure in the global community of the 21st century?' (2) 'What strategy, with several concrete steps, could be used to implement each of those actions?' These questions are general enough to be relevant anywhere, especially in India where so much seems to be desired in the area of science research and education, but no well thought out, purposeful plan of action seems to emerge. In the 2010 review, the 'four overarching recommendations' of the original report are summarized: (1) 'Move the United States K-12 education system in science and mathematics to a leading position by global standards'. (2) 'Double the real federal investment in basic research in mathematics, the physical sciences and engineering over the next seven years (while, at *a* minimum, maintaining the recently doubled *real* spending levels in the biosciences)'. The parenthetical statement is a clear acknowledgement of the dramatic investments in biomedical research in the United States over the past decade. (3) 'Encourage more United States citizens to pursue careers in mathematics, science and engineering'. (4) 'Rebuild the competitive ecosystem by introducing reforms in the nations tax, patent, immigration and litigation policies'. Clearly, science policy makers in America are uncomfortable with overly restrictive immigration policies which limit the pool of researchers.

The 2005 report listed '20 specific implementing actions which included programs to create a new generation of science and mathematics teachers and to "strengthen the skills of 250,000 current teachers" by encouraging and subsidizing master's degree programs and enhancing "participation in workshops", specific recommendations for research funding ("sowing the seeds") and enhancing higher education ("best and brightest"). A particular point of interest in India would be the resolve to "improve visa processing for international students". Several "incentives for innovation" were also proposed. The 2010 review notes, somewhat dramatically, that most actions to boost science and technology competitiveness in the last five years have been taken in other countries and lists "Saudi Arabia, China, Russia, India, Luxembourg and the United Kingdom". India is credited with launching "a multi-year initiative . . . to make the country a global nanotechnology hub including constructing 14 new "world class universities" '. The National Academies report then lists a number of consequences of the economic downturn and makes a curious comparison with other countries which also struggle in an adverse economic climate: 'Yet many such nations do not face the task of sustaining the lifestyle which has come to be enjoyed – and expected – by America's citizenry'. The 2010 review notes that 'a paradox exists in the debate over whether there is a shortage of scientists and engineers or whether there are too many scientists and engineers for the jobs that are available'. The conclusions of the 2010 review are undoubtedly intended to raise the level of investment and public commitment to the science and technology enterprise in the United States: 'In balance, it would appear that overall the United States long term competitiveness outlook (read jobs) has further deteriorated since the publication of the *Gathering Storm* report five years ago'. There is a Churchillian tone in the concluding sentence: 'The *Gathering Storm* is looking ominously like a Category 5 . . . and, as the nation has so vividly observed, rebuilding from such an event is far more difficult than preparing in advance to withstand it'. 'Category 5' refers to the highest intensity of hurricanes that often form in the Atlantic Ocean.

Those interested in science policy issues would do well to read the 2010 review of *The Gathering Storm* report. It provides insights into the formulation and implementation of policy in an advanced country, which does not confront the multitude of problems that need to be addressed in India and the many countries of the developing world. We need to assess and review our policies for investments in science and higher education. Far too often, populist public pronouncements result in grandiose, ill thought out schemes which cannot be implemented. The '14 world class universities' are only one example of desire that will remain unfulfilled in the absence of a purposeful plan of action. Setting unrealistic goals serves little purpose. *The Gathering Storm* report provides many quotes, some of which are strangely reminiscent of the ideas espoused by Vannevar Bush and C. P. Snow several decades ago. In India, as in other countries, the proponents of science have always argued for enhanced investments in research. Today it is clear that even greater attention needs to be paid to the education pipeline, which will produce the scientists of tomorrow. Is money and more money the solution to the problems that affect scientific research and higher education in India? Money is undoubtedly necessary, but by no means sufficient to ensure progress. Ernest Rutherford, quoted at the start of the 2010 National Academies review said it well: 'Gentlemen, we have run out of money. It is time to start thinking'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

IIT, MIT and the Evolution of Institutions

Technical education in India was transformed in the post-Independence era by the creation of the Indian Institute of Technology (IIT). The need for a thrust in engineering education was recognized as early as 1945, when a committee, headed by Nalini Ranjan Sarkar was set up to examine the requirements of post-Independence India. Planners need models and the Sarkar Committee chose MIT (The Massachusetts Institute of Technology) as an example to follow, developing an engineering curriculum that had a strong science base, with courses in humanities and an evaluation system that differed dramatically from the annual examination approach favoured by most existing institutions. IIT Kanpur, which I entered in the late 1960s, embodied in many ways an entirely new experiment in science and engineering education. Half a century later the IITs have an image that reflects the success of these experiments, in undergraduate engineering education. In his book, *An Eye for Excellence* (HarperCollins, 2008), E. C. Subba Rao notes that the Kanpur Indo-American Program (KIAP) provides an example of 'how the dynamics of a four-cornered configuration – IITK, consortium universities, governments of India and the US – were adroitly managed … an instructive story for all inter-institutional and inter-governmental collaborations'. He goes on to quote the US Ambassador to India in the early 1960s, John Kenneth Galbraith: 'Not often, and perhaps not ever, has an economic and technical effort so rewarded both the donor country and the major recipient.' In an incisive analysis entitled 'Exporting MIT: Science, Technology and Nation Building in India and Iran', S. W. Leslie and R. Kargon examine the events that led to the establishment of IIT Kanpur and provide an assessment decades later: 'Pioneering programs in aeronautical engineering, computer science and materials science, so effective at MIT, turned out students overqualified at home and best prepared for graduate training and eventual employment abroad.' They go on to quote a 'common witticism in India', clearly one which must have emerged from the IIT itself: 'When a student enrolls at an IIT, his spirit is said to ascend to America. After graduation his body follows' (*Osiris*, 2006, **21**, 110). Leslie and Kargon observe that 'The Massachusetts Institute of Technology (MIT) redefined engineering education in the 1950s, then became a model and mentor for the rest of the world in the 1960s and 1970s.' The 'MIT model', envisioned and implemented in the 1950s provided 'a solid foundation in advanced mathematics and fundamental science' for prospective engineers. Gordon Brown, a key figure in the transformation of the curriculum, hoped that 'students would acquire the purposefulness, the creativity and the sound judgement found in the brilliant engineering of science – and become men who get things done' (*Technology Review*, 1959, **60**, 19). The vision of 'a university polarized around science' is described by Leslie and Kargon as 'a place where the basic sciences encompassed and contributed to interdisciplinary centres, constituent departments and education at all levels'. They go on to note that MIT was the best positioned amongst America's universities to address the challenge posed by President Truman in his 1949 inaugural address: '… to embark on a bold new program for making the benefits of our scientific advances and industrial progress available for the improvement and growth of underdeveloped areas'. The late 1940s and early 1950s were fundamentally transformative in science and in world affairs. The electronics revolution and the antibiotic era were about to begin; physics, chemistry, biology and a host of other disciplines were set to explode and many new nations were born, India and China amongst them, creating a new world order. In engaging with a changing world, Leslie and Kargon note, with a touch of surprise: 'Oddly enough, given the relative numbers and reputation of their faculties, the work of MIT's social scientists has overshadowed the arguably more enduring foreign policy legacy of its engineers, who believed that the institute itself could be a powerful model for economic development and nation building.'

The very first IIT at Kharagpur 'got no direct advice from MIT'. Kanpur was the chosen location, where a direct engagement by MIT was sought. In the words of Leslie and Kargon, 'the Indian government did its best to make MIT an offer it could not refuse'. The first delegation to assess 'the prospects for IIT Kanpur' was led by Norman Dahl, a mechanical engineer. Their conclusions were positive, especially after a meeting with P. K. Kelkar. The desire to push ahead with 'strong graduate and faculty research programs' seemed a departure from existing models in India. Dahl believed that India in the 1960s needed ' "problem recognizing" and "problem solving" graduates who will have the confidence, inclination, and training to do something about India's problems'. His colleagues were skeptical. Leslie and Kargon

quote from Robert Halfman, who would later oversee the KIAP: 'I have come to realize that Indian culture … is in many ways only superficially responsive to the objective techniques of science and engineering. The capable, modern, imaginative engineer with initiative is a misfit, a man a little ahead of his time who must have courage, perseverance and patience in the face of endless frustration'. American prejudice, apparent in this cautionary assessment would undoubtedly have been reinforced by a glimpse of Kanpur, which presented a stark contrast to MIT's surroundings in Cambridge, Massachusetts. Retrospective views of the Kanpur experiment have sometimes worried that the MIT model may not have been as influential, as originally envisaged in the Indian context. P. V. Indiresan and N. C. Nigam provide a view that is cited by Leslie and Kargon: '[It is] critically important for the faculty and staff to develop a pride in the Institute as an *Indian* Institute of Technology, not as an imitation of some foreign technological institute. This entails an orientation towards problems confronting India and a realization that the development of an Indian technology for dealing with Indian problems can be both interesting and exciting.' They note that IIT Kanpur's 'constituency is India and the Indian people' (*The Indian Institutes of Technology: Excellence in Peril in Higher Education Reform in India*, Chitnis, S. and Albach, P. G. (eds), New Delhi, 1993, pp. 334–363). This sentiment is true for all our institutions and especially relevant in today's context, where energy, water and health research appear to be central to future national development. The IITs, specifically the first five institutions, are undoubtedly the pride of India's higher education system today. My attention was drawn to the Leslie–Kargon article on IIT Kanpur's origins by a book about MIT, that appeared on my table.

Becoming MIT: Moments of Decision (Kaiser, D. (ed.), MIT Press, Cambridge, Massachusetts, 2010) is a celebration of MITs 150 year history. The book is a fascinating collection of articles which present a broad view of MIT's evolution. An introductory essay notes that at the centennial, half a century ago, the MIT President Julius Stratton hoped that sometime in the future we might have 'a coherent account of the flow of ideas that have influenced academic aims and the methods of teaching at the Institute throughout the century'. He was clear that academic evolution 'can be understood only when placed in the context of the great industrial and intellectual movements of the times'. David Kaiser's introduction notes that the individual chapters 'interrogate those moments of decision that have helped to define the Institute [MIT] we know today. Some of these turning points have been dramatic, their import obvious to participants at the time. Others have been more subtle, their full impact only recognizable with hindsight. Moments of intense strife and uncertainty take their place … alongside some of MIT's great successes'. How does one choose 'moments of decision' in tracing the evolutionary history of an institution? Kaiser notes that 'though invoking the past, anniversary celebrations like all history, are conducted in the present with an eye to the future'. He adds that 'we always strive to craft a usable past – one whose highlights and contours are shaped by our current concerns and anticipations'. Having reflected, albeit with minimal pretensions to historical scholarship, on the century long progress of the Indian Institute of Science, I was drawn to Kaiser's view of institutional evolution: 'Episodes and individuals that once loomed large in the collective memory inevitably fade from view, our sense of meaning shading, at times imperceptibly upon each reconsideration.'

MIT's 'foundational years' span the years 1861–1894 and the story as told by Merrit Smith is a story of an individual, William Rogers, pursuing a dream of a 'polytechnic school' even while the American Civil War was raging. MIT's first laboratory (1867) was set up in chemistry by Francis Storer. Physics appeared shortly thereafter, focusing on laboratory experience for students. Even in this early period MIT seems to have experimented with curricula and teaching methods, directly confronting the competing demands of laboratory work and classroom teaching. The chemist Charles Eliot went on to note in his inaugural address as Harvard's president, in 1869, that the problem was 'not what to teach, but how to teach'. Nearly 150 years later, I suspect that this problem has become even more acute, as scientific advances have far outstripped the pace of reform in curricula and teaching methodology. MIT began with 'fifteen students in 1865' and 'grew to three hundred in 1881'. Merrit Smith points out that the Latin words on MITs seal, *Mens et Manus*, translates as Mind and Hand. This phrase in his assessment 'aptly expressed the defining academic values of MIT and the aim to produce both cutting-edge scientists and engineers'. A brief essay entitled 'Mergers and Acquisitions' recounts the attempts to merge MIT with its older, richer and more established neighbour, Harvard. MIT operated largely as an 'undergraduate engineering school' in its early years. The transformation to a research university happened in the years between 1910 and the late 1930s. These years were marked by MIT's increasing involvement with industrial research, but the truly transformative period lay just ahead. The war years impacted MIT in a manner that few institutions have experienced. The 'military–industrial–university complex' was a phenomenon without precedent, driving scientific and technological advance at a rapid pace, but leading to a loss of innocence, which would trigger, at times violent, confrontation in the Vietnam years of the late 1960s and early 1970s.

Evolutionary history can be instructive, even for institutions. We can only hope that recognition of the turning points in the growth of our institutions will one day provide a framework for charting a future course.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Indian Science: The Need for Public Engagement

Scientists, especially the organized and apparently exclusive bodies that seem to be repositories of expertise, suddenly appear to be besieged by a blitz of critical scrutiny in India. The controversy over the *Bt* brinjal report, produced by six scholarly academies – whose membership spans the fields of science, engineering, medicine and agriculture – has been quickly followed by the 'S-band spectrum' saga, which has raised many questions on the credibility of decision making in large, monolithic organizations, that have acquired a special status over the years. Even before the 'brinjal' and 'spectrum' stories became the staple diet of media, hungry for scandal and sensation, the credibility of the IPCC projections on glacier melting was questioned, raising doubts on the abilities of scientific bodies to provide accurate and carefully worded assessments on matters of great public interest. The last few days have seen an escalation of the criticism that Indian scientists do not engage in contributing meaningfully to public debate and that scientific assessments on matters of public importance are sometimes provided in a casual fashion, with little regard for the complexity of the issues involved. The credibility of advice received from scientific bodies was severely dented in public perception by the dismissal of the Academies' *Bt* brinjal report by the Minister for Environment and Forests, Jairam Ramesh, as a document without substance. More recently, the Minister has labelled 'environment impact studies' 'a bit of a joke'. He is reported to have said that 'even reputed government institutions do cut and paste jobs' (*Times of India*, March 20, 2011); a clear reference to the reluctance of many groups to conduct a serious, independent study of the issues involved. The tensions between the imperatives of development and the growing awareness of the need to protect the environment make it very difficult to produce and accept scientifically dispassionate impact assessment reports on highly visible infrastructure projects. Beleaguered by many recent controversies, India's science bodies may have little desire for public engagement.

A pointed critique by Sunita Narain, who heads the Centre for Science and Environment, a non-governmental organization (NGO), begins provocatively: 'I suspect Indian scientists have retired hurt to the pavilion.' The cricketing metaphor is particularly apt in view of the great interest in ongoing World Cup matches. In assessing the response to the ill-fated ISRO–Devas contract she concludes that 'while there is more to the story' there can hardly be any 'informed discussion' if 'the scientists who understand the issue are not prepared to engage with the public'. She adds that 'the cynical public which sees scams tumble out each day, believes easily that everybody is a crook'. In a harsh commentary on the attitudes of the leaders of science, she argues that 'the country's top scientists have withdrawn further into their comfort zones, their opinion frozen in contempt that Indian society is scientifically illiterate'. She is pessimistic in her prediction, 'that in future there will be even less conversation between scientists and all of us in the public sphere' (*Business Standard*, March 14, 2011). Narain reflects on the revised Academies' report on *Bt* brinjal: 'The final report of the apex-science group is marginally better in that it includes citations but it reeks of scientific arrogance cloaked in jargon. . . . But the world has changed. No longer is this report meant only for top political and policy leaders who would be overwhelmed by the weight of the matter, the language and the expert knowledge of the writer. The report will be subjected to public scrutiny, its lack of rigour will be deliberated, its unquestioned assertion challenged'. Narain's conclusions merit attention amongst scientists: 'This is the difference between the manufactured comfortable world of science behind closed doors and the noisy, messy world outside. It is clear . . . that Indian scientists need confidence to creatively engage in public concerns. The task to build scientific literacy will not happen without their engagement and their tolerance for dissent. The role of science in Indian democracy is being revisited with a new intensity. The only problem is that the key players are missing in action.'

Coincidentally, even as Narain charged scientists with being unwilling to engage in public debate a piece authored by J. Gowrishankar appeared in *The Hindu's* regular column 'Speaking of Science'. This opinion piece entitled 'GM crops debate: consensual versus adversarial approaches' advances an interesting view that scientific progress relies on a consensual approach and not the adversarial approach, 'as adopted in wars between nations, in courts of law between opposite parties, and in democracies, between Government and the opposition'. He argues, and here I suspect that many scientists will disagree that 'peer review represents the epitome of a

consensual approach in scientific discourse'. He is right when he says that 'adversarial arguments begin from an assumption of mistrust between the parties'. He may be naive when he suggests that the 'consensual approach' favoured by scientists 'rests on an assumption of trust and all the parties are expected to self-declare conflicts of interest'. As commercial interests dominate many areas of scientific research 'conflicts of interest' are often suppressed, sometimes emerging only when a controversy erupts. In Gowrishankar's analysis: '. . . several aspects of the debate in this country on genetically modified (GM) crops and foods have adopted the adversarial approach rather than a consensual one. Furthermore, academic scientists have been drawn into the discussions on unfamiliar terms and territory that have been dictated by activists and the lay public. . . . Scientists are not trained to hold placards, shout slogans, mobilize crowds, or denigrate their so-called opponents and hence have been unable to match the activists in their strategies and actions. Indeed, the scientists of this country are being exposed for the first time to practices previously encountered by their counterparts in the developed world' (Gowrishankar, J., *The Hindu*, March 10, 2011).

Are there well documented examples where Western scientists have allayed the fears of the lay public about new technologies? By a curious coincidence, an account of the history of MIT that I alluded to in my previous column (*Curr. Sci*., 2011, **100**, 599) describes an example of 'How community outreach catalyzed the life sciences at MIT' (Durant, J., In *Becoming MIT: Moments of Decision* (ed. Kaiser, D.), The MIT Press, Cambridge, Massachusetts, 2010, pp. 145–163). The early 1970s saw the birth of 'genetic engineering' and 'recombinant DNA technology'. The ability to create 'hybrid DNA' raised the question of whether organisms with modified genetic characteristics could pose a hazard. Two scenarios appeared troubling. Durant lists them: 'What if genes conferring resistance to specific antibiotics were recombined into naturally pathogenic bacteria? Or what if genes causing cancer were transferred from viruses into the normally harmless bacteria that inhabit the human gut?' These and other scenarios were discussed within the growing community of 'molecular biologists' in the United States to weigh the risks of many proposed lines of research. In a famous letter that appeared in 1974 a panel led by Paul Berg proposed a 'voluntary moratorium on designated categories of rDNA experiments and urged the establishment of an experimental program at a secure facility to assess potential hazards'. Durant notes that 'many of the scientists who became involved in the rDNA debate in the 1970s worked with a heightened sense of social responsibility that had everything to do with their experiences of the movements and political battles of the late 1960s, especially the role of science and technology in the Vietnam War'. The 'adversaries' in the early phase of this debate were from within the community of biological researchers. Many young 'activists' within the community would later embrace the new technologies and contribute immensely to the scientific resolution that followed, which led to modern biotechnology. While the Berg letter was intended to initiate informed debate within the scientific community it was not long before 'there was a full-blown public controversy over wider health, environmental and defense issues as well as the role of citizens and their representatives in helping to determine what sorts of DNA research should be done'. Durant describes the public hearings held in Cambridge, Massachusetts in mid-1976, which would decide the future of molecular biology research at Harvard and MIT. The hearings were presided over by Cambridge's colourful Mayor Alfred Vellucci. His opening words may have some relevance in the present context of science and public policy in India: 'The subject matter . . . is important to all of us. No one person or group has a monopoly on the interest at stake. Whether this research takes place here or elsewhere, whether it produces good or evil, all of us stand to be affected by the outcome. As such, the debate must take place in the public forum with you the public taking a major role'. Durant describes the unprecedented public engagement between the proponents and opponents of rDNA technology which eventually led to a resolution in early 1977. Durant suggests that there are lessons to be learnt from this story. The Cambridge Experimentation Review Board (CERB) which included many members of the lay public was viewed by some 'as an inspirational model of how citizens can engage constructively with scientists in decision making on socially and politically sensitive areas of research'. Such an engagement is possible only if scientists follow Mayor Vellucci's memorable injunction at the start of the public hearings: 'Refrain from using the alphabet. Most of us in this room . . . are lay people. We don't understand your alphabet, so you will spell it out for us so we know exactly what you are talking about because we are hear to listen.' In reflecting on MITs response to the controversy, Durant notes: 'The Institute rose to this challenge. It did not retreat into the ivory tower. Instead, it reached out in a process of constructive engagement with citizens and community representatives.'

Narain's charge that Indian scientists have 'retired hurt' into the seclusion of their laboratories and Gowrishankar's plea for 'civilized debate' drawn attention to the great need for scientists to engage actively in enhancing the public perception of science and its role in development. The range of contentious issues keeps increasing. We will have to engage in public discussion of the safety of nuclear power, genetically modified crops, climate change mitigation strategies, ethical issues in health research and implications of nanotechnology in the future. This engagement will require scientists and their institutions to return to the field of play.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Luring the Overseas Indian Scientist

Not a day passes without policy makers calling for a dramatic expansion of our university system, to meet the aspirations of a rapidly growing population of young Indians. The need for expansion is widely recognized and accepted. The first steps have already been taken. New IITs, IIMs, IISERs, Central Universities and several private institutions are springing up at locations dotted across the country. Older institutions are in the throes of expansion and many new schemes are on the anvil to inject funds and purpose into a university system that is plagued with a multitude of problems. A recurrent theme for discussion is the shortage of faculty to staff the new institutions and to sustain and expand existing ones. Where did faculty come from when India embarked on the first phase of expansion of scientific and technical institutions in the 1950s and 1960s? A very large number of new recruits in those days were educated in the West, although ironically some of the very best were homegrown. That generation has largely passed from the scene; academicians to whom must be given much of the credit for building the most visible of our current institutions. Unfortunately, not every institution that sparkled with promise in the first two decades after independence has been able to sustain the enthusiasm and optimism of that era. How will we address the problem of faculty shortages today? The consensus, one that leaves me mildly uneasy, is that we must make vigorous attempts to entice Indian students and academics who are currently overseas, primarily in the United States, to return and build teaching and research careers in India. I was, therefore, struck to receive a report, coauthored by a group drawn from the Tata Institute of Social Sciences, Mumbai, Penn State University and Rutgers, entitled: 'Will They Return?' This study was intended to address 'the willingness of potential faculty to return to India and the key factors affecting their decisions' (Venkatesh Kumar, B., Finegold, D. L. and Winkler, A.-L., http://smlr.rutgers.edu/research-and-centers/smlr-research).

The authors draw upon a FICCI report (*Making the Indian Higher Education System Future Ready 2010*) to set the stage for their analysis. They use the FICCI data to project a need for an additional 800 universities and 41,000 colleges by 2020. The existing figures cited are 504 universities and 26,000 colleges. They note that 'perhaps the biggest constraint on being to be able to meet these ambitious growth targets, while improving rather than diminishing quality, is the availability of a sufficient supply of well qualified faculty members with advanced degrees'. Universities must necessarily recruit faculty with Ph Ds and even undergraduate colleges would benefit by increasing the number of faculty with Ph D degrees, with an interest in research. Venkatesh Kumar *et al.* provide estimates of a staggering faculty shortage and argue that Indian Ph D production rates are unlikely to meet the demands of the Indian academic sector. Curiously enough, even Ph Ds from the best of our institutions will find it difficult to obtain academic positions, given the range of institutional constraints in recruitments. These constraints are not addressed in the analysis and the authors go on to suggest that 'it is vital that [India] be able to attract back some of the thousands of Indian students who left the country over the last several decades to obtain graduate degrees abroad'. The questionnaire based survey is the staple of studies in social sciences. The Rutgers study uses responses from 998 'current or recent Indian students'. The authors note that 'two-thirds of those surveyed are current graduate students – 40% Masters and 26% Ph D – with another 8% completing a postdoc. As expected, 'nearly three-quarters of the sample is male (73%)' and interestingly 'the vast majority (85%) are under the age of 30—52% are 20–25, 33% are 26–30, with another 10% 31–35'. The demographic and educational profile of the respondents may turn out to be one of the key factors diminishing the relevance of an important study. Academic faculty and researchers in national laboratories are generally drawn from the large pool of Indians engaged in post-doctoral research in the United States, most of whom would have crossed the age of 30. This group is extremely underrepresented in the sample studied. The large number of Master's students surveyed are unlikely to be potential candidates for faculty positions in India in the immediate future. In engineering only 24% of those surveyed were pursuing Ph Ds or in post-doctoral positions; in the natural sciences this number was 79%. In the survey sample engineering students (478) vastly outnumbered the science students (98). Interestingly, the vast majority cite 'high-quality teaching' and 'cutting edge research' as the reasons for choosing

the US as a destination for graduate education. Only a very small number cite the 'desire to find a job and emigrate to the US' as a motivating factor. Most respondents (74%) indicate a desire 'to return to India eventually', a finding that the authors describe 'as encouraging news for Indian policy makers'. I suspect that if a similar survey had been carried out a decade ago, or even earlier, a very similar pattern of responses would have been received. Unfortunately, the nature of the sample surveyed seems to defeat the very purpose of the study which was intended to guage 'the willingness of potential faculty to return to India'. Only 36% of those pursuing science degrees and 16% of the engineering students indicated a definite desire to return, with significant numbers being undecided. The survey summarizes the main factors influencing the decision to remain in the US. The three most cited reasons are 'corruption', 'amount of red tape' and 'academic work environment'. Why do Indians wish to return from the United States? Most respondents cite 'family reasons', 'giving back to the motherland' and 'comfort with society/culture'. Many respondents appear to express a desire to contribute to the Indian higher education system. The authors provide a wide range of suggestions to attract expatriates to return. An interesting suggestion that caught my attention was the creation of a 'Teach for India' – higher education fellowship, with the opportunity to do research. In suggesting ways to attract 'older Indian academics' to return to their country, the authors perceive a 'knock-on benefit of freeing up more openings for new graduates in the US'. The absence of adequate research opportunities and poor governance seemed to concern most respondents.

China has been particularly aggressive in persuading Chinese–American scientists to return. There has been an organized hunt for high profile researchers, who have been enticed to return, being offered benefits and inducements that might even attract the attention of the best of Western scientists. A recent report draws attention to the growing problem that 'high-priced recruiting of talent abroad raises hackles' (Hao Xin, *Science*, 2011, **331**, 834). The price tag for winners of 'prestigious international prizes – including the Nobel prize' is stated to be '150 million yuan ($23 million), with China setting a target of 10 such recruitments. The most decorated scientists in China may then command benefits that should bracket them with European football stars and Indian cricketing icons. An earlier program (Qianren Jihua) for 'recruitment . . . of global experts' launched in 2008 had a goal of hiring 'up to 2000 experts from abroad over 5 to 10 years'. The program has already notched up 1143 recruitments but the scheme seems to be 'foundering'. The *Science* report highlights the growing criticism of the attempts to lure overseas researchers who are already well established, while underplaying the need to support researchers working in China. The critics are trenchant. In describing Chinese initiatives as 'a massive waste of resources' one observer notes 'that it is better to invest in a whole new generation of talent than to buy reputation'. Mu-Ming Poo, a prominent neuroscientist in Shanghai, is reported to have characterized the Qianren Jihua program as 'a huge disaster', arguing that China's current policy tells 'the best and brightest to spend most of their productive years abroad'. Inevitably, the new schemes to attract overseas talent have created divisions within the research community, with the incentives sometimes attracting many with undesirable qualities. The Chinese experience may be worth studying and there may be much to learn, even as Indian agencies formulate new schemes to persuade overseas Indian scientists to return.

In recent years several new schemes have been introduced to attract and support young researchers, who choose to explore career opportunities in academic institutions. The Ramanujan fellowship of the Department of Science and Technology (DST) and the Ramalingaswami and DBT–Wellcome fellowships of the Department of Biotechnology provide both salary support and moderate research grants, primarily for starting researchers. Schemes for attracting 'senior scientists' are also on the anvil. A recently advertised program called the Margdarshi Fellowship announced by the DBT appears to be directed at established overseas Indians. Curious about the origins of the name, I made discreet enquiries and was informed by a knowledgeable colleague that 'margdarshi', a Hindi word, could be loosely translated as 'one who shows the path'. Undoubtedly, we are now set on a path by which our way forward will be illuminated by a new cadre of returnees. This scheme is also part of DBTs alliance with the Wellcome trust, all of whose initiatives are inevitably focused on those who return from the UK or the USA. Will the induction of senior scientists from overseas, with large research grants and fellowships, have an impact on our existing institutions, many of which are relatively old with large numbers of faculty already in position? If the Chinese experience is any guide, I suspect that there will be both resentment and criticism. We need to have schemes to retain, absorb and encourage the increasing number of Ph Ds being produced in our own institutions. A growing number of women with Ph D degrees are sometimes unable to spend extended postdoctoral periods overseas. Should there not be a mechanism which allows us to tap this resource and support their research efforts? Strangely, while schemes for attracting overseas talent are enthusiastically administered in the funding agencies, initiatives that promote local talent are invariably run with limited interest and efficiency. Looking outward may be attractive and fashionable. Looking inward may be desirable and essential.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Maxwell and Faraday: The Anniversary of a Unification

Colloquia, where accomplished researchers present an overview of their work to an audience of colleagues from other disciplines, are presumably a device to promote a broad understanding of diverse activities in academic institutions. At a recent event in my own institution, a distinguished and cheerful mathematician presented a talk with a deceptively simple title: 'Differentiation?(!)'. Despite an intrinsic fear of mathematics (and sometimes, mathematicians) I tried to concentrate on the talk. Formidable terms like Mobius groups, Hilbert space and the Weyl commutation quickly made me realize that I was witnessing a trip into an academic space that required a background that I quite simply did not possess. Mathematics in all forms, but especially so in its pure form, seems austere and remote, with the promise of forbidding beauty, that only its most devoted practitioners are privileged to behold. As is customary, there was a time for questions and answers. In seminars, this is a session that is often both enlivening and enlightening. I was struck that among the animated discussants was a young theoretical physicist, a string theorist, confirming a suspicion that many distant observers must share, that the frontiers of theoretical physics merge seamlessly into esoteric mathematics. In thinking about subjects that I know very little about, I could not help recalling a metric often used by a thoughtful colleague of mine (a physicist, of course) in judging prospective applicants for academic positions in physics: 'Can the candidate teach Maxwell's equations to students?' In these days of interdisciplinary research, faculty are often recruited on the basis of lists of publications acquired during doctoral and post-doctoral years, together with their ability to present their past work and also envision their future research. The most glamorous publications are often the result of group effort in well organized laboratories, with considerable depth of expertise. Judgements about an individual researcher's potential and promise are often hard to make. Was my colleague's metric in assessing a fundamental background in physics robust and reliable? Why did my colleague seem so convinced that an understanding of Maxwell's equations provides an assurance of a firm grounding in the foundations of physics? When did mathematics and physics become so inextricably linked? Was there a time when physics advanced without the support of a firm mathematical underpinning, driven only by experiments and observation? Did it not appear that Newton's invention of the calculus, driven as it seemed to be by his insights into natural phenomena, was really a means of codifying and extending a domain of human endeavour that seemed rooted in observation? An anniversary came to my rescue in providing answers to some of these questions. Almost exactly 150 years ago, James Clerk Maxwell (1831–1879) published a paper in the *Philosophical Magazine* with a title, 'On Physical Lines of Force', that seems both cryptic and understated by modern standards. Between 1861 and 1862 Maxwell effected a profound transformation of science by unifying electricity, magnetism and light; subjects that even a century and a half later are sometimes still introduced as distinct and unrelated topics. A recent editorial in *Nature* (2011, **471**, 265) celebrates the anniversary by asking a question: 'What is it that makes physicists proud to be physicists?' The answer, most certainly 'one answer', must lie 'in James Clerk Maxwell's equations. Physicists can rejoice in a historical moment of great insight, can share in the expressions of that insight that only they can understand in any depth . . . and, above all, can roam freely in deploying the power thus provided for understanding the world and, on occasion, changing it'.

Public perception about scientists and their impact can be dramatically different from those of scientists themselves. In 1985, *New Scientist* reported the results of a poll where people were asked to name 'three famous scientists, living or dead'. The clear winner was 'Don't know', polling 47% of the vote. Trailing far behind were Einstein (28%), Newton (13%) and Fleming (11%). Faraday polled only 4%, while Darwin (2%) was even further behind (*New Scientist*, 1985, February 21, p. 16). Maxwell was not on the list, an omission, I suspect, which would undoubtedly be repeated today. I was fortunate to lay my hands on a marvellously written, engaging biography, *The Man Who Changed Everything: The Life of James Clerk Maxwell* (Mahon, B., Wiley, 2003). The author, Basil Mahon, an engineer, paints a warm and affectionate portrait of a man, who by all accounts was a genius, but also 'the warmest and most inspiring of companions'. Biographies can often be heavy and dull. Mahon's book on Maxwell is fast paced and fascinating

in its description of one who was 'to physicists, easily the most magical figure of the nineteenth century'. Maxwell was, clearly, a precocious child publishing his first paper, 'On Rolling Curves' in 1848, at the age of 14. Mahon's account is captivating; the young Maxwell employing both experiment (drawing curves with pins, pencil and string) and mathematical insight to draw curves with multiple focal points. Professors at the University of Edinburgh then discovered that the only prior work in the area was by Rene Descartes; as a forerunner of things to come, Maxwell's construction was simpler and more general and the bi-focal curves had a 'practical application in optics'. More remarkably, early success did not influence his personality, ensuring that he constantly learnt from the work of his predecessors and contemporaries. In thinking about the role of experiments and theory in science, I found in Mahon's book an account of Maxwell's experience in Edinburgh as a student: 'James was not at all impressed by Professor Wilson's lectures in moral philosophy which, to his mind, served only to demonstrate that wooly thinking leads to wooly conclusions. He enjoyed chemistry but thought it odd that lectures from Professor Gregory were given separately from practical chemistry sessions under Mr Kemp, particularly as "Kemp the practical" was apt to describe procedures taught by Gregory as "useless and detrimental processes invented by chemists who want something to do". This experience helped to form Maxwell's conviction that practical work is not only essential to a proper scientific education, but should be part and parcel of the lecture course, not tacked on as an afterthought.' This appreciation of the importance of experimentation was to serve Maxwell well in later life, when he went on to establish the Cavendish laboratory at Cambridge, which in the 20th century was to be at the centre of two scientific revolutions, the first in atomic physics and the second in structural molecular biology. In a remarkable experiment, designed as a demonstration for a lecture at the Royal Institution in London, in 1861 Maxwell produced 'the world's first colour photograph'. This experiment could never be repeated; Mahon notes that 'the mystery was solved about 100 years later by a team at Kodak Research Laboratories'. The experiment had worked for the wrong reasons. Maxwell believed that one should 'never . . . dissuade a man from trying an experiment no matter how slim the prospect of success, because he might find something entirely unexpected'.

No retrospective of Maxwell's unification of electricity, magnetism and light can fail to be dazzled by the path illuminated by Michael Faraday, arguably the greatest experimental scientist of the modern era. His experiments demonstrated the magnetic 'lines of force' and he also postulated 'electric lines of force'. Reading Faraday's writings, Maxwell was struck by the description of 'his unsuccessful as well as his successful experiments and his crude ideas as well as his developed ones, and the reader however inferior to him in inductive power, feels sympathy even more than admiration and is tempted to believe that, if he had the opportunity, he too would be a discoverer' (Mahon, p. 59). This view of Faraday must be contrasted with Maxwell's reported characterization of Ampere's descriptions of his work: 'If you have built a perfect edifice, do not remove all traces of the scaffolding by which you have raised it'. The concept of 'fields' emerges from Maxwell's analysis of Faraday's classic experiments; 'a new and mathematically impeccable concept'. Maxwell's first paper in the area 'On Faraday's Lines of Force' used the analogy of fluid flow 'and did not offer even a semblance of a theory as to the nature of electricity or magnetism'. Faraday's reaction was 'charming': 'I was at first almost frightened when I saw such mathematical force made to bear upon the subject and then wondered to see that the subject stood it so well.' In *Michael Faraday and the Royal Institution* (1991, Adam Hilger, Bristol), J. M. Thomas notes that 'such was the prodigality of his [Faraday's] output and the diversity of his skills that modern chemists, no less than physicists, engineers and materials scientists regard him as one of the founders of their subjects: some sciences and technologies owe their very existence to his work'. Thomas goes on to add that 'in none of his four hundred and fifty publications is there a single differential equation, for he knew no mathematics'. He also notes Maxwell's view of Faraday: 'in reality a mathematician of a very high order – one from whom the mathematicians of the future may derive valuable and fertile methods'. But it is Maxwell who finally produced the four equations that Mahon describes as 'majestic mathematical statements, deep and subtle yet startlingly simple. So eloquent are they that one can get a sense of their beauty and power even without advanced mathematical training'. Maxwell's synthesis 'not only explained all known electromagnetic phenomena, it explained light and pointed to the existence of kinds of radiation not then dreamt of'.

Maxwell died young, even by the standards of the 19th century. His contemporaries and successors were unstinting in their admiration. Mahon quotes Oliver Heaviside (an 'acerbic and cynical man') who noted that men like Shakespeare and Newton 'live the best part of their lives after they are dead. Maxwell is one of those men'. Mahon's introduction borrows from Feynmann who said that 'the most significant event of the nineteenth century will be judged as Maxwell's discovery of the laws of electrodynamics'. Einstein, as always, is quotable: 'One scientific epoch ended and another began with James Clerk Maxwell'. To his admirers Maxwell's physics was poetry.

P. Balaram

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EDITORIAL

Elements: Impending Scarcity

Chemistry is a curious word, pregnant with many meanings. In one of the endlessly contentious, shrill and, at times, unedifying television discussions on corruption in public life, a visible and voluble politician held forth on the 'chemistry between lawyers and the judiciary', which apparently seemed to influence the outcome of legal proceedings. The use of the word 'chemistry', by someone unlikely to be schooled in the discipline seemed a clear indication of a widely held perception that the subject involved the study of 'good' and 'bad' interactions between substances or materials; an extension to human behaviour seems both inevitable and justified. The noisy discussion on corruption faded, leaving me to puzzle over the subject of chemistry; a topic that seemed especially relevant in a year that has been formally dedicated to the celebration of the subject. Chemistry is, almost always, introduced to students by describing the elements and the Periodic Table. Mendeleyeev's recognition of the periodicities in the properties of elements known in the mid-19th century and his conception of the 'Table' must surely rank as one of the pillars on which the edifice of modern science has been raised. But, the definition of the term 'element' has often troubled beginning students, most of whom forsake the subject at an early opportunity. Indeed, the word 'element' is used in many different contexts in English, outstripping 'chemistry' in its versatility. Arthur Conan Doyle made the derivative 'elementary' famous, in Sherlock Holmes' explanations for his many success: 'Elementary, my dear Watson.' 'Simple' seemed the synonym.

The elements when introduced in chemistry courses indeed seem simple, leaving unwary students quite surprised by the complexities that follow. The elements, the basic constituents of all matter seemed even simpler to early civilizations, where the material world really seemed to be largely composed of earth, air, water and fire. Paracelsus thought of the world in terms of salt, sulfur and mercury, in simpler times. Robert Boyle writing in *The Sceptical Chymist*, in 1661, began to herald the dawn of modern definitions of the elements: '. . .certain primitive and simple, or perfectly unmingled bodies, or of one another, are the ingredients of which all those perfectly mixt bodies are immediately compounded and into which they are ultimately resolved'. Modern chemistry was to emerge two centuries later when Dalton and Mendeleeyev midwifed atoms and the Periodic Table into the modern age. Even definitions of elements in modern chemistry textbooks sometimes seem ambiguous and incomplete: 'A chemical element is a substance that cannot be broken down by chemical means.' A qualifying sentence is invariably added: 'Elements are defined by the number of protons they possess.' This apparent quibble acknowledges the enormous progress in understanding the nature of the atom and the discovery of isotopes. The elements, inevitably gold, attracted the alchemists in the 17th and 18th centuries; Newton amongst them. Transmutation seemed a most desirable goal. In a BBC program entitled 'Chemistry: A Volatile History', Jim Khaleeli, a theoretical physicist presents an engaging account of the road to the elements. He waxes eloquent when he says: 'Chemistry was forged in the furnaces of the alchemists.'

Should one even consider a subject so elementary on the editorial pages of a scientific journal? What indeed is the provocation for this column? *Nature Materials* (2011, **10**, 157) in a recent editorial entitled 'Elements in short supply' noted that 'when we think of scarce natural resources, the availability of oil is the main one that comes to mind'. In noting that many scarce materials are 'valuable because of the chemical elements they contain', the editorial notes 'that oil is a molecule that is valuable for the energy stored in it and not for its carbon and hydrogen atoms'. Chemistry courses often focus on relatively few elements in Mendeleeyev's Table; organic chemists being especially choosy, while their inorganic counterparts range somewhat more widely. What are the elements that are valuable and scarce, whose looming shortages must worry governments and policy makers? To answer this question, I turned to a report produced by the US Department of Energy (DOE) (Dec. 17, 2010), which outlined a 'critical materials strategy'. Fourteen elements in the Periodic Table were marked as 'key materials'. These are: lithium (Li), cobalt (Co), yttrium (Y), gallium (Ga), indium (In), tellurium (Te), lanthanum (La), cerium (Ce), praseodymium (Pr), neodymium (Nd), samarium (Sm), europium (Eu), terbium (Tb) and dysprosium (Dy). The last seven elements, beginning with lanthanum, belong to the class of elements called 'rare earths', which are often intensely disliked by many students of chemistry. There is a certain monotony of properties that are normally discussed, which quickly induce a sense of boredom. Current discussions of 'critical' and 'strategic' elements are fuelled by the Chinese restricting rare earth exports, after having become the major supplier

of these materials, mining as much as '97% of the global rare earth material supply'. The device of 'squeezing any competition out of the market' by pricing ore very cheaply and then imposing export restrictions appears to have adversely affected global supply. The fourteen elements listed in the DOE report are crucial for 'clean energy' technologies, which are being so intensely sought. Solar cells (photovoltaic films) require indium, gallium and tellurium; magnets for both wind turbines and electric vehicles require praseodymium, neodymium, samarium and dysprosium; batteries for energy storage need lithium, cerium, praseodymium, neodymium, lanthanum and cobalt; phosphors in efficient fluorescent lighting require lanthanum, cerium, europium, terbium and yttrium. It appears clear that a clean future which minimises the use of fossil fuels, relies heavily on elements whose supply (and cost) is likely to be affected by looming shortages. The US DOE report does not discuss the actinide elements, uranium, plutonium and thorium amongst them, which are crucial to nuclear energy programs.

Elements and minerals from which they may be extracted are unevenly distributed across the globe. Not all known deposits can be easily mined. Extraction and processing can be both expensive and environmentally harmful. The DOE report summarizes information on many key elements, comparing both production and reserves in major producers. Interestingly, the US data for lithium and tellurium is listed as 'withheld', presumably for strategic reasons. Indeed in this area data is often 'proprietary' and not available. China holds an almost complete stranglehold on rare earth production and is also a major producer of lithium, cobalt and gallium. India is listed only as a minor producer of rare earths, a clear indication of scarcities which may loom in the future. The DOE report presents an interesting 'criticality matrix' where the 'supply risks' and importance to clean energy are mapped for both a short term assessment (0–5 years) and a long term projection (5–15 years). The rare earths, europium, yttrium, neodymium, dysprosium and terbium appear to have the highest supply risk and also the greatest importance to clean energy technologies. While the DOE report presents an American perspective, large countries like India which lack a substantial base of natural mineral resource may need to constantly assess strategies to deal with impending scarcities.

Japan, a technologically advanced but resource strapped country, has evolved long term strategies. In a commentary entitled 'Managing the scarcity of chemical elements', E. Nakamura and K. Sato discuss Japan's 'Element Strategy' (*Nature Materials*, 2011, **10**, 158). They characterize the period from the 1960s to 1980s as 'an era when pioneers were racing through the unexplored and fertile wilderness of the periodic table'. The many new materials 'found in this "element hunt" have been successfully used in high-tech products that now play an indispensable role in our lives'. These authors highlight the fact that 'element hunters' were not only materials scientists but even included the organic chemists. They provide the 2010 Nobel prize in chemistry awarded to Heck, Negishi and Suzuki as an example; a case where 'the properties of palladium, zinc and boron were used to the full'. Although much of the world focus is on rare earths and the elements listed in the DOE report, Nakamura and Sato point out that even phosphorus is 'considered to be in possible danger of depletion'. A scenario may unfold over the next fifty years where 'a shortage of phosphorus fertilizer would cause significant damage to agriculture'. Uneven distribution of scarce but critical elements will raise complex issues of politics and trade. The authors note that 'the production of the platinum group metals, important as catalysts, is concentrated in South Africa and Russia; niobium which is used as a steel additive, is mostly produced in Brazil and Canada. The tantalum used in capacitors is almost exclusively produced in Australia and Brazil'. The fact, that India with its enormous population is almost completely absent from the list of countries with some strategic resources, must be a matter of concern in the long run.

Are there solutions in sight to address the problems of looming element scarcity? Nakamura and Sato strike an optimistic note reminding us of the Haber–Bosch ammonia synthesis which used 'nitrogen in the air' and led to the fertilizer revolution in agriculture. Scientific advance may be anticipated, catalysed by a crisis. The Japanese 'Element Strategy Initiative' is based on the premise 'that where resources are scarce but high technology is abundant, necessity is the mother of invention'. The strategy rests on 'four pillars: substitution, regulation, reduction and recycling'. The drive to find substitutes will undoubtedly provide a motivating factor for future chemical research. Organic semiconductors which have made a beginning will have to be evaluated not only in terms of 'energy policy but also in terms of the element strategy'. Nakamura and Sato list many novel examples, including the organic ferroelectric, croconic acid ($C_5O_5H_2$), which contains none of the scarce and exotic elements. While regulation and reduction of scarce element usage will undoubtedly form an important part of any 'element policy', recovery and recycling must also be central to a long term strategy. The authors quote a study by the National Institute for Materials Science in Japan which estimates element content in 'urban mines' which contain 'discarded high tech devices'. The numbers are staggering: '6800 tons of gold (16% of the world reserves), 60,000 tons of silver (22%) and 1700 tons of indium (15.5%)'. Recycling by microbial leaching may assume importance in future.

The elements form the core of chemistry. The rarest amongst them are sometimes central to the modern world. Addressing the problems posed by the looming scarcity of the most critical elements of modern technology will be anything but elementary.

P. Balaram

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EDITORIAL

Day Science, Night Science

Science is often described as basic or applied, good or bad, useful or useless (esoteric, may be less dismissive). I was puzzled by the terms 'night science' and 'day science' which appeared in a recent essay (Jacob, F., *Science*, 2011, **332**, 767). The author, Francois Jacob, a recipient of the 1965 Nobel Prize in Physiology or Medicine, was reflecting on the 50th anniversary of the birth of the 'operon' in molecular biology. By the late 1950s, the gene as a DNA molecule was an established fact and the chemistry of heredity seemed to emerge seamlessly from the complementarity of the double helix. The genetic code was just over the horizon. Genes led to proteins, which in turn controlled the myriad pathways of biochemistry. Control in turning genes on and off seemed necessary to explain the amazing coherence of life's chemistry. The seminal paper that launched the 'operon' and opened the field of gene regulation was written almost exactly half a century ago, by Francois Jacob and Jacques Monod. Entitled, 'Genetic regulatory mechanisms in the synthesis of proteins' (*J. Mol. Biol*., 1961, **3**, 318), the article introduced the idea of a switch, controlling the 'expression of a cluster of structural genes' that directed the synthesis of several proteins, responsible for connected biochemical tasks. Switches could be turned on and off by specific interactions between molecules, DNA and proteins amongst them. The ideas of operators, repressors and inducers emerged from this study; terms commonplace in molecular biology today. The 1961 paper was pathbreaking, laying bare in Jacob's words: '… a mechanism fundamental to all living beings from their very beginnings and that would persist as long as they exist.… More than ever research seemed to be identified with human nature.… It was by far the best means found by man to face the chaos of the universe.' Jacob adds, undoubtedly aided by the wisdom of hindsight: 'Our breakthrough was the result of "night science"; a stumbling, wandering exploration of the natural world that relies on intuition as much as it does on the cold, orderly logic of "day science".' Puzzled, as I was, by an unfamiliar description of different styles of science, there seemed no alternative but to turn to the reference provided by the author: *The Statue Within: An Autobiography* (Jacob, F., Unwin Hyman, London, 1988).

Jacob's book is unusual for an account of a life in science. A very large part of the narrative describes his early years; a youth rudely interrupted by the war years. Curiously, the book begins by considering suicide and death; Cleopatra 'holding out an arm to the asp rising, hissing from the figs' and 'Socrates in prison, meditating and holding in his hand the goblet of hemlock' are the images he evokes. Jacob writes, at times hauntingly, about aging and death: '… a life that shrivels up, slowly rots, goes soft as a pulp. This worry about decline grabs me by the throat as I awake.' He justifies writing about his life: 'I am neither to wallow in the mire of self satisfaction nor to settle old scores, but rather to set myself a new purpose, and thus a new existence. It is to take my past and produce the future.' Jacob's urge 'to do' has its corollaries: 'This endless race with time, this preference for desire over enjoyment is not without its drawbacks. Too often, it prevents us from understanding, and nurtures the illusion of life rather than life itself.' Jacob is reflective and must strike a chord in readers who think about science: 'It took me a long time to realize that this drive towards tomorrow has an advantage in at least one domain – in research. Late, very late, I discovered the true nature of science, of how it proceeds, of the men who do it. I came to understand that contrary to what I had believed, the march of science does not consist in a series of inevitable conquests, or advance along the royal road of human reason, or result necessarily and inevitably from conclusive observations dictated by experiment and argumentation.… To my surprise, those who achieved the unexpected and invented the possible were not simply men of learning and method. More than anything else, they possessed extraordinary minds, enjoyed the difficult and often were creatures of amazing vision. Those in the front ranks displayed exotic blends of passion and indifference, of rigor and whimsy, of naivete and the will to power, in a triumph of individuality.' Jacob worked at the Pasteur Institute in Paris, a centre with a rich tradition of the finest research in biology. Jacob describes the Institute as 'a curious mixture of excellent science and *laissez-faire*, of boldness and routine, of paternalism and incompetence…'.

Pasteur is venerated in France. Jacob describes an annual event at the end of September where the institution commemorates 'the death of its founder', and a solemn procession enters a 'mausoleum out of Byzantium'. Jacob recalls a childhood devotion to Napoleon and a

grandfather's admonition: 'Admīre, yes. Idolīze, no. Neither gods nor men. Not gods, for they do not exist. Not men, for they are not gods.' The scene at the tomb evokes wonder: 'On the walls to either side of the tomb were marble panels on which were carved like Napoleon's victories in the Invalides, Pasteur's victories. Instead of the battles of Austerlitz, Jena and Friedland, one read of molecular dissymmetry, fermentation; so-called spontaneous generation; studies on wine; silkworm diseases; studies on beer; virulent diseases; vaccines; prophylaxis against rabies. … how could one not marvel at this unbroken series of triumphs, at this sure ability to deduce, from a theory, its applications or, on the contrary, to extract from the most theoretical problem, the most concrete aspects? How could one not admire this scientific odyssey, this vaulting from one domain to another, of going from chemistry and crystallography to the study of living things, from the diseases of beer to those of man?… Like Napoleon, Pasteur had fought many battles.… Like Napoleon's, Pasteur's art consisted in always joining the battle at the moment of his own choosing, at the place of his own choosing, on his own ground. And his ground was the laboratory; his weapons were experiments, protocols, the culture flasks. Whatever new domain he entered … Pasteur sought each time to transform the problem, to translate it into other terms, to open it to experiment.… It is Pasteur and this strategy that began modern medicine and what is now called "public health". Without a doubt, Pasteur's saga was as stirring as Napoleon's!' The Pasteur Institute provided Jacob a unique ambience, working between two men who would years later share a Nobel prize with him, Andre Lwoff and Jacques Monod. In linking the apparently distinct lines of research in the laboratories of his collaborators, Jacob chanced upon his breakthrough, a consequence of what he calls 'night science'.

Jacob argues that 'once admitted, once taught, science is cold. As cold as the techniques that derive from it. As cold as the texts explaining its content or the books reporting its history. Science in the works has two aspects: what could be called day science and night science. Day science employs reasoning that meshes like gears and achieves results with the force of certainty. One admires its majestic arrangement as that of a da Vinci painting or a Bach fugue.… Conscious of its progress, proud of its past, sure of its future, day science advances in light and glory. Night science, on the other hand, wanders blindly. It hesitates, stumbles, falls back, sweats, wakes with a start. Doubting everything, it feels its way, questions itself, constantly pulls itself together. It is a sort of workshop of the possible, where are elaborated what will become the building materials of science. Where hypotheses take the form of vague presentiments, of hazy sensations.… It is impossible to predict whether night science will ever pass to the day condition.… When that happens, it happens fortuitously, like a freak.… What guides the mind then, is not logic. It is instinct, intuition.' These reflections precede Jacob's account of his famous work with Monod, who from all accounts could be described as a practitioner of 'day science'.

Reading about Monod and his style, I turned, inevitably, to a compelling account of biology, written by him forty years ago. *Chance and Necessity* (Monod, J., William Collins Sons, Glasgow, 1972) begins with a quote from Democritus: 'Everything existing in the Universe is the fruit of chance and of necessity.' Monod argues with the sureness of a 'day scientist' that biology and life as we know it must be a one off occurrence and that life has originated by chance, unlikely and improbable in the Universe. Biology, in his assessment, 'occupies a position among the sciences both marginal and central. Marginal because, the living world constituting only a tiny and very "special" part of the universe, it does not seem likely that the study of living beings will ever uncover general laws applicable outside the biosphere. But if the ultimate aim of the whole of science is indeed … to clarify man's relationship to the universe, then biology must be accorded a central position, since of all the disciplines it is the one that endeavours to go most directly to the heart of the problems that must be resolved.' Monod's book provides a deeply insightful analysis of many problems that continue to attract attention today. In an essay entitled 'Maxwell's demons' he provides a view of enzymes and catalysis, linking to modern views of 'the equivalence between information and negative entropy'. In a chapter titled 'Microscopic cybernetics' he outlines the logic of regulation, that underlies the growing discipline of systems biology. Monod introduced the idea of 'allostery' in 1965 to explain how isolated oxygen binding sites on hemoglobin could 'sense' one another; another example of 'on–off' regulation. This was an example of a conjecture developing into a theory. Max Perutz independently established allostery experimentally, by X-ray diffraction. Years later he wrote: 'There are two ways out of an impasse in science: to experiment or to think. By temperament, perhaps, I experimented, whereas Jacques Monod thought. In the end our paths converged' (*I Wish I'd Made You Angry Earlier: Essays on Science and Scientists*, Perutz, M., Oxford University Press, 1998).

In thinking about experiment and theory, night science and day science I must return to Jacob's recent editorial essay: 'In today's vastly expanded scientific enterprise, obsessed with impact factors and competition, we will need much more night science to unveil the many mysteries that remain about the workings of organisms.' There is something appealing about Jacob's 'night science', stumbling along in the dark, hoping that chance will illuminate the transformation into 'day science'.

P. Balaram

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EDITORIAL

The Mirage of World Class Institutions

The Indian Institutes of Technology (IITs) are, in many ways, the flagship of the higher education system in India. In the fifty years or so since their inception, they have established a worldwide reputation for the quality of their students. While the focus has always been on a high standard of undergraduate education in engineering, post-graduate teaching and research in science and engineering have also contributed, in some measure, to the image of these institutions. Over the years, the pressure to enter the IITs has made the Joint Entrance Examination (JEE), a benchmark for procedures that select students, in limited numbers, from a very large pool of aspirants. The lustre of the JEE has dimmed in recent years as 'coaching' has become a lucrative profession. Many successful and highly ranked entrants to the IITs perform indifferently in their courses of study. Murmurs of students who are already burnt out by years of preparation are also heard, at times, in the corridors of these institutions. The public image of the IITs, built over the last few decades, has rested greatly on the performance of its alumni, many of whom have had stellar careers in diverse professions across the world. Many among the generations of students, who graduated from the IITs since the 1960s will look back fondly (and undoubtedly, with gratitude) on their IIT days and remember teachers, who enthused and inspired them. In recent years, the IITs have grown in number and their 'brand equity', a favourite phrase of some of IITs most successful alumni, is in danger of being diluted. Nevertheless, the now mature and middle aged, 'original IITs' remain amongst the most sought after destinations for students in India. As an alumnus of the IITs, albeit in science and not engineering, a long time observer of the Indian academic scene and as one who has spent (or possibly misspent) an entire career in one of India's oldest institutions, a forerunner of the IITs, I read with some surprise the intemperate remarks attributed to the Minister for Environment and Forests. Speaking on the sidelines of an event, where he announced government's intention to partner with one of India's largest corporate conglomerates, Reliance, to set up a research centre focusing on marine diversity, Jairam Ramesh pronounced judgement on the IITs. He dismissed the faculty of the IITs as being mediocre in their research and argued that the reputations of the institutions rested solely on the quality of students, whom he declared as 'world class'. The immediate provocation, for delivering this dismissive assessment of our flagship institutions, appears to have been a question posed by an intrusive reporter, who asked why the ministry had not sought a public institution as a partner for the proposed research initiative. Dismissing all research in public institutions as ineffective and irrelevant may have been intended to justify government's new found love for public–private partnerships, in the arena of research. What went unnoticed in the media storm that followed the Minister's public criticism of the IIT faculty, is the absence of any assessment of the commitment to research by private industry, even after a decade in which they have profited hugely by the many concessions extended by government.

The Minister's, characteristically provocative, remarks appeared just as I prepared to participate in the annual convocation of IIT Kanpur. As an alumnus, I was especially proud to be going back, memories rekindled after over four decades. I had entered the IIT, largely unfocused, vaguely contemplating careers in journalism or the civil services. Two years later I left, seduced by science. How do students transform, mature and grow in academic institutions? They do so, undoubtedly, influenced by their peers and their teachers. Much has changed since then. But, has it really changed so much? Are today's faculty less accomplished, less committed and less capable than those who served the IITs in their youthful years? Or is it that the ambience has changed; the aspirations of both students and faculty have transformed over the years and the expectations of the outside world have dramatically increased? It is these questions that bothered me on the long journey to Kanpur. But, more was to come. A few days later on the sidelines of another function to promote science in Bangalore, C. N. R. Rao (see *Curr. Sci*., 2011, **100**, 1466), an enormously accomplished scientist and a former member of the faculty of the IITs dismissed the IITs and, for good measure, the Indian Institute of Science (IISc) as mediocre research institutions, far from being 'world class'. These independent and magisterial pronouncements by widely disparate commentators does raise serious questions. Are our leading research and educational centres in science and engineering as poor as they are made out to be, by Ramesh and Rao? Is the yardstick of 'world class' well understood (or possibly,

mis-understood) by politicians, policy makers and sometimes, scientists themselves? Have our best institutions deteriorated from a once-held position of strength? Are increased resources and a dramatic improvement in 'governance' the key to the transformation of institutions into 'world class' academic centres?

Coincidentally, even as the debate on our institutions was sparked in the public domain, the report of the Kakodkar Committee, set up by the Ministry of Human Resource Development (MHRD) to 'recommend autonomy measures to facilitate IITs scaling greater heights' was released. The report entitled 'Taking IITs to excellence and greater relevance' is a large document of over 250 pages. Chapter 5 of the Kakodkar report begins with the terse statement: 'World-class institutions require world-class faculty members.' Clearly, the transformation, from institutions with the primary mandate of imparting high quality undergraduate engineering education into 'research universities', places significantly higher demands on faculty research output. The debate on the quality and quantity of research in the best of our institutions has been fuelled in recent years by world rankings of universities, in which Indian institutes do not appear in the top 100. While a few make an entry in the top 500, there is a considerable desire to move up the scale. While the term 'world-class' has not been defined in quantitative terms, by many commentators, I suspect the magical figure of 100 is probably uppermost in the minds of many. Curiously, MHRD uses the term in somewhat cavalier fashion, probably borrowing it from the many reports and documents which project visions for the future. Pronouncements are frequently made on the intention of establishing dozens of 'world-class' universities. Another widely misused word, 'innovation', is also a favourite. National 'innovation universities' are on the anvil, even as our existing institutions (or at least those who govern them) wilt under the weight of criticism from politicians and policy makers. Unfortunately, many discussions centering around the problem of raising our institutions to 'world-class' status are carried out with little or no understanding of the procedures used for generating the world rankings. In attempting to chart a course to take the 'IITs to excellence and greater relevance' the Kakodkar committee made a brief visit to China. Their observations, at least in part, merit reproduction: 'Universities enjoy institutional autonomy under Central government guidance. There is a system of comprehensive performance assessment of the faculty, which is done annually. Faculty compensation may vary by as much as a factor of 10 depending on the performance measured on the basis of teaching, research output, technology, resources brought in, IPR generated, linkage with industry/local government, etc.' As I read this, I could not but help succumb to a feeling of despair, that even the most well intentioned efforts at review and reform can be derailed by the affliction of the China syndrome. Kakodkar and his colleagues must undoubtedly be aware of the difference between India and China in their systems of governance and the fact that the words 'autonomy' and 'guidance' may have distinctly different meanings in the two countries. The tendency of committees to embark on 'fact finding missions' overseas is certainly one that leaves me distinctly uneasy (although, I must confess that my view may be prejudiced). The most recent IIT review adds little to the 2004 Rama Rao report and its projections for expansion seem to have little relation to the task of raising the IITs (or indeed any of our institutions) to 'world-class' status.

In a recent column, P. V. Indiresan (a former Director of an IIT) agrees in part with Ramesh: '… the IITs are not world-class research institutions. However, he is not correct in asserting that the students are world-class. The JEE has become so trainable that it puts out more mugpots than brilliant minds.' Indiresan notes that the Minister's statement is a 'half-truth'. He goes on to quote Mahatma Gandhi: 'a half-truth is worse than a lie; therefore, I call it a lie and a half' (*Business Line*, 4 June 2011). Jairam Ramesh, C. N. R. Rao and P. V. Indiresan, three commentators as disparate as can be, appear to have reached a conclusion that reflects an uncomfortable reality. Our best institutions must indeed improve their academic performance dramatically, to become internationally competitive in research. It must also be realized that our institutions are embedded in an environment that can be severely constraining to any reform process. An understanding of institutional histories and the process of evolution may prove necessary in any informed debate. It would be foolish to imagine that Cambridge, with a history of 800 years, or Harvard with 375 years of experience can be matched by mere public pronouncements of intent. An understanding of historical legacies and shackling constraints must precede any discussion of a sweeping reform process. Institutions also need to introspect, conduct internal reviews and define both short-term and medium-term goals. Visions of a distant future are best left to government appointed committees. Learning from others can also be done by reading and reflection. In a recent editorial, Bruce Alberts writing about 'The New Egypt' notes: 'There are lessons to be learned from the mistakes made by other nations. For example, years ago I was shocked by the remarks made by a scientific leader in India who, when asked about the major problem in his large research institute told me that it was "getting people to work". …I discovered that in India's government institutions, life tenure in one's position is normally granted after 1 year of work' (*Science*, 2011, **332**, 513). While 'accountability' is a popular word it is rarely used in an academic context. Institutional reform must begin within organizations, with the bar on academic performance being raised considerably; otherwise 'world-class' institutions will be a mirage, which will continually beckon us.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Cancer: Human Mortality and Cellular Immortality

Cancer is a word used as a metaphor for unchecked malignancies in human societies. Corruption, indiscipline, lawlessness and intolerance spread uncontrollably, like cancers, if left unchecked. But, their advance can be halted by purposeful action, supported by public consensus. Is the disease itself, stoppable? Is the war on cancer, declared almost exactly 40 years ago, going well? In marking the anniversary of the National Cancer Act, signed into US law in 1971, a recent retrospective notes that annual spending by the National Cancer Institute (NCI) has grown from $233 million to over $5 billion, in four decades. Thus far, $90 billion is the estimated amount spent on battling cancer. The 'war', a costly metaphor, has not gone the way researchers and policymakers anticipated in the 1970s. A 1986 critique, appearing 15 years after the battle had begun, noted that 'cancer incidence and mortality had not changed much' (Marshall, E., *Science*, 2011, **331**, 1540). The 25 March issue of *Science* presents a retrospective on the experiences of the 'cancer crusade'. The major forms of the disease, which attack the lung, colon, rectum, breast, prostate, blood (leukemia), pancreas and liver, are still formidable killers, despite the range of weapons thrown at them. Oral cancer, a major public health problem in India as a result of unrestrained tobacco usage, appears less of a concern in the United States. In 2003 the then NCI director optimistically pronounced that victory over cancer was imminent and that the war would be over by 2015. The end does not really seem to be in sight. Churchill's famous exhortation, after one of the early successes of the Allies at El Alamein in the Second World War, may be apt: 'This is not the end. It is not even the beginning of the end. But it is, perhaps, the end of the beginning.' Churchillian rhetoric may in fact be needed to garner public support for a campaign that is likely to become ever more costly.

Cancer is truly 'the emperor of all maladies', a phrase borrowed from a nineteenth century description by Siddhartha Mukjerjee for the title of his compelling, Pulitzer Prize winning 'biography' of the disease. Mukherjee, an oncologist and a writer of rare talent, traces the long history of the disease, presumably as old as human history; a disease that has resisted and, at times, repulsed the enormously powerful attack of twentieth century science. In Mukherjee's words, his extraordinary account is really 'a military history – one in which the adversary is formless, timeless and pervasive. Here, too, there are victories and losses, campaigns upon campaigns, heroes and hubris, survival and resilience – and inevitably, the wounded, the condemned, the forgotten, the dead'. In fashioning a biography of a subject whose origins are shrouded in a distant past and whose end is not even visible on a distant horizon, Mukherjee weaves a story that must be read by every scientist who believes that modern biomedical research, so firmly founded on advances in molecular biology and genetics, will soon conquer cancer. It must also be read by those who believe technology will trump nature. It must be read by all those who are captivated by good writing, for Mukherjee displays an eloquence in writing about science that is elevating. The broad and scholarly historical approach to cancer allows Mukherjee to emphasise that our understanding of the disease has continually transformed over the centuries and even 'morphed, often radically, from decade to decade' in the turbulent advance of twentieth century science. Cancer is a term used to describe a heterogeneous group of conditions 'caused by the uncontrolled growth of a single cell'. This growth, in lung or liver, in blood or brain, or indeed in any organ of the human body 'is unleashed by mutations – changes in DNA that specifically affect genes that incite unlimited cell growth. In a normal cell, powerful genetic circuits regulate cell division and cell death. In a cancer cell, these circuits have been broken, unleashing a cell that cannot stop growing' (*The Emperor of All Maladies*, Siddhartha Mukherjee, Harper-Collins, London, 2011). What is it that frustrates most efforts to retard this uncontrolled growth? Mukherjee answers: 'Cell division allows us as organisms to grow, to adapt, to recover, to repair – to live. And distorted and unleashed, it allows cancer cells to grow, to flourish, to adapt, to recover, and to repair – to live at the cost of our living. Cancer cells grow faster, adapt better. They are more perfect versions of ourselves.' The difficulty in distinguishing between normal and malignant cells has limited the efficacy of every therapy advanced for cancer. The 'adaptability' of cancer cells lies at the heart of the problem of drug resistance. Mukherjee is eloquent when he views his subject: 'Cancer is built into our

genomes: the genes that unmoor normal cell division are not foreign to our bodies, but rather mutated, distorted versions of the very genes that perform vital cellular functions. And cancer is imprinted in our society: as we extend our life span as a species, we inevitably unleash malignant cell growth … If we seek immortality, then so, too, in a rather perverse sense, does the cancer cell.' Will science enable a future generation to win the war on cancer? Mukherjee recalls a memorable quote from J. B. S. Haldane: 'The universe is not only queerer than we suppose, but queerer than we can suppose' and adds, 'so is the trajectory of science'.

The origins of surgical excision of tumors go back across the millennia. Mukherjee narrates the tale of Atossa, the queen of Persia in about 500 BC, detecting 'a bleeding lump in her breast'. Her tumor was excised by a Greek slave, providing a reprieve, leading her to persuade the king, Darius, to invade Greece so that her benefactor could return home. Mukherjee wryly notes: 'It was Atossa's tumor, then, that quietly launched a thousand ships.' Surgical intervention made facile by technical advances in surgery is often used to remove accessible tumors. Surgery fails when cancer cells migrate, metastasize, and lodge themselves in other locations in the body, to grow again unchecked. Radiation and chemotherapy are the only other weapons available in the war on cancer. While early detection heightens the chance of limiting the spread of the disease, cancer often strikes silently, announcing its presence only when it is too late. Mukherjee traces the origins of chemotherapy for childhood leukemia to Sidney Farber's transition from being a pediatric pathologist, 'more preoccupied with the dead than the living', to a clinical researcher searching for a new therapeutic strategy. When he began, acute leukemia cases were 'discussed on medical rounds with professional grandiosity and then … diagnosed, transfused – and sent home to die'. Farber chose his enemy well. Leukemia, characterized by the wild proliferation of white blood cells could be quantitatively measured, counted. The effects of any drug could then be readily assessed. The road to experiment was open. It is at this point that Mukherjee draws attention to an Indian connection. Millworkers in Bombay, struggling in poverty, malnourished and over-worked, suffered from an anemia that could not be reversed by vitamin B-12. Mukherjee describes the discovery of Wills factor, folic acid or folate. Farber turned to folate, a dangerous act of empiricism, in treating leukemia. The disease accelerated in patients administered folic acid – a clinical trial gone awry in the days before regulation. But a new question arose – would antifolates halt the growth of white blood cells? The folate 'antagonists' were indeed available with Yellapragada Subbarao working at the Lederle Labs. The first trial with pteroylaspartic acid began in September 1947, when Farber treated a two-year-old patient, whose immature white blood cells 'were dividing in a frenzy'. The drug had little effect. A few weeks later a new sample, aminopterin, arrived from Subbarao. The patient, by now, was near death. The response was dramatic. Chemotherapy for leukemia had been launched at a time when no ethical guidelines were in effect for controlling clinical trials. Coincidentally, 'the Nuremberg code for human experimentation, requiring explicit voluntary consent from patients was drafted on 9 August 1947'.

Mukherjee's description of a cancer cell as a 'cell seeking immortality' reminded me of HeLa cells, derived from the biopsy tissue of a patient with a particularly devastating form of cervical cancer. HeLa cells are central to a great deal of biomedical research; a human 'cell line' that can be maintained and multiplied in laboratory culture across the world. The biopsy tissue was obtained from a woman, Henrietta Lacks, almost exactly half a century ago, in early 1951. They first grew in culture in a Johns Hopkins laboratory. Later, they grew and grew, immortal in laboratories across continents. In *The Immortal Life of Henrietta Lacks*, Rebecca Skloot (Crown Publishers, New York, 2010) chronicles the story of a black woman, her family and her cells which have remained alive, dividing and well, in research laboratories, contributing to the advance of modern medical science. Skloot, is a writer immersed in her subject, who weaves a tale of haunting sadness, where a family struggles to comprehend the meaning of cellular immortality, even as they accept, stoically, the stark reality of human mortality. Skloot's portrayal of Henrietta Lacks' daughter, Deborah, is warm and moving, describing her determination, in the midst of great adversity, to understand the biological mystery of her mother's cells. Throughout the book runs the theme of medical ethics, not a critical public issue in the early 1950s, when Henrietta Lacks tumor was excised. While millions upon millions of dollars have been made using research based on HeLa cells, her family, in the 21st century, remains unable to afford health insurance. Do people have ownership over their bodies? Do scraps of tissue which turn into a gold mine, as in the case of Henrietta Lacks, place moral obligations on a society that benefits from research? These and many other troubling questions arise even as a reader moves smoothly through Skloot's captivating narrative.

In reflecting on the cancer wars, Mukherjee notes: '…while the content of medicine is constantly changing, its *form* … remains astonishingly the same. History repeats, but science reverberates. The tools that we will use to battle cancer in the future will doubtless alter…. But much about this battle will remain the same: the relentlessness, the inventiveness, the resilience, the queasy pivoting between defeatism and hope, the hypnotic drive for universal solutions, the disappointment of defeat, the arrogance and the hubris.' Cancer presents a strange paradox; reminding us starkly of human mortality, even as our cells seek immortality.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Defaming the Dead: History and Statistics

Deconstructing the work of famous scientists, many years after their passing, is a task that has been undertaken by many historians of science and writers of great distinction. A recent editorial in *Nature* (2011, **474**, 419) notes that 'it is impossible to libel the dead, but equally impossible for them to defend themselves'. There is a certain excitement in following the trail of research published decades or even a century ago and discussing evidence for misinterpretation. The thrill heightens when the conclusions drawn from experiments and observations appear to be the result of conscious bias or deliberate tuning of data, to fit a compelling hypothesis. From Newton to Mendel, science's icons have been dissected long after their deaths, by modern historians of science, aided by the benefits of hindsight. In an article that appeared almost forty years ago, entitled 'Newton and the Fudge Factor', Richard Westfall passed judgement: 'Not the least part of the *Principia*'s persuasiveness was its deliberate pretense to a degree of precision quite beyond its legitimate claim. If the *Principia* established the quantitative pattern of modern science, it equally suggested a less sublime truth – that no one can manipulate the fudge factor quite so effectively as the master mathematician himself' (*Science*, 1973, **179**, 751). Fudge factors have been commonplace in the centuries since Newton, as theorists strive to reconcile their results with experimental outcomes. Adjustable parameters are the staple of empirical theoretical approaches. Newton could 'mend the numbers', his physical insights reaffirmed by calculations whose precision, in Westfall's analysis, were often 'brazenly manipulated'.

In 1936, seventy years after Gregor Mendel published his immortal paper which launched the field of genetics, R. A. Fisher, a celebrated statistician, muddied the waters. Mendel's paper, unlike much of the modern literature, provided enough raw data to permit independent analysis. In devoting a great deal of effort to re-analysing Mendel's data, Fisher came to a startling conclusion: '… the data of most, if not all, of the experiments have been falsified so as to agree with Mendel's expectation' (Fisher, R. A., *Ann. Sci*., 1936, **1**, 115; http://digital.library.adelaide.edu/). Fisher's analysis, in turn, was dissected seventy years later leading to the hope that 'against all experience that Fisher's allegation of deliberate falsification can finally be put to rest, because on closer analysis it has proved to be unsupported by convincing evidence' (Hartl, D. L. and Fairbanks, D. J., *Genetics*, 2007, **175**, 975). Scientists are often accused of 'selecting data' to fit a pet hypothesis; errant data points that deviate wildly from expectation are discarded as erroneous. This is a practice that is not uncommon; one that statisticians seem to frown upon. Fisher, however, did not appear to be critical of Mendel's 'data trimming'; rather he seemed to be raising an alarm that some of Mendel's data seemed to fit an expectation much too well. A common saying in experimental laboratories is that sometimes 'data appears to be too good to be true'. Happy accidents do happen, although statisticians may add that the probabilities are low. C. R. Rao, the eminent statistician revisited the Fisher analysis noting that the overall probability of agreement between Mendel's data and his expectation 'is 7/100000, which is very small'. He goes on to quote Fisher: 'Although no explanation can be expected to be satisfactory it remains the possibility among others that Mendel was deceived by some assistant who knew too well what was expected' (Rao, C. R., *Statistics and Truth*, World Scientific, Singapore, 1997, pp. 78–79). Hartl and Fairbanks suggest that Fisher may have been misled by his reading of the traits that Mendel scored in his experiments. There are other explanations too. While Mendel's work remains one of the conceptual pillars on which modern biology rests, allegations once made tend to resurface. In their title, Hartl and Fairbanks emphasize the fact that 'mud sticks'. They pay the highest tributes to the manner in which Mendel presented his raw data, noting that his 'celebrated paper … is an inexhaustible source of inspiration and controversy for each succeeding generation of geneticists and historians of genetics'. In their assessment, 'Mendel's paper appears to reflect the author's simplicity, modesty, and guilelessness'. C. R. Rao credits Mendel with 'introducing the *indeterministic paradigm* in the history of science … by observing data subject to chance fluctuations'; a precursor to modern evolutionary theory which has been described as 'a mixture of chance and necessity – chance at the level of variation and necessity in the working of selection' (pp. 35–36). Newton and Mendel are iconic examples of creativity; scientists whose instincts and insights seem to enable them to slice away irrelevant and confusing data. To the purist, steeped in the modern methods of data analysis, there is a disturbing hint of picking and choosing data; fudging a calculation by mathematical sleight of

hand. Reading about the attempts to deconstruct Newton and Mendel, I was reminded of how chance often showers its blessings on those who are singularly talented. Football fans will recall Diego Maradona's famous goal for Argentina in a World Cup quarter final against England in 1986, when the 'hand of God' propelled the Argentinians to a famous victory, in a clash that took place not long after the Falklands War between the two countries.

Few writers have matched Stephen Jay Gould in his abilities to revisit famous successes, failures and controversies in science. In 1987, Gould resurrected an old controversy in paleontology, a field replete with many famous disputes. In turning to the fossil of the Archaeopteryx, 'an evolutionary intermediate, a transitional form between reptiles and birds', Gould focused on one of Victorian England's famous fueds; the running battle between Richard Owen and Thomas Huxley (*New Scientist*, 12 March 1987, pp. 32–36). The fossil was a prized acquisition for the British Museum, then headed by Richard Owen. Darwin's theories had just been published and here was a fossil likely to link two major biological lineages. Unfortunately, Owen interpreted the orientation of the fossil on the slab incorrectly, an error that left him open to Huxley's merciless criticism. Huxley had scored another victory over his foe. But Gould points out that 'the fullness of history often drowns an immediate triumph in irony. Although he muffed his description, we now realise that Owen was right and Huxley wrong, in the central aspect of their general interpretations'. Gould's 1987 essay entitled 'The Fossil Fraud that Never Was', was sparked by a claim by Fred Hoyle that the Archaeopteryx fossil is a forgery, 'manufactured from modern feathers pressed into a layer of artificial cement painted atop the genuine skeleton of a small dinosaur'. Gould describes Hoyle as an 'eminent cosmologist of years past and more recent supporter of uncommon causes' and proceeds to demolish Hoyle's attempt to paint Owen as the perpetrator of a major fraud. Gould has advice for historians who revel in deconstruction: 'I do not advocate an unthinking ancestor worship, but scholars do make a tacit pledge when they enter this exacting profession – to honour the struggles of those who have gone before and to treat arguments with respect and integrity'. The Hoyle–Wickramasinghe attack on Richard Owen would have been considered libellous if it had not been made nearly a century after Owen's death. A review of the book *Archaeopteryx, The Primordial Bird* by Fred Hoyle and Chandra Wickramasinghe (Christopher Davis, 1987) describes it as containing 'demonstrable falsehoods, as well as hardly imaginable calumnies of persons unable to defend themselves' (Halstead, B., *New Scientist*, 10 September 1987, p. 70). Indeed many attempts at deconstructing past scientific discoveries often degenerate into an obsessive attempt to defame long dead scientists.

The most recent example is an analysis which suggests that Stephen Gould's 1978 dissection of Samuel Morton's work on cranial capacity was flawed (Lewis, J. E. *et al.*, *PLoS Biology*, 2011, **9**, e1001071). Morton, a physician, published a measurement of cranial capacity, based on a collection of nearly 1000 human skulls in 1839 and 1849. Cranial capacity was thought to be a rough indicator of overall intelligence in Morton's study. Human racial differences appeared to be manifested in his measurements, leading inescapably to a ranking of races. Morton's work passed into obscurity until Gould resurrected it 127 years after Morton's death. In a paper in 1978, intriguingly subtitled 'Unconscious manipulation of data may be a scientific norm', Gould sets out to demonstrate that Morton's biases 'produced the results anticipated in an age when few Caucasians doubted their innate superiority' (*Science*, 1978, **200**, 503). Gould quotes extensively from Morton's writings which have a tone that is unabashedly racist, but must be viewed in the specific cultural and historical context of the first half of the 19th century in America. Curiously, Morton's measurements included as many as '17 Hindu skulls' leading him to note that 'the skulls of these people are probably smaller than those of any other existing nation'. Gould argues that Morton chose his data selectively, made convenient omissions and points to 'slips' and 'miscalculations'. He concludes, when the 'data are properly reinterpreted, all races have approximately equal capacities'. His judgement of Morton is charitable: 'I find no indication of fraud or conscious manipulation'. In reflecting on Morton's work, Gould notes 'that lying with statistics is easier than fudging an experiment'. He adds: 'Unconscious or dimly perceived finagling is probably endemic in science, since scientists are human beings rooted in cultural contexts, not automatons directed toward external truth'. Three years after his analysis in *Science*, Gould used Morton's work to title his book, *The Mismeasure of Man* (W. W. Norton, New York, 1981). Three decades later, Lewis *et al.* 'falsify Gould's hypothesis that Morton manipulated his data to conform with his *a priori* views'. Readers of this paper in *PLoS Biology* may find parallels to the Hartl–Fairbanks reanalysis of the Mendel–Fisher controversy. Ironically, Lewis *et al.* conclude that 'Gould's own analysis of Morton is likely the stronger example of bias influencing results'.

Biases and prejudices are an integral part of human nature. Experiments must support hypotheses, theory must edge towards agreement with experiment; these are the pressures of science. Gould was right when he argued that 'finagling is not a disease'. He was even more right when he added, 'the only palliations … are vigilance and scrutiny'. To buttress my limited knowledge of statistics and statisticians, I had turned to C. R. Rao's *Statistics and Truth*. Here I found a quote that seemed apt as a conclusion:

'*All knowledge is, in final analysis, history*
All sciences are, in the abstract, mathematics
All judgements are, in their rationale, statistics.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Blogs, Google and Cognitive Decline

The physicist John Archibald Wheeler is reported to have remarked that 'the only reason universities have students is to educate the professors'. He counted amongst his students Richard Feynman. As a long time professor, I have had the good fortune of being educated by generations of students; each group reflecting changing times. When research students leave, after years of apprenticeship under a mentor, there are mixed feelings; an undoubted sense of relief on both sides, that a long drawn out battle to produce a Ph D thesis has been won, superimposed on a sense of sadness and apprehension that the bird must, inevitably, leave the nest. Parting gifts given by grateful students are treasured by mentors and I have received my share. Boxes of sweets, carefully wrapped, provide both cheer and nutrition. More recently, as I have aged, I have noticed that students (and colleagues) give me books, undoubtedly suggesting that the mind too needs nutrition. The nature of the books that are gifted sometimes is a clear sign of the times. In quick succession, I have been presented with books that are an emphatic reminder that we live in the age of information, imprisoned by the world wide web and the internet. A student, who still hoped to evoke a latent entrepreneurial spirit in me, gave me a book describing the phenomenal rise of *Google* (*The Google Story*, David A. Wise, Pan Books, London, 2005). Having been captivated by the *Google* logo for the past few years and having read popular accounts of the fairy tale story of Sergey Brin and Larry Page, Google's founders, I did not believe that the book would grip my attention. I set it aside. A few days ago, another departing student gave me a book with an eye catching title, '*blog*!'. The absence of capital letters and the exclamation mark in the title were all signs of a new age. While presenting me with the book my student added, for good measure, that it was time that I learnt to blog. Her message was simple and direct; the student attempting to educate the professor. Anyone with pretensions to writing for a general readership, she felt, would be well advised to blog, in the hope of attracting the increasing number of people who seem to flit from site to site on the internet. Laptops, palmtops and 'smart phones' now allow, those who rush about ceaselessly, to exercise their fingers, while they surf through cyberspace. The book she left me proclaims to describe 'how the newest media revolution is changing politics, business, and culture' (*blog*!, David Kline and Dan Burstein, CDS Books, New York, 2005). I have only skimmed, superficially, through the book but it is clear that the 'on-line' world has transformed dramatically in the half-a-dozen years since its publication. Facebook, Twitter and YouTube are terms I hear frequently. 'Blog', I learnt, topped the US Dictionary Words of the Year in 2004 making me feel quite dated, having begun to visit a few sites only three years ago. In reading the first few pages of *blog*!, I was reminded of Charles Dodgson's (more famous as Lewis Carrol) characterization of man, 'as an animal that writes letters'. Kline and Burstein list a number of qualities that mark us as human and add: 'But there is another distinguishing characteristic of human beings that has been unknown or underestimated till recently: We blog.' Blogs and the new social media are transforming global communication and impacting not only politics and culture, but also science and scientific journals in a manner that must leave senior citizens (and I count myself among them) bemused.

The book on *Google*'s short and spectacular history begins by proclaiming: 'Not since Gutenberg invented the modern printing press more than 500 years ago, making books and scientific tomes affordable and widely available to the masses, has any new invention empowered individuals and transformed access to information as profoundly as *Google*.' Interestingly, the words 'blog' and 'google' both appeared in 1997; both are used today as nouns and verbs. It is hard to imagine searching for information before *Google* entered our lives. The days of hunting through stacks of dusty and disorganized piles of printed journals, scouring indices in large, heavy, bound back volumes are fading rapidly from memory. A new generation of students has arrived, for whom the printed journal is almost as distant as the dinosaur. Few realize that searching the internet may require some skill, a little patience and significant foreknowledge, especially in wading through the literature of science. Typing in a few ill chosen keywords or search terms into *Google* can result in an outpouring of irrelevant 'hits'; their numbers large enough to convince novices that the literature they seek is before them. In the hands of those who are

experienced, *Google* can be a weapon of great power, enabling information to be accessed in comfort. 'Googling' can be an addictive pastime; like all addictions those who have succumbed may have to pay a price.

In 2008, Nicholas Carr wrote a provocative piece in *The Atlantic* (2008, **302**, 56) asking the question: 'Is Google making us stupid?' I found Carr's thesis, on 'what the internet is doing to our brains', strangely disturbing. He begins, recalling a scene from Stanley Kubrick's *2001: A Space Odyssey*, where the supercomputer HAL laments '… my Mind is going… I can feel it, I can feel it,' even as its memory circuits are disconnected. Carr notes that 'over the past few years, I've had an uncomfortable sense that someone or something has been tinkering with my brain, remapping the neural circuitry, reprogramming the memory'. He observes that reading books and lengthy articles has become difficult. In words that must undoubtedly strike a chord, in those for whom reading was a hobby in the pre-internet era, he says: 'Now my concentration often starts to drift after two or three pages. I get fidgety, lose the thread, begin looking for something else to do. I feel as if I'm always dragging my wayward brain back to the text. The deep reading that used to come naturally has become a struggle.' Have Google and the internet already begun to mold our brains? Is the plasticity, that is so often discussed by neuroscientists, making our brains respond to the new and all pervasive technologies in a manner that could hardly have been anticipated? A study just published online in *Science* seems to suggest that the ways we handle information are already beginning to show the imprint of the new age. In a report entitled *Google effects on memory: Cognitive consequences of having information at our fingertips*, Betsy Sparrow, Jenny Liu and Daniel Wegner demonstrate that the knowledge that information can be found is indeed resulting in an impairment of the ability to 'remember' the information (*Sciencexpress*, 14 July 2011). A news analysis notes that the study by Sparrow *et al*. was based on the old concept of 'transactive memory'; a term used to describe the ability of groups of people to 'divide the labour of remembering certain types of shared information' (Bohannon, J., *Science*, 2011, **333**, 277). Sparrow *et al.* use a set of cleverly designed experiments to test the ability of large groups of student volunteers to memorise information, when they can access Google to search instead. Using four sets of experiments they suggest 'that when faced with difficult questions people are primed to think about computers and that when people expect to have future access to information, they have lower rates of recall of the information itself and enhanced recall instead of where to access it'. They conclude that the internet and the search engines have together 'become a primary transactive memory source'. They also observe that 'we are becoming symbiotic with our computer tools, growing into interconnected systems that remember less by knowing information than by knowing where the information can be found'.

If Google is making us more forgetful, are blogs affecting the way science is discussed? The book by Kline and Burstein makes no reference to science. Some years ago science journals had already begun to notice blogs. A survey that trawled 46.7 million blogs and ranked them (in the manner that search engines do) found only five, by scientists writing about science, that made it into the top 3500. The topmost science blog came in at 179, a site maintained by a biologist and named *Pharyngula*, in the style unique to blogs and bloggers. The other four sites trailed far behind, testimony to the limited drawing power of science in cyberspace (Butler, D., *Nature*, 2006, **442**, 9). The intrusion of the brave new world of blogs into the traditional methods of disseminating science has been dramatically illustrated by the episode of the 'arsenic-eating bacteria'. Even as the paper went on-line in *Science*, a NASA press conference had triggered a burst of attention. The first sceptical comments on a blog appeared within a couple of days, catalysing an outpouring of critical analysis. The paper's appearance in print was delayed to accommodate simultaneous publication of as many as eight technical comments, all of which cast doubts on the major premise of the paper, that bacteria may conceivably adapt themselves to using arsenic instead of phosphorus; the latter element being central to the biochemistry of life (*Science*, June 3, 2011). Two blogs on the *Scientific American* site (June 16, 2011) summarize the saga. In a posting titled *From the Shadows to the Spotlight to the Dustbin – the Rise and Fall of GFAJ-1*, Rosie Redfield details the overwhelming case against the original paper. In the post, *Arsenic-Eating Bacteria Have Changed Science Education*, Marie-Claire Shanahan addresses 'the changing nature of scientific critique', which appears to be moving 'from the private world of science to the public world of blogs, twitter and online news'. She adds that 'it will be essential for students as future citizens to be able to navigate these media astutely'. I suspect that it may be necessary also for present-day professors to develop some understanding of the rapidly changing face of science communication.

In reflecting on the profound impact of computers and the internet, I am drawn again to Carr's essay, which conveys a sense of bleakness. He turns to the world of *2001* where 'people have become so machinelike that the most human character turns out to be a machine'. For Carr there is a darkly prophetic message in Kubrick's film: '…as we come to rely on computers to mediate our understanding of the world, it is our own intelligence that flattens into artificial intelligence'. It would be one of the great ironies of the age of technology if the growing power of computers fosters collective cognitive decline.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Speaking of Science: Lecturers and Teachers

Rummaging through stacks of long forgotten papers, consigned to the dark dampness of storerooms, is not a task that is attractive to most people. For the collector, interested in archival material, there is something irresistible in searching and reading documents which may have little to do with the tasks at hand. A sense of history, and the importance of preserving past records have rarely been subjects of concern in our scientific institutions and universities. Few institutions in India maintain a well supported archive which can provide scholars, interested in the evolution and growth of our laboratories and institutes, the material necessary for constructing factually correct histories. A few years ago this journal approached its 75th year of existence, a 'platinum jubilee' which needed to be marked; an organizational milestone that could not pass unnoticed. It was then that I realized that no real documentary records of the struggles to start this journal in 1932 were available. Little seemed to be known about the first editor, C. R. Narayana Rao, and the small band of enthusiasts who promoted the journal in its early years. The hunt for unearthing long forgotten information was undertaken with boundless enthusiasm by a young associate of mine, Riki Krishnan, allowing us to piece together a moderately accurate account of this journal's origins (*Current Science*, 2007, **92**, 129). Shortly thereafter, I realized that my own institution, the Indian Institute of Science, was approaching a centenary; a realization that catalysed another round of searching and collecting by my indefatigable colleagues. Collectors are a strange breed; they are often excited by material that the normally busy professional would find completely irrelevant. There is, however, an infectious enthusiasm that collectors display that is hard to resist. I was, therefore, quite enthused when a colleague burst into my room to display his latest find. He had indeed unearthed some remarkable documents, which detail an unknown chapter in the history of Bangalore's institutions. But, what attracted my attention was a copy of an article in *Science* (1966, **154**, 1613) entitled 'The Art of Talking About Science' by Lawrence Bragg. Curiously, this article was reprinted in this journal over twenty years ago when S. Ramaseshan was trying hard to revive the flagging fortunes of the journal (*Current Science*, 1990, **59**, 1341). The reprint, I now held, was a photostat copy, reproduced under the banner of the 'Information Papers' of the Indian Institute of Science. Bragg's article was the first of a series of 'excerpts of educational interest reproduced from current journals'. Relatively few scientists remain in our midst, who learnt and practised their craft at a time when photocopiers (the ubiquitous 'Xerox machine') did not exist and the personal computer and laser printer were not even specks on a distant horizon. Information was transferred slowly in those days, and more importantly, judiciously and selectively. Choosing articles for laborious dissemination would have been a labour of love. I could not but help marvelling at the choice of the first article for reproduction.

W. L. (Lawrence) Bragg (1890–1971) invented 'X-ray analysis for finding the arrangement of atoms in crystals and his determinations of the atomic structures of the rocks that make up the bulk of the Earth's crust revolutionized the foundations of chemistry, mineralogy and metallurgy'. These are the words used by Max Perutz in an essay reflecting on Bragg's contributions to X-ray crystallography. Bragg succeeded Rutherford as the Cavendish Professor of Experimental Physics and it was under his benign stewardship that the revolution in molecular biology began, with the elucidation of the structures of DNA and proteins. Bragg was an exceptional writer and lecturer. Perutz is unstinting in his praise: 'When reviewing scientific work I sometimes paraphrase people's papers, but when I tried to paraphrase Bragg's I always found that he had said it much better. Bragg's superb powers of combining simplicity with rigour, his enthusiasm, liveliness and charm, and his beautiful demonstrations conspired to make him one of the best lecturers on science that ever lived' (Perutz, M. F., *International Union of Crystallography*, 2 February 1990; *I Wish I'd Made You Angry Earlier*, Oxford University Press, 1998). Speaking and writing are an integral part of the life of an academic scientist. Classes need to be conducted, lectures delivered and research papers written. Listening is also a key activity, since seminars are often the most painless way of acquiring new information. Sadly, many accomplished scientists are poor speakers and indifferent writers making communication of their results an ordeal. Bragg begins his essay on 'Talking about science' by asking questions that must often occur to many of us. 'How should we present our branch of science to fellow scientists who work in quite another field? How can we present science to those who have little or no scientific background, as is often the case with men of high ability who are important in affairs of state? How can we make the nonscientist understand why its study means so much to us, a passion they sometimes find difficult to understand?' Bragg argues that 'the gap between C. P. Snow's

two cultures is not so much due to a lack of understanding as to a lack of a desire to understand'. He adds: 'There are Philistines as regards science as well as regards the arts.'

Lecturers who try to promote interest in science by speaking to diverse audiences often face a daunting task. Bragg, who for years lectured to general audiences at the Royal Institution, notes that the 'primary object is to create a state of mind or point of view, not to convey information'. How often have audiences wilted under the deluge of detail that earnest speakers have thrown at them? Bragg measurers the success of a talk 'by the extent to which an average member of the audience remembers it next day'. In his view, 'the value of a lecture is not to be measured by how much one manages to cram into an hour, how much information has been referred to, or how completely it covers the ground. It is to be measured by how much a listener can tell his wife about it at breakfast next morning, or, if she is not interested, a friend in the morning train'. There are many lecturers who read from prepared texts; a practice more common amongst social scientists. But, in these days of Power Point, there are many who read from slides. If the lecturer stumbles and mumbles the audience suffers silently. Bragg is harsh on the 'readers'. He writes: 'I feel that to collect an audience and then read one's material is like inviting a friend to go for a walk and asking him not to mind if you go alongside him in your car'. Bragg notes that there are exceptions, Eddington among them, who read their lectures to great effect. How do these talented 'readers' succeed? Bragg suggests that these 'are the people who so refine and weigh every word and sentence that their beautiful prose almost become poetry – it is like a poet reading verse'.

Bragg makes a very clear distinction between the written and spoken word. His concern is for situations where speakers confront a general audience. Indeed, most academic seminars are delivered to audiences of scientists many of whom may be unfamiliar with the details of the subject being discussed. The written account has as 'its main function' the need to 'be a storehouse of information'. In Bragg's words: '…one need not cross all the "t's" and dot all the "i's" in a lecture.' In fact, 'the talk would be spoiled by an attempt to do so'. In reading the essay, I was drawn to Bragg's apparent discomfiture when written accounts were requested after a talk had been delivered. This is an uncomfortable situation often faced by those who hazard to speak on many public occasions. He notes that 'to prepare a talk, and to write an account of it, are two separate tasks and the latter may be much the heavier'. Bragg then moves on to a situation that many speakers face: 'The most embarrassing thing is to be told that a tape recording will be made, and asked if one would please correct it. It is embarrassing to see a verbatim report with all the remarks recorded literally and it is generally far less trouble to write it from the beginning than to patch the record.' A point that many organizers would do well to note is the need to be kind to lecturers. Here he writes on behalf of those speakers who spend considerable time, effort and nervous energy in preparing and delivering lectures: 'A lecture is a *tour de force* and a good and conscientious lecturer is both nervous beforehand and prostrate afterwards'. He advocates a practice in vogue at the Royal Institution where the lecturer is 'immured in a small private room termed "The Lecturer's Room" for at least half an hour before the lecture starts'. Bragg narrates a famous anecdote which recounts the origins of this practice. Apparently 'a lecturer (actually Wheatstone of Bridge fame) ran away from nervousness just before the lecture started, and so a guard has been placed over the room ever since'.

In the old papers I received was another gem, reproduced in 1968; an essay on the 'Making of a Scientist', based on an address delivered by Hans Krebs, widely known to biochemistry students as the discoverer of a key metabolic reaction cycle (*Nature*, 1967, **215**, 1441–1445). The Krebs cycle occupies a central position in the charts, depicting intermediary metabolism, which adorn the walls of laboratories in the area of biological sciences. Krebs, schooled in the strict discipline of his Prussian origins and the rigorous system of German biochemistry in the pre-War years, writes more formally than Bragg. He asks a question which must have come to many of us over the years: 'How can distinction or excellence be attained in science?' He emphasizes the importance of outstanding teachers and advances a view: 'Scientists are not so much born as made by those who teach them research'. Krebs traces his own academic roots constructing a genealogy, that parallels the history of chemistry: Otto Warburg, Emil Fischer, Adolf von Baeyer, Kekule, Liebig, Gay-Lussac, Berthollet and Lavoisier. He asks: 'What then, is it in particular that can be learned from teachers of special distinction?' He answers: 'We measure everything, including ourselves by comparisons; and in the absence of someone with outstanding ability, there is a risk that we easily come to believe that we are excellent and much better than the next man. Mediocre people may appear big to themselves (and others) if they are surrounded by small circumstances. By the same token, big people feel dwarfed in the company of giants, and this is a most useful feeling. So what the giants of science teach us is to see ourselves modestly and not overrate ourselves.'

Reading the decades old essays of Bragg and Krebs, reproduced undoubtedly to stimulate the small band of scientists in Bangalore in the 1960s, I could not but help feeling a sense of regret. The passage of years has undoubtedly transformed the way in which science is practised and communicated. Motivations for doing and speaking about science seem to have changed. In reflecting on these essays, I realized that reading old documents can be diversionary and disturbing. 'Speaking of science', a term I have borrowed from a popular column in *The Hindu* by D. Balasubramanian, can be challenging for both lecturers and teachers.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Auditing Academia: Sherpas and Stars

Auditors have always been associated, in common perception, with the responsibility of ensuring that the financial transactions of institutions and organizations are above board, following norms laid down by government and regulatory bodies. Auditors also examine whether public institutions have overspent money or delayed projects, often pointing to managerial inefficiency. In assessing the factors that lead to cost and time overruns, even when the highest standards of financial propriety have been maintained, auditors have the wonderful benefit of hindsight. In recent times, the Comptroller and Auditor General of India (CAG) has acquired a larger than life image as the guardian of integrity and efficiency in public projects and institutions. The present climate of public suspicion and recrimination in India may lead to a situation where doing nothing may be the safest option for those who are charged with the responsibility of overseeing expansion and development. Academic institutions are also subject to audit, but the focus has always been on matters related to finance, accounting, purchases and construction activities. While the scale of spending in our research institutions is increasing, it is still a relatively minor figure in the ever increasing expenditures of government. Institutions often benefit from the sometimes uncomfortable observations of auditors, as corrective measures invariably follow the dreaded 'audit para'; a not uncommon outcome of every annual exercise. Many heads of academic institutions are generally innocent of the byzantine world of finance and accounts (I count myself among them). The external auditors, are therefore, a most welcome addition to internal oversight mechanisms. While financial audits are performed with great efficiency by teams with accounting expertise, 'academic audits' may still be largely uncharted territory. While project reviews and departmental assessment by external 'expert committees' are the norm, they certainly do not seem to be invested with the same stamp of authority that is conveyed by financial audit. Performance audits in the sphere of higher education and research are often subjective and open to debate. How does one assess the performance of an academic institution? The easiest, and undoubtedly escapist, way appears to be to follow the ranking schemes that have become so popular; the Shanghai rankings or those produced by *The Times*. These, however, largely focus on the best in the world; the top 500, leaving thousands more well beneath the radar. Nobel prizes, papers in *Science*, *Nature* and in the top 1% of cited articles may be criteria that permit distinctions between the top research universities; they may hardly be the index by which one can judge whether the vast majority of institutions are doing their jobs well. Institutions are invariably judged by the academic performance of their faculty and students. Strong teaching and mentoring programs create an image of well trained and capable alumni. Productive research programs are the hallmark of a scholarly faculty. When both flourish, institutions scale great heights and bask in the warm glow of public appreciation. When either teaching or research become the exclusive focus, the academic profile of an institution becomes excessively skewed. Small, exclusive research institutions are often well hidden from public view; rarely do they confront the public scrutiny to which large universities are often subject.

Two entirely disparate events conspired to turn my thoughts towards the problem of assessing performance ('auditing') of academics and their institutions. First, of course, was the barrage of discussion in the media on financial audits conducted by the CAG. Can there be other kinds of audit, especially where performance in a mandated task is measured? Secondly, I stumbled entirely by accident on a provocatively written report, commissioned by the Regents of the Universities of Texas. Here, a highly paid outsider was brought in to assess the state of two major universities. The subject of 'academic audit' may indeed be highly relevant in India today, especially as a rapid expansion of the higher education and academic research sector has now been set in motion. The building of new universities and institutions is being undertaken in the backdrop of serious concerns about the state of existing universities across the country, many of which have completed several decades of existence. There has been widespread and, often depressing discussion on the state of governance, declining academic standards, precarious funding positions, faculty shortages and a deteriorating ambience at many universities, many of which have seen better days. Legislation is often seen as the only possible approach to the problems at hand, with the result that a large number of bills dealing with higher education await the approval of a Parliament, that has many more pressing concerns on its agenda. In the run up to the 12th Five Year Plan, discussions about new initiative to repair the eroding structures of our academic institutions are being heard with increasing frequency, in Delhi's meeting rooms. Over the last few years a succession of commissions and committees have laid out road maps for resurrecting the academic system. Planning a

reform agenda appears possible; implementing even the least controversial elements of change appears impossibly difficult. I, therefore, read with some interest the Texas report, which has strongly stirred resentment in the academic community and has been widely denigrated and summarily dismissed, as motivated.

The Texas report entitled, *Higher Education's Faculty Productivity Gap: The Cost to Students, Parents and Taxpayers* is authored by Rick O'Donnell, an analyst of education policy, with an outlook that might place him at the conservative end of American policy commentators. The O'Donnell report analyses faculty performance at the University of Texas and Texas A&M University, using data provided by the institutions. The report begins by noting that these are 'the only two public universities in the country to have released such detailed data', shining 'a bright light on higher education's faculty productivity gap'. In provoking readers to plunge into the report the author advances his thesis: 'The data shows in high relief what anecdotally many have long suspected, that the research university's employment practices look remarkably like a Himalayan trek, where indigenous Sherpas carry the heavy loads so Western tourists can simply enjoy the view.' O'Donnell's analysis uses data on workloads for over 7000 faculty members at the two universities. Using two parameters, the number of students taught in an academic year and the amount of external research grants generated, he categorizes faculty in a most colourful manner, undoubtedly with the clear, albeit unstated, intention of stirring institutions out of comfortable slumber. Using descriptors that will certainly be used for some time to come, he divides faculty into five categories: *Dodgers*, *Coasters*, *Sherpas*, *Pioneers* and *Stars*. The *Dodgers* 'are the least productive faculty' who do no research, teach very little and 'have figured out how to dodge any but the most minimal of responsibilities'. *Coasters* are largely 'protected by tenure and seniority' and do little teaching and bring in negligible research funding. *Sherpas* are the ones who do much of teaching 'on a grueling time-table' and 'maintain interactions with students'. O'Donnell's metaphor is evocative, conjuring up an image of the largely unsung toilers, who have helped to build the reputations of many climbers, famed for their conquests in the Himalayas. The *Pioneers* are 'highly productive research faculty' who 'buy released time from teaching'. *Stars* are 'highly productive faculty who do a lot of teaching and a lot of funded research'. The analysis provides quantitative data, which will undoubtedly be contested, on the distribution amongst the five categories. A quick estimate for the two institutions revealed that the *Dodgers* accounted for 35–44%, *Coasters* 32–33%, *Sherpas* 21–30%, *Pioneers* 0.8–1.4% and the *Stars* trailed behind at 0.6–0.8%. O'Donnell asks some disturbing questions, which may indeed find echoes elsewhere. How did teaching loads get so light, with tenured and tenure track faculty not performing the majority of the teaching? The answer that is usually advanced is, 'that although these faculty have light teaching loads, it is because they are doing research'. For the large Texas institutions the statistics used in this report undercut this argument; approximately 20% of the faculty bring in 99% of the research funding. O'Donnell, an acerbic critic of academia, is quick to note that Pareto's 80/20 rule is 'more like 20/99 in research'. He emphasizes the need for university administrators to ask the questions that need answers if improvements are to be made. Some questions are general enough to be asked in academia across the world: 'Who are the *Dodgers* and *Coasters* and has a "Fat City" culture been allowed to flourish in any part of the university? How do we ensure the quality of teaching undertaken by the *Sherpas* many of whom are part time? Who are the *Pioneers* and *Stars* and how do we identify and recognize them, honour them and attract more of them to our universities?'

In a 'policy perspective' that asks, 'Is academic research a good investment for Texas?', O'Donnell is merciless on much of what passes for academic research. In questioning the value of scholarly papers he quotes William Broad, a former editor of the *Journal of the American Medical Association*: 'There seems to be no study too fragmented, no hypothesis too trivial, no literature citation too biased or too egotistical, no design too warped, no methodology too bungled, no presentation too self serving, no argument too circular, no conclusions too trifling or too unjustified, and no grammar and syntax too offensive for a paper to end up in print.' Even invective can sometimes be pleasing to the ear. Critics of O'Donnell's analysis have been quick to point out that 'research dollars generated' may be a poor index of productivity and, that such criteria do a disservice to scholars in humanities and social sciences. However, one cannot escape the feeling that the broad conclusions of the analysis may hold even if alternate criteria for measuring research output are used. He has been perceptive in realizing that 'big time researchers…need access to the best of the best graduate students'. His advice: 'The best way to do that is to teach.' I suspect that there will be many in our institutions who will disagree. His conclusion: 'The bottom line is that high-performing researchers are also high performing teachers for very self interested reasons. The search for talent.' Predictably, the O'Donnell audit has been widely condemned and his services terminated.

In reading about the happenings in faraway Texas one cannot but help wondering about the possible conclusions of a similar academic audit of our institutions. There is little doubt that even a cursory study will reveal many disquieting facts. Academic freedom and institutional autonomy are prized. Responsibility and accountability are rarely demanded in academia. Institutional ambience and peer pressures are the sole driving forces in maintaining standards in our laboratories and universities. If these forces do not operate, decay is inevitable. If the world class aspirations that are constantly trumpeted are to be met, we must praise and encourage the *Sherpas* even as we search for the *Stars*.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Chemistry: Shaping Bodies, Moulding Minds

We are in the midst of a year that celebrates the richness and utility of chemistry. Chemicals are ubiquitous; there is no escape from them. Biology's foundations are deeply rooted in chemistry. The DNA double helix, whose formulation heralded the birth of molecular biology, established the chemical basis of heredity. Natural selection, so fundamental to thinking about biology, operates at the level of molecules and their functions, determining the biochemistry of living organisms. The burgeoning field of materials science has chemistry at its core. Nanoscience, with its promise of spawning technologies that will transform our lives, seems suspiciously dependent on traditional chemistry. Even physicists and engineers, who seem to move with facility into the greener pastures of biology, are compelled to acknowledge that molecules and their chemistry are hard to ignore. Chemicals are often perceived as harmful, pollutants and poisons among them. The popular press is full of stories about endosulfan, an insecticide that has gained notoriety in India, and the dioxins which are slowly leached from the ubiquitous 'plastic' bottle. Chemistry's public image is not helped by the fact that chemists communicate in a language of structural formulae, which is largely indecipherable for the uninitiated. Chemicals are, however, central to life and a broad public understanding of chemistry may be useful even in shaping policy. In a commentary appearing in this issue, one of India's most prominent researchers in the area of nutrition asks, 'Can a malnourished nation become a sporting nation?' (Bamji, M. S., *Curr. Sci*., 2011, **101**, 602). Despite the widespread discussion on the spread of diabetes and obesity, problems of excess rather than scarcity, there is little doubt that undernourishment may be the fate of a majority of Indian children. Mahtab Bamji notes that nutritional deficits are alarming: 'There is rampant deficiency of micronutrients (vitamins and minerals), iron deficiency anaemia (50–70%), iodine deficiency and vitamin A deficiency being of particular concern. Deficiencies which are knocking at the door to be recognized on the basis of recent evidence are: vitamin D deficiency (despite the tropical sun), B-vitamins (folic acid, B_{12} and B_2) and zinc.' The constant and often unedifying discussions on India's sporting performances fail, in Bamji's view, to address an important issue: 'Vast segments of people in India, particularly those residing in the villages where the talent lies are malnourished and in poor health.' She emphasises the connection between nourishment and 'physical build' and quotes a report that advances the urge to rapidly build physical strength in talented but poorly nourished athletes as a reason behind the current doping scandals that have swept through the community of athletes. The reference to 'doping', the use of banned substances that enhance physical performance, caught my eye. Chemistry seemed to intrude again.

The news reports on Indian athletes highlight the fact that many appeared unaware of the nature of the drugs ingested; most believe that they were nutritional supplements. The finger of suspicion pointed to coaches, many of whom emerged from countries that were once part of the erstwhile Soviet bloc. The cries of outrage and demands for swift and exemplary punishment seemed hypocritical, especially when they emanated from those who manage sporting bodies. The fact that 'doping' is widespread in international sports only suggests that exposure of Indian athletes to performance enhancing drugs has happened at a relatively late stage, as compared to the more advanced countries. It is in the area of 'doping' that science and sport meet in full measure. While science and scientific methods can enhance sporting performance, most attention is directed at the use of substances that promote physical performance. On the eve of the 2008 Beijing Olympics, *Science* (2008, **321**, 624) asked: 'Neuroimaging, high-tech materials, new asthma meds, detection-eluding drugs, thermoregulation—will all these make athletes stronger and faster at the 2008 Summer Games in Beijing?' Undoubtedly, the question will be repeated next year when the London Olympics begin. In discussing doping, the *Science* article noted that 'by the tough standards of modern medicine there is little hard evidence for the efficacy of dozens of compounds on the list of the World Anti-Doping Agency (WADA)'. Carefully conducted trials including placebo controls are rare, with a few notable exceptions, amphetamines and the anabolic steroids among them. The former promote performance 'in short explosive activities, such as sprinting', while the latter 'increase muscle mass and enhance performance among male athletes in sports that require strength, such as weightlifting and shot-putting; in women they appear to work for endurance sports as well'. Many drugs on WADA's banned list are useless for the purpose of enhancing performance and 'are listed simply because athletes used them or were rumored to use them'. While clinical trials must precede approval of therapeutics, no real trials can be easily done to evaluate the efficacy of many chemicals in improving athletic performance. The most notorious of the many drugs that

have plagued sport is testosterone, an androgenic, anabolic steroid. A highly readable review that appeared before the Beijing games traces the use of testosterone to the 1954 World Weightlifting Championship in Vienna. The effects of androgens in improving sports performance were widely known in what is described as the 'underground press', including a source intriguingly called the 'Underground Steroid Handbook'. This is an area where the 'scientific literature was years behind' (Fitch, K. D., *Asian J. Andrology*, 2008, **10**, 384).

Analytical chemistry using cutting edge technologies is the major weapon in the battle against drug abuse in sports. The classical radio-immunoassay used in the mid-1970s, has now been replaced by gas/liquid chromatography, coupled to increasingly sophisticated mass spectrometers. However, the problems of establishing an acceptable level for an endogeneous substance can sometimes be difficult. Fitch describes in his review the tale of testosterone (T). He recounts work in the 1980s which established a criterion for the ratio of two chemicals, T and an isomer, epitestosterone (E). The T/E ratio was accepted as an indicator of ingested T. Doping with steroids was practically state policy in the German Democratic Republic (GDR, East Germany), during the period 1965–1989. From evidence which came to light after German reunification, it became clear that the most detailed understanding of the T/E ratio was possessed by the head of the GDR Doping Laboratory. When the T/E ratio became the gold standard for detection of androgen abuse, the GDR handlers administered E just before international competitions. Fitch notes that the head of the GDR program 'was a member of the IOC Medical Committee from 1981 to 1989'. The infirmities of sporting bodies appear to have a long history. As in any arms race, the advances in analytical methodology to detect the presence of banned substances have been countered by ingenious strategies to evade detection. 'Designer drugs' which can pass unnoticed are a product of innovative 'underground chemistry'. Sadly, the efficacy of many substances used by athletes is questionable, even as their detrimental effects on long term health are well established. WADA, the international watchdog has introduced a 'biological passport' for athletes. Regular monitoring of as many as eight hematological parameters is recommended to identify physiological effects even when the drug is no longer detectable. An editorial in a scientific journal calls for a 'rapid expansion of this panel to include measurements of endocrine function' and, in anticipating future developments suggests that 'the potential for gene doping must also be explored' (Heath, V., *Nature Reviews Endocrinology*, 2010, **6**, 413). In any general discussion on substance abuse one viewpoint suggests legalization of drugs as a potential solution. Should performance enhancing drugs be permitted in sports under medical supervision? A carefully presented analysis suggests that this is undesirable and concludes that any suggestions in this direction 'should not be entertained' (Wiesing, U., *Sports Medicine*, 2011, **41**, 167).

When athletes fail a doping test the resultant effect on their careers can be devastating. Is the 'science of doping' robust enough to avoid errors, which can destroy careers of innocent athletes? In the run up to the last Olympics *Nature* pronounced judgement: '…drug testing should not be exempt from scientific principles and standards that apply to other biomedical sciences, such as disease diagnostics. The alternative could see the innocent being punished while the guilty escape on the grounds of reasonable doubt' (*Nature*, 2008, **454**, 667). This editorial view was prompted by the appearance of a provocative commentary on 'The Science of Doping', which argued that 'the processes used to charge athletes with cheating are often based on flawed statistics and flawed logic' (Berry, D. A., *Nature*, 2008, **454**, 692). In testing for chemicals, both sensitivity (the ability to detect substances at very low concentrations) and specificity (the ability to detect only a very specific component in a complex mixture of substances, some of which may appear quite similar) are critical. These are key issues in medical diagnostics where the need to avoid both 'false positives' and 'false negatives' is well recognized. Berry argues that in sports drug testing validation leaves much to be desired and statistical analysis is clouded by faulty reasoning, which is termed as 'prosecutor's fallacy'. Berry uses the case of the cyclist Floyd Landis, accused of taking synthetic testosterone, to illustrate the fact that 'false positives' may not be as rare as one might expect. The reactions to the charge of flawed reasoning were quick, with critics noting that 'anti-doping is a forensic science not a medical one'; a statement that implies different standards for validating the results of chemical analysis (Sottas, P.-E. *et al.*, *Nature*, 2008, **455**, 166). Both guilt and innocence seem difficult to establish in the arena of sports drug testing.

Chemicals influence both the body and the mind. A recent essay by a prominent chemist describes how his research on molecules that act on the brain has been used to produce designer drugs that can be marketed as substances producing 'legal highs' (Nichols, D., *Nature*, 2011, **469**, 7). Indeed, work published by Nichols, on 4-methylthioamphetamine (MTA) in the 1990s, was used by 'underground chemists' to produce MTA, which when ingested caused some deaths. Laboratory chemistry never has as its objective the task of testing the safety of molecules that are synthesized. None of the hundreds of thousands of compounds produced in academic laboratories are intended for human consumption. Nevertheless, the spectre of an 'entrepreneur' finding a profitable, but dangerous, use for some classes of substances is an ever present danger that Nichols highlights. There is a dark side to chemistry.

Nutrition, sports doping and designer drugs, three diverse topics that I have touched upon in this column are tied together by the unifying thread of chemistry. We are indeed prisoners of the molecules that shape our bodies and mould our minds.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Economics and Biology: The Invisible Hand and Natural Selection

Politics and economics appear to be inseparable in human affairs. While the former has no pretensions to being a science (despite university courses called 'Political Science'), economics, especially in recent times, has advanced with substantial inputs from mathematics, statistics and, at times, even biology. Governments, in democracies, always seem to be battling either a political crisis or an economic downturn, which quickly snowballs into a political battle. Markets, inflation and recession are words that have entered everyday discourse. Growth and inflation are terms widely discussed; promoting the former while controlling the latter appears to be a desirable, but elusive, goal. In today's world, where governments battle burgeoning fiscal deficits, tightening belts seems an option that spells political doom. Economic growth is presumably fuelled by low interest rates, which allow corporates to access capital at favourable terms. Control of inflation, which appears important to most ordinary people, is traditionally done by raising interest rates and diminishing the flow of money. Monetary policy is administered in mysterious (at least to the commoner) ways by central banks, the Reserve Bank of India (RBI) in our case. Ideally, central banks are, apparently, largely free of interference by the political executive. I was therefore intrigued to come across a column which asked: 'Is this man killing India's growth', a bold headline placed alongside the benign portrait of the RBI Governor, D. Subbarao (Sugata Ghosh, *The Economic Times*, 18–24 September 2011). The columnist notes that undergraduate economics teaches us that to control inflation one must sacrifice growth. Alternatively, we must accept inflation in an economy that actively facilitates growth. The writer is reminded of Robert Frost's 'two roads that "diverged in a yellow wood"', both of which cannot be traversed simultaneously. When I read this, the cynic in me was immediately reminded of a famous literary encounter. In Lewis Carroll's classic, Alice meets the Cheshire Cat and asks for directions:

'"Would you tell me, please, which way I ought to go from here?"
"That depends a good deal on where you want to get to," said the Cat.
"I don't much care where-" said Alice
"Then it doesn't matter which way you go," said the Cat
"– so long as I get somewhere", Alice added as an explanation
"Oh, you're sure to do that," said the Cat,
"if you only walk long enough".'

(*Alice's Adventures in Wonderland*, Penguin Popular Classics, London, pp. 75–76).

One school of policy makers undoubtedly subscribes to the view that if countries 'walk long enough' on the path of growth, inflation will eventually subside.

Columnists provide suggestions and solutions to every economic crisis, their prescriptions appearing almost daily in newspapers and magazines. The courses of action suggested are often contradictory. The differences of opinion amongst economists seem to be far deeper and irreconcilable, as compared to the debates in science. Theories and experiments do not appear to settle the arguments that rage over decades between economists belonging to different schools. In the 1990s, as the Soviet Union imploded and China reinvented communism, clearly separating political philosophy from economic policy, it seemed that free market, capitalist economic ideas had won the day. State controls were anathema in an arena where markets reigned supreme. Unrestrained capitalism driven by consuming greed, based on apparently sophisticated, but fundamentally flawed, models of risk assessment, has contributed in substantial measure to the recent recession in the West, with its attendant fallout across the world. Arguments about the best means of tackling the recession, in the face of ballooning fiscal deficits, have become the subject of a fierce debate in the United States and elsewhere. Curiously, even in a struggling economy there is little political support for policies that seek to raise taxes for the rich. Economists and politicians are lining up in significant numbers on both sides of the debate. In India attempts to cut subsidies on cooking gas or diesel, as a means of raising public resources are met with stout and vocal resistance. Indeed, all those who are capable of paying the right price for goods and services (which include higher education) are among the most vocal, when there is even a hint of a discussion on reducing public subsidy. Governments need resources in order to subsidise essentials like food, healthcare and education for those who are unable to afford them. Where will these resources come from if every attempt to raise taxes and lower subsidies is fiercely resisted? Uncontrolled public expenditure and the difficult issue of endemic corruption serve to ensure that government policies are viewed with suspicion in India. Ministries and departments often work at cross purposes, leaving ordinary observers puzzled and confused. There are few clear answers to the questions that confront economic policy makers; economics and politics are enmeshed in a manner that allows only short term, and sometimes risky, solutions to be implemented. Clearly, economics and human

behaviour can hardly be separated. 'Behavioural economics' is, indeed, a respectable subject of study in academia. This is a discipline that draws its essential elements from psychology, bridging the gulf between the 'hard' sciences and the 'softer' social sciences.

Can behavioural economists learn from the long history of studies of animal behaviour in biology? Should sociobiology and natural selection become subjects that are routinely taught to students of economics? Is it likely 'that economists a hundred years from now will be more likely to name Charles Darwin than Adam Smith as the intellectual founder of their discipline?' The last question is raised and answered in the affirmative by Robert Frank in his book, intriguingly titled, *The Darwin Economy* (Princeton University Press, Princeton, New Jersey, 2011). Frank, a Professor of Economics at Cornell has subtitled his book, *Liberty, Competition and the Common Good*. He begins by quoting one of the founders of behavioural economics, Amos Tversky who was fond of saying: 'My colleagues, they study artificial intelligence. Me? I study natural stupidity.' Modern economics traces its roots to Adam Smith's, now iconic book, *An Enquiry into the Nature and Causes of the Wealth of Nations*, published in 1776. Smith's work must also be viewed in the context of the times. It appeared in the year the American Revolution began, over a dozen years before the French Revolution, well before the Industrial Revolution set in and the British Empire reached its high point. Smith observed and analysed a predominantly agricultural society in feudal times. He is quoted widely even today, most often inappropriately, for his immortal phrase, 'the invisible hand', which is a favourite with proponents of free markets. He used the phrase only twice in his classic work. In discussing an individuals labour and his contributions to society, Smith penned his famous lines: '…every individual necessarily labours to render the annual revenue of the society as great as he can. He generally, indeed, neither intends to promote the public interest, nor knows how much he is promoting it. By preferring the support of domestic to that of foreign industry he intends only his own security; and by directing that industry in such a manner as its produce may be of the greatest value, he intends only his own gain, and he is in this, as in many other cases, led by an *invisible hand* to promote an end which was no part of his intention. Nor is it always the worse for the society that it was no part of it. By pursuing his own interest he frequently promotes that of the society more effectually than when he really intends to promote it.' Enlightened self interest can often benefit groups; for Smith the 'invisible hand' aligned self interest and the common good. He is also often quoted for his observation on self interest: 'It is not from the benevolence of the butcher, the brewer, or the baker, that we expect our dinner, but from their regard to their own interest'. In *The Darwin Economy*, Frank notes that Smith was right when he argued that competition benefits consumers but argues that 'Smith never believed that the invisible hand guaranteed good outcomes in all circumstances'. Smith has had his share of detractors none more devastating than the Harvard economist Joseph Schumpeter: 'His [Smith's] very limitations made for success. Had he been more brilliant he would not have been taken so seriously. Had he dug more deeply, unearthed more recondite truth, had he used more difficult and ingenious methods he would not have been understood. But he had no such ambitions; in fact he disliked anything that went beyond plain common sense. He never moved above the heads of the dullest readers. He led them on gently, encouraging them by trivialities and homely observations, making them feel comfortable all along.' (*History of Economic Analysis*, Oxford University Press, 1954)

In asking whose views are most relevant in the modern context, Smith or Darwin, Frank argues that 'unbridled market forces often fail to channel the behaviour of self-interested individuals for the common good'. He goes on to note that 'Charles Darwin saw clearly, individual incentives often lead to wasteful arms races'. Frank advances the view that individuals (and by implication corporations) can make decisions that are departures from 'rational choice'. The outcomes can be viewed 'with regret' or 'without regret'. The former arise from what Frank terms as 'cognitive errors' which can subsequently be corrected. The more dangerous outcomes are the result of 'choices without regret', since 'we typically lack both the means and the motive to alter behaviours we don't regret, even when those behaviours generate large social costs'. Frank notes that 'Smith never believed that the invisible hand guaranteed good outcomes in all circumstances'. He adds: 'The real challenge to the invisible hand is rooted in the very logic of the competitive process, itself.' He then turns to Darwin: 'One of his central insights was that natural selection favours traits and behaviours primarily according to their effect on individual organisms, not larger groups. Sometimes individual and group interests coincide… and in such cases we often get invisible hand-like results…. In other cases, however mutations that help the individual prove quite harmful to the larger group. This is in fact the expected result for mutations that confer advantage in head-to-head competition among members of the same species'. In a chapter titled 'Darwin's wedge', Frank emphasises that natural selection, with its imperative of ensuring reproductive fitness in a relative sense, must be understood to explain the natural world. In extrapolating Darwinian principles to the arena of market economics, Frank notes that 'even perfect competition will not always guide behaviour in ways that promote the common good. Individual and group interest often diverge sharply… and in such cases individual interest generally carries the day.'

Sociobiologists who study altruism may well relate to Smith's 'invisible hand', while economists turn to Darwin.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Malaria: Recognising a Chinese Triumph

Malaria is a disease that has been with us for all of human history. Malarial parasites are known to infect all terrestrial vertebrates. They are notable for their ability to survive and develop in the environment of vectors that transmit them from host to host, spreading disease. A recent account by Francis Cox traces, in engaging fashion, the 'history of the discovery of the malaria parasites and their vectors' (Cox, F. E. G., *Parasites and Vectors*, 2010, **3**, 5). The earliest references to malaria in recorded history may be found 'in a Chinese document from about BC 2700, clay tablets from Mesopotamia from BC 2000, Egyptian papyri from BC 1570 and Hindu texts as far back as the sixth century BC'. Modern research on malaria really began only in the 19th century; driven by the imperatives of colonial expansion. The causative agent of malaria, a protozoan parasite, was first detected in human blood by a French army officer, Charles Laveran, stationed in Algeria. Laveran who was to later receive a Nobel Prize in 1907, made a discovery that was, in Cox's assessment, 'without precedent as no protozoan had previously been found inhabiting any kind of human blood cell'. Ronald Ross' subsequent demonstration that mosquitoes, specifically anopheline mosquitoes, are responsible for transmitting the disease was based on work at Secunderabad and Calcutta (now Kolkata), when he served as an army medical officer in India, between 1898 and 1899. The story of Ross is now legend, recognized by the Nobel Prize in 1902. A century later the complete genome sequence of *Plasmodium falciparum*, the most feared of the pathogens that cause human malaria, was announced (Gardner, M. J. *et al.*, *Nature*, 2002, **419**, 489).

Malarial parasites are unimaginably old, coevolving with their hosts over millions of years. Monkeys and men, mice and birds, even lizards, can harbour malarial parasites; each one fashioned by natural selection to survive and multiply within a specific host environment. *Plasmodium falciparum* is the most dangerous of the parasites that cause human disease. Its origins are still shrouded in an aura of mystery and uncertainty; a common situation when science attempts to trace the evolutionary history of living organisms. Recent DNA analyses of host populations across sub-Saharan Africa suggest that the human malaria parasite, '*P. falciparum* is of gorilla origin and not of chimpanzee, bonobo or ancient human origin'. The most virulent of the parasites that infect man may have 'resulted from a single cross-species contamination event' (Liu, W. *et al.*, *Nature*, 2010, **467**, 420). While parasites rarely, indeed very rarely, switch hosts, it is precisely this ability that has been advanced 'as an important adaptive trait of the parasite' (Hayakawa, T., *Mol. Biol. Evol.*, 2000, **25**, 2333). While the origins of the parasites are obscure, their resilience and adaptability have ensured that malaria remains a major public health issue in many parts of the world, even in the 21st century. An estimated 250 million people are afflicted by malaria today, with the annual death toll from the disease approaching one million. Vector control, in the case of diseases that are spread by insects, requires elimination of breeding sites; a task that can be a formidable civic problem in many countries. The 'mosquito net', an old reliable in the fight against malaria, remains a mainstay in current attempts to battle the spread of disease in some parts of the world, most notably Africa.

Malaria research, in the century after Laveran and Ross, has greatly advanced our knowledge of the biology of the parasite and its mosquito vector; their genomes have been laid bare by the successes of sequencing efforts, while immunology has provided many insights into the responses of human hosts to infection. Yet, vaccines, so eagerly sought after, have remained a mirage. DDT raised the hope that an alternate strategy which eliminated the mosquito vector, would break the vicious cycle of disease transmission. The spraying campaigns of the 1950s and 1960s ran into public opposition, as the long term deleterious effects of DDT exposure became widely known. DDT and the family of insecticides have proved a double edged sword, in the war on insect pathogens in agriculture and in vector control in the arena of public health. With children being at the highest risk of mortality from malarial infection, especially in Africa, widely different opinions currently prevail on the strategies for vector control. The issue of balancing short term gains versus long term concerns is difficult and, at times, contentious. The old adage that 'prevention is better than cure' has been the basis for the push towards vaccines and vector control. However, the mainstay in the battle against malaria has always been the use of agents that clear parasites from infected patients. The Spanish colonisers who descended on South America quickly encountered both the disease and remedies that were favoured by local inhabitants. The curative properties of the cinchona tree were recognized; knowledge relayed by Jesuit priests working in Peru, to the salons of Europe in the 17th century. The long trail of research, beginning with the isolation of the alkaloid quinine by Pelletier in the 1830s to

Woodward's synthesis of the molecule in the 1940s, is now part of the folklore of natural products chemistry. Quinine quickly became the key weapon in the war on malaria; the mainstay of therapy until the appearance of the synthetic antimalarials. Herbal concoctions have always been central to the practice of traditional medicine in China and India, with a long recorded history of the use of plant materials for medicinal purposes. Indeed, it is from China that artemisinin emerged as the drug of choice for the treatment of malaria caused by parasites resistant to the attack of chloroquine. The 2011 Lasker-Debakey Clinical Medical Research Award honours Tu Youyou, a Chinese scientist for her stellar role in steering the project that led to the discovery of artemisinin.

Tu Youyou, now 81, tells a remarkable story, 'The discovery of artemisinin (qinghaosu) and gifts from Chinese medicine', in a recent commentary in *Nature Medicine* (2011, **17**, xix). Tu began her career in 1955, at an institution devoted to traditional Chinese medicine. She credits a training course 'designed for professionals with backgrounds in Western medicine' for guiding her 'to the wonderful treasure to be found in Chinese medicine'. Tu was assigned to work on Project 523 which, as the Lasker award citation notes, derives its name from the date it was announced – 23 May 1967. The project was a covert operation, launched at the beginning of the cultural revolution, even as the Vietnam war intensified. This research was 'apparently prompted by the requests of Ho Chi Minh to Zhou En Lai for antimalarial drugs to protect his Vietnamese troops' (White, N. J., *Science*, 2008, **320**, 330). Tu headed a group 'comprising both phytochemical and pharmacological researchers', quickly beginning an exhaustive survey of herbal preparations. In initial work the group screened 'more than 2000 Chinese herb preparations and identified 640 hits that had possible antimalarial activities'. Using a mouse model for malaria Tu and her colleagues evaluated 'more than 380 extracts obtained from ~200 Chinese herbs'. She notes that 'progress was not smooth and no significant results emerged easily'. The observation that a qinghao (*Artemisia annua* L.) extract 'showed a promising degree of inhibition against parasite growth' was the 'turning point'. A stumbling block was encountered almost immediately; the observations were irreproducible. The Chinese team realized that the only relevant reference to the use of qinghao appeared in a classic text dating back to 340 AD – Ge Hong's *Handbook of Prescriptions for Emergencies*. A single statement provided a clue: 'A handful of qinghao immersed with 2 liters of water, wring out the juice and drink it all'. Up to this point Tu and her group had extracted the plant material at high temperature, a step that destroyed activity. Lowering the temperature did the trick. On 4 October 1971 the Chinese obtained an 'extract that was 100% effective against parasitemia', in both infected mice and monkeys. The absence of toxicity was established when Tu and her colleagues consumed the extract themselves. Clinical trials carried out quickly in Hainan demonstrated recovery of patients in a cohort where chloroquine proved ineffective. Artemisinin (qinghaosu) was purified and characterized in 1972 and its unusual structure determined in 1975. The road from the molecule to the drug was rapid. In beginning her account of the road to artemisinin, Tu draws attention to an essay of Joseph Goldstein, where he suggests that 'creation and revelation' are 'two different routes to advancement in the biomedical sciences' (*Nature Medicine*, 2007, **11**, 1151). For Tu Youyou the discovery of artemisinin was 'the revelation'; transforming the molecule into a drug was 'creation'. The artemisinin project was carried out almost entirely during China's Cultural Revolution, which lasted for an entire decade between 1966 and 1976. Both Zhou En Lai and Mao Zedong died in 1976. By 1981 an international meeting on the Chemotherapy of Malaria was held in Beijing. In 1985, a review in *Science* announced the arrival of an antimalarial drug from China (Klayman, D. L., *Science*, 1985, **228**, 1049). Artemisinin was undoubtedly discovered in China under the most difficult circumstances. Tu and her colleagues had no contact with the world of Western science.

While much of cutting edge modern, biomedical science has flourished in the West, malaria has always been a cross that has been borne by the underdeveloped world. Colonial interests drove malaria research until the middle of the 20th century; the subsequent American military involvement in the killing fields of South East Asia, home to malaria, provided a rationale for research investments until the 1970s. Pharmaceutical companies, necessarily constrained by commercial interests, have never found any strategic advantage in malaria research. Current efforts to control and combat malaria rely largely on rapidly growing funding from philanthropic foundations. But increases in resources alone may be inadequate in the war on malaria. Resistance is a constant threat, as natural selection renders both the parasite and the mosquito vector impervious to chemical attack, after constant exposure. The first signs of artemisinin resistance in *P. falciparum* malaria appear to have surfaced along the Thai–Cambodia border, 'historically a site of emerging antimalarial resistance' (Dondorp, A. M. *et al.*, *New England Journal of Medicine*, 2009, **361**, 455).

In concluding her account of the artemisinin saga, Tu suggests that there may be other gifts from Chinese medicine. In a statement that may resonate with proponents of Ayurveda, Tu notes: 'However, the use of a single herb for the treatment of a specific disease is rare in Chinese medicine. Generally, the treatment is determined by a holistic characterization of the patients syndrome, and a prescription comprises a group of herbs specifically tailored to the syndrome'. In reading about Tu Youyou I turned, inevitably, to *Wikipedia*. She is described (albeit under the heading '*trivia*') as 'the *Professor of Three None's* (no postgraduate degree, no study/research experience abroad, not a member of any Chinese national academies'. Artemisinin and its life saving properties are surely an unmatched reward.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Seeing is Believing: Quasicrystals and the Demise of Perfect Order

The Nobel prize in Chemistry is often awarded to scientists who are formally viewed as practitioners of other disciplines. Ernest Rutherford, who professed a considerable disdain for subjects other than physics, was anointed a laureate in chemistry in 1908. Since then, biologists, physicists and even barely disguised mathematicians have been inducted into chemistry's hall of fame; a tribute to the richness of a field, which shares its borders with many other disciplines of science. This year's Nobel award to Dan Schechtman for his discovery of quasicrystals once again recognized a major scientific advance that, at first glance, appeared to be far removed from the concerns of traditional chemistry. Shortly after the announcement a physicist colleague exclaimed: 'Ah, this is a physics prize'. Not to be outdone, a colleague in materials engineering, classically metallurgy, proclaimed: 'This is a prize for materials.' Curiously, my chemistry colleagues seemed subdued, although the study of materials might legitimately be classified under chemistry. Indeed, the vast majority of the practitioners of science, in the years before disciplinary boundaries crystallized sharply, were engaged in understanding the nature of matter, living and non-living, organic and inorganic, gaseous, liquid and solid. Alloys are the province of the metallurgist; materials whose utility has been immense. The serendipitous observation of a remarkable electron diffraction pattern in a manganese–aluminium alloy by Schechtman in 1982, led to the discovery of quasicrystals, eventually transforming our understanding of the crystalline state of matter. In a lecture at Schechtman's seventieth birthday celebration at the Technion in Haifa, chemical historian and structural chemist Istvan Hargittai reflects on 'the lessons of a discovery' (*Struct. Chem.*, 2011, **22**, 745). Crystallography has its roots firmly embedded in the mid-19th century in the work of Bravais on periodic lattices. By the turn of the century it was clear that periodic, long range order imposed restrictions on the observable symmetries in crystals. The 20th century was marked by the remarkable growth of X-ray diffraction beginning with the work of Lane and the Braggs. Chemistry and biology have been propelled along by the spectacular successes of X-ray crystallography, to an extent that X-rays and crystals seem inextricably linked. Schechtman's observation of 'impossible' symmetries under the electron microscope appeared to fly in face of established principles. Five-fold and ten-fold symmetries that he observed seemed easier to dismiss as artefacts of twinning of crystals. Hargittai enumerates several lessons that might be learnt from Schechtman's discovery. Amongst the most important pieces of advice he gives are: 'Do not discard the unexpected' and expose your findings 'to wide scrutiny'. In Hargittai's assessment published earlier this year: 'The discovery caused a paradigm change in physics, chemistry, crystallography and materials science. It deserves the highest recognition.' That has now happened.

In a prelude to Schechtman's experimental discovery, Alan Mackay visited the relationship between 'crystallography and the Penrose pattern', noting that 'equivalence is replaced by quasi-equivalence' in Penrose tiling schemes. He considered the possibility of 'tiling of two-, three- (and perhaps four-) dimensional space by tiles of two kinds in a unique non-periodic pattern'. In reviewing the field nearly two decades after the initial discovery, J. W. Cahn, a coauthor on the seminal 1984 paper (Schechtman *et al.*, *Phys. Rev. Lett.*, 1984, **53**, 1951), writes that 'the study of quasicrystals benefited greatly from prior research in the mathematical subjects of quasiperiodic functions, aperiodic tilings and hyperspace crystallography'. He notes that the discovery has 'led to much interdisciplinary activity involving mainly materials science, physics, mathematics and crystallography' (*J. Res. Natl. Inst. Stand. Technol.*, 2001, **106**, 975). Materials science and crystallography today are really under the broad umbrella of chemistry.

Are quasicrystals of great practical utility? In 1999, an overview of the area listed a number of potentially useful applications for the hundreds of alloys with quasicrystalline structures, that appeared in the wake of Schechtman's finding (Jacoby, M., *Chem. Engg. News*, 1999, **77**, 44). New cooking surfaces and hardening steels are hardly applications likely to excite the Nobel committee. Two previous awards for work done in the 1980s, that might broadly be classified as 'materials science', come to mind when thinking about quasicrystals. The discovery of high temperature superconductivity in 1985 in the perovskite type oxides was announced in a modestly entitled publication: 'Possible high T_c superconductivity in the Ba–La–Cu–O system' (Bednorz, J. G. and Muller, K. A., *Zeit. Phys.*, 1986, **B64**, 189). The perovskite structures were well known in this class of materials. There was little glamour associated with their structures or their chemistry. Their promise as materials that would make superconductors widely accessible touched off a frenzy of activity, propelling the discoverers to a Nobel Prize in 1987, a remarkably short time after the first publication.

The enthusiasm for high temperature superconductors, that might revolutionise power transmission and generate a whole world of new applications, is now muted. At around the same time in September 1985, a new form of carbon, C_{60} or Buckminsterfullerene, was discovered by Harold Kroto and his colleagues (*Nature*, 1985, **318**, 162). C_{60} attracted immediate attention; chemists of all hues were seduced by the beauty of its structure. To transform a substance as prosaic as graphite into a molecular object of great symmetry and beauty was an act reminiscent of the elegant swan emerging from the ugly duckling. The beguiling symmetry of the material, fuelled a search for applications which still continue; albeit, with less enthusiasm as newer forms of carbon, nanotubes and graphene, have emerged. Fullerene (C_{60}) has the same structure as a soccer ball (football, to the world outside America). Kroto, Curl and Smalley received a Nobel prize in 1996, just over a decade after their discovery. In his Nobel lecture Kroto notes 'that, perhaps, the molecule's most delightful property lies in its inherent charisma which arises from its elegantly simple and highly symmetric structure that is quite unlike any other'. Schechtman's quasicrystal announcement which appeared in 1984 has transformed the way we view the solid state; indeed the beauty of crystals is enhanced by expanding their universe to encompass aperiodic structures.

The criticism of the idea of quasicrystals and forbidden symmetries in crystalline solids by Linus Pauling, soon after publication of the Schechtman paper in 1984, has been widely written about after the Nobel announcement. There is an appealing David and Goliath angle to the early controversies about quasicrystals. Pauling (1901–1994) was 84 years old when he began his criticism of the idea of quasicrystals, advancing instead alternative explanations for the observed symmetries in diffraction patterns. His credibility had eroded in the later years of his long and extraordinary career, especially in his controversial espousal of vitamin C as a universal therapy. He wrote his last paper, clinging to his interpretations, just before he turned 90 (*Proc. Natl. Acad. Sci*., 1990, **87**, 7849). There were many studies, in laboratories across the world, that quickly followed Schechtman's publication, including work at Bangalore which lent support to the idea of quasicrystals (Chattopadhyay, K. *et al.*, *Curr. Sci.*, 1985, **54**, 895). The theoretical underpinning was strong, with the work of Mackay and Levine and Steinhardt (*Phys. Rev. Lett*., 1984, **53**, 2477). Pauling's formidable reputation did not deter many critics of his twinning hypothesis (cf. Heiney, P. *et al.*, *Phys. Rev. Lett*., 1987, **59**, 2119). Hargittai notes that 'Pauling died in 1994 without accepting Schechtman's discovery'. It is not clear whether Pauling by then had many followers. Hargittai quotes Max Planck: 'A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.'

Students of the history of chemistry will recall Herman Kolbe's famous attack on van't Hoff's proposal in 1874 of the tetrahedral disposition of the bonds about a saturated carbon atom; an idea that catapulted chemistry into the third dimension: 'A Dr J. H. van't Hoff of the Veterinary School at Utrecht has no liking, apparently for exact chemical investigation. He has considered it more comfortable to mount Pegasus (apparently borrowed from the Veterinary School) and to proclaim in his *La Chimie dans L'espace* how the atoms appear to him to be arranged in space, when he is on the chemical Mt. Parnassus which he has reached by bold flight.' Harshly worded attacks were common in the 19th century when some of the most profound concepts of modern science were born. In van't Hoff's day, chemists used the idea of atoms as a convenient device to picture the bewildering range of molecules being uncovered by organic chemistry. Physicists of the time were impatient with a discipline that seem to be based on tenuous imagery. Von Helmholtz famously said: 'The whole extraordinarily comprehensive system of organic chemistry has developed in the most irrational manner, always linked with sensory images which could not possibly be legitimate in the form in which they are presented' (Riddell, F. G. and Robinson, M. J. T., *Tetrahedron*, 1974, **30**, 2001). I cannot think of past battles over the structure of matter without turning to Boltzmann. In his classic narration of the *Ascent of Man*, Jacob Bronowski stands at Boltzmann's grave and reflects: 'Who would think that only in 1900, people were battling, one might say to the death, over the issue whether atoms are real or not. The great philosopher Ernst Mach in Vienna said, No. The great chemist Wilhelm Ostwald said, No. And yet one man, at the critical turn of the century, stood up for the reality of atoms on fundamental grounds of theory. He was Ludwig Boltzmann…. Did Boltzmann just argue? No. He lived and died that passion. In 1906, at the age of sixty two, feeling isolated and defeated, at the very moment when atomic doctrine was going to win, he thought all was lost, and he committed suicide.' In remembering past battles in an area of science that has seen its share, the duel over the nature of quasicrystals seems mild.

Quasicrystals were uncovered by the electron microscope. Microscopy has come a long way since van Leeuwenhoek discovered a 'world within a world' in the latter half of the 17th century. Electron microscopy promises to provide even more surprises as the power of the method grows with every technical advance. Some years ago Alan Mackay wrote that 'if we can escape from our preconceptions engendered by the immense success of X-ray single-crystal structure analysis', we might expect 'still more varied structures' that 'lie outside the austere domain of classical crystallography' (*Nature*, 1988, **391**, 334). Gautam Desiraju, another perceptive commentator, noted: 'As chemistry moves towards systems of greater complexity and diversity, the meaning and scope of the term "crystal" can only evolve and expand' (*Nature*, 2003, **423**, 485). Schechtman 'saw' quasicrystals in the images produced by an electron microscope. Most often, seeing is believing.

P. Balaram

CURRENT SCIENCE

Volume 101 Number 9 10 November 2011

EDITORIAL

Systems and Synthetic Biology: Physiology and Biochemistry Strike Back

Biology has advanced on a very broad front over the past half a century. By the mid-1970s the recombinant DNA revolution was on and the term genetic engineering became commonplace. The Boyer–Cohen experiment of 1973 produced the first 'cloned DNA', a key step in the thrust to produce protein pharmaceuticals. Genentech, the first modern biotechnology company, was born in 1976. Four years later when the company went public, its initial stock offering raised over $38 million and its share price more than doubled in a few minutes of trading. At that moment, the company had no marketed product and its dramatic initial success in the world of business rested on the promise of the new biotechnology (*Genentech*: *The Beginnings of Biotech*, Sally Smith Hughes, University of Chicago Press, 2011). For the past three decades biotechnology has been propelled forward by several major scientific advances that have emerged from research laboratories, the polymerase chain reaction, DNA sequencing and synthesis, methodologies for protein expression and purification and, of course, the dramatic progress of biological research cutting across the disciplines of cell biology, microbiology, plant and animal science. Physics and chemistry have repeatedly intruded, as structural biology has, time and again, demonstrated the virtues of reductionism. By the 1990s biology's leading front was genomics, fuelled by the rapid pace of advance in DNA sequencing technologies. Whole genome sequencing promised to reveal 'genetic blueprints' for organisms as diverse as bacteria and man. A decade has passed since the successful completion of the campaign to sequence the human genome. The champions of genomics proudly proclaim that individuals can now have their genomes sequenced; a first step in entering the brave new world of pharmacogenomics and personalized medicine. Even as the power of sequencing has grown and costs have fallen there appears to be an air of disquiet. Has genomics delivered on the many promises of its most ardent champions? Has the cause of human health been advanced by our intimate knowledge of the genome? Has Nature concealed more than it has revealed in the genome? In a thoughtful commentary, a prominent geneticist voices the concerns of an insider: 'Translating current knowledge into medical practice is an important goal for the public who support medical research, and for the scientists and clinicians who articulate the critical research needs of our time. However, despite innumerable successful gene discoveries through genomics, a major impediment is our lack of knowledge of how these genes affect the fundamental biological mechanisms that are dysregulated in disease. If genomic medicine is to prosper, we need to turn our attention to this gaping hole' (Chakravarti, A., *Science*, 2011, **334**, 15). Translating the successes of basic research in biology into practical medical application can be an extremely difficult task. In making the connection between variations in DNA sequence and disease, the complexities of the mechanisms that underlie clinical problems intrude, rather rudely.

Mendelian diseases, caused by variations in single genes, sickle cell anaemia being the classic example, are easily identified by genetic analysis. The greatest challenges of our times, cancer, diabetes and neurodegenerative disease amongst them, are more complex, leading Chakravarti to note that, 'we need to answer not only which DNA variants in which genes lead to disease, but how they do so'. He is succinct and emphatic: 'The lessons from genome biology are quite clear. Genes and their products almost never act alone, but in networks with other genes and proteins…. Genome biology now needs to move to cell biology and physiology (systems biology) to understand how genetic perturbations lead to downstream dysregulation of proteins, their networks, and cells in disease.'

Genomic research was marketed in the aftermath of the sequencing successes as the driver of a revolution in clinical medicine. The euphoria of the early days is now muted and some pessimists have begun to wonder if genomics and high throughput genetics will indeed have a significant impact on medical practice. What then does the future hold for biology in the post-genomic era? Bruce Alberts argues that biology's 'grand challenge' would be to 'aim at a complete understanding of the simplest free-living cells'. He quotes Richard Feynman – 'What I cannot create, I do not understand' – in arguing that the growing field of 'synthetic biology', with its aim of 'creating networks' has the 'promise to become a powerful new tool' in fuelling biology's next major advance (*Science*, 2011, **333**, 1200). Are the terms systems and synthetic biology descriptors of a new approach to biology? Has the reductionist approach of studying individual components of complex biochemical systems finally reached the end of the road? Has the deluge of data,

genomic, proteomic and metabolomic finally drowned reductionist biochemistry? In confronting these uncomfortable questions we might draw solace from Sydney Brenner's view: 'The problem of biology is not to stand aghast at the complexity but to conquer it'. Brenner is acerbic in his characterization of biology's current fashion: 'You see, everybody's running around talking about systems biology and integrative biology. It's nothing new. It's called physiology' (*Discover*, April 2004).

Molecular biology in its tumultuous advance in the second half of the 20th century completely overwhelmed two classical disciplines, physiology and biochemistry. Ironically, it is these very disciplines which now appear critical in advancing biology's new frontiers. In tracing the roots of 'systems biology' Denis Noble, an eminent physiologist, draws attention to the fact that one of the field's most famous successes was when Hodgkin and Huxley 'integrated their equations for the nerve impulse in 1952'. Noble notes that Claude Bernard could legitimately be called 'the first systems biologist' and also the first proponent of mathematical biology. Bernard 'formulated the systems principle of control of the internal environment' in 1865, a cornerstone of modern physiology. He also believed that the 'application of mathematics to natural phenomena is the aim of all science, because the expression of the laws of phenomena should always be mathematical' (Noble, D., *Philos. Trans. R. Soc. A*, 2010, **368**, 1125). The growing emphasis on systems biology is a consequence of the recognition that the connection between single genes (and their products) and physiological function is often tenuous. The new technologies of 'gene knockouts' have led to the conclusion that organismal functions often remain unimpaired even when a gene is rendered dysfunctional, leading to the conclusion that underlying protein networks are robust, clearly buffered against individual failure at some connections (Noble, D., *J. Physiol.*, 2011, **589**, 1007). Noble suggests that individual gene-centric approaches must give way to discussions of 'cooperative genes', conceptually and metaphorically distinct from Richard Dawkins' 'selfish gene'. Noble emphasises the need for physiology to 'reconnect with developmental and evolutionary biology', with computational systems biology developing as a key tool.

If systems biology hopes to provide a broad understanding of the complexities of cellular processes, synthetic biology will provide a test of the emerging conceptual framework. Understanding must precede design. Much can be learnt when design strategies do not yield the desired result. 'The allure of synthetic biology' rests to a large extent on the possibility that gene manipulation can be extended to 'DNA-encoded circuits' (protein networks and metabolic processes with complex interconnections). Synthetic biologists talk of 'rewiring cellular circuitry to control biological processes' (Vinson, V. and Pennisi, E., *Science*, 2011, **333**, 1235); a language that bears a resemblance to the control of networks, so well understood in the world of electrical engineering and communications technology. Synthetic biology appears to promise a control over cellular chemistry that could hardly have been taken seriously in the era before the genomics revolution. A recent special section in *Science* (2 September 2011) highlights the potential of synthetic biology, drawing attention to many projects that might have been considered inconceivable even in the last decades of the 20th century. Rewiring cells may appear a difficult process, but there is a hope that this area will also succumb to the power of the new technologies. Ongoing work attempts to develop a methodology called multiplex automated genome engineering (MAGE), where millions of normal bacteria, *E. coli*, enter a 'contraption' and a 'menagerie of microbes with new genomes comes out the other end' (Bohannon, J., *Science*, 2011, **333**, 1236). The special section in *Science* emphasises the promise of synthetic biology in transforming the drive to produce algal biofuels, a process that would not compete for agricultural land. The promise in biomedicine and in the clinic appears immense; cynics may recall that this was true also of genomics in the midst of the sequencing revolution. Bioengineering, a field that is yet to emerge as a coherent discipline from its intensely interdisciplinary origins, will undoubtedly be an area where synthetic biology will make its major contributions. It is here that a hard question will be asked: 'How synthetic can synthetic biology be?' (Schwille, P., *Science*, 2011, **333**, 1252).

The passing of the golden age of molecular biology, where reductionism triumphed repeatedly in advancing our understanding of cellular processes, has been acknowledged, even by the pioneers of the field. Sydney Brenner notes that 'no use will be served by regretting the passing of the golden years of molecular genetics'. He argues that 'we should welcome with open arms everything that modern technology has to offer us but we must learn to use it in new ways. Biology urgently needs a new theoretical basis to unify it and it is only theory that will allow us to convert data into knowledge'. In contrast to many of the proponents of systems biology, Brenner argues for a new thrust to the reductionist view. In his view it is not DNA or the genome that is the focus as the fundamental element of biology; rather, 'the correct level of abstraction is the cell' (*Philos. Trans. R. Soc. A*, 2010, **365**, 207). Where does all of this leave biochemistry? Brenner once remarked 'that two things disappeared in 1990: one was communism, the other was biochemistry and that only one of these should be allowed to come back'. He went on to add: 'We do not have to resurrect biochemistry, and it will flourish because it provides the only experimental basis for causal understanding of biological mechanisms' (*Trends Biochem. Sci*., 2000, **25**, 584). Biology's future may rest on returning to its roots, as physiology and biochemistry strike back in the age of systems and synthetic biology.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Journals: Costs and Impact

Rupert Murdoch and his publishing empire have gained considerable notoriety in recent times. His enormously profitable enterprise has been subjected to a great deal of scrutiny. Publishing tabloids must be, or so I thought, a considerably more lucrative business than the stodgy world of publishing scientific journals. An article in the *Guardian* by George Monbiot (29 August 2011) drew my attention, instantly, since it boldly proclaimed, 'Academic publishers make Murdoch look like a socialist'. The column drew attention to one of my pet peeves – the costs of maintaining academic libraries, where science journals 'consume 65% of their budgets'. Scientists need journals and journals need publishers. Scientists are, most often, authors, readers, reviewers and editors. Publishers bear the costs of management and production, adding value in many ways to the final product. The *Guardian* article asks provocative questions and provides even more provocative answers: 'Who are the most ruthless capitalists in the Western world? Whose monopolistic practices make Walmart look like a corner shop and Rupert Murdoch a socialist? You won't guess the answer in a month of Sundays. While there are plenty of candidates my vote goes not to the banks, the oil companies or the health insurers, but – wait for it – to academic publishers…. Of all the corporate scams, the racket they run is most urgently in need of referral to the competition authorities.' This, rather dramatically worded, critique of academic publishing monopolies may be important, especially as Indian institutions appear to be spending larger and larger sums of money on journals, with little thought of strong and collective action against unreasonable journal pricing, even as the print medium rapidly gives way to electronic communication. The *Guardian* piece provides an interesting discussion of costs: 'You might resent Murdoch's paywall policy, in which he charges £1 for 24 hours of access to the *Times* and *Sunday Times*. But at least in that period you can read and download as many articles as you like. Reading a single article published by one of Elsevier's journals will cost you $31.50, Springer charges €34.95, Wiley $42. Read 10 and you pay 10 times. And the journals retain perpetual copyright. You want to read a letter printed in 1981? That'll be $31.50.' On reading this the reactions of most readers would probably be one of puzzlement. After all, researchers almost never pay for reading single articles; instead they rely on institutional libraries, both real and virtual. The *Guardian* critique anticipates this argument: 'Of course you could go into the library (if it still exists). But they too have been hit by cosmic fees. The average cost of an annual subscription to a chemistry journal is $3,792. Some journals cost $10,000 a year or more to stock… Though academic libraries have been frantically cutting subscriptions to make ends meet journals now consume 65% of their budgets, which means they have had to reduce the number of books they buy. Journal fees account for a significant component of universities' costs which are being passed to their students.' This concern about unreasonable journal costs is not widely shared in India, since public institutions and their libraries turn to government to continually cover escalating budgets. Publishing cartels are usually successful in convincing individual researchers and libraries about the importance of their journals and 'information products', with the result that there is very little user oversight in fighting rapidly increasing prices.

Is academic publishing a profitable business? The *Guardian* article notes that while newspaper and magazine publishers, Murdoch among them, pay journalists and editors and often pay for content, 'academic publishers get their articles, their peer reviewing … and even much of their editing for free. The material they publish was commissioned and funded … through government research grants and academic stipends. But to see it, we must pay again, and through the nose.' According to the *Guardian* critique 'Elsevier's operating profit margin was 36% (£724 m on revenues of £2 bn)' last year. Three commercial publishers, Elsevier, Springer and Wiley now publish nearly '42% of journal articles'. Interestingly, the profit margins in academic publishing have remained at about 35–40%, for well over a decade. The *Guardian* notes in an aside: 'Perhaps it's not surprising that one of the biggest crooks ever to have preyed upon the people of this country (UK) – Robert Maxwell – made much of his money through academic publishing.' Maxwell was the buccaneer publisher who, among other things, owned Pergamon Press which in the 1960s introduced *Tetrahedron Letters*, with camera ready copy prepared by authors for printing. The journal also fuelled the rush to publish shorter and shorter papers, eventually leading to discussions about 'least publishable units' of research,

especially in the area of chemistry. Maxwell was notorious for his business practices and after his death in 1991, the sum of £470 million was found missing from the company's pension fund account. Twenty years later a few commercial publishers have established a stranglehold on scientific journals, with only a few scientific societies holding out.

Modern scientific journals can trace their origins to 1665 when the Royal Society produced *Philosophical Transactions*. In the 17th, 18th and much of the 19th century, science had not yet fragmented into well defined disciplines. Natural philosophy was a favoured term to describe the areas that were of interest to practitioners of science. Societies are today the publishers of some of the most influential journals in science. Yet many journals produced by societies in Europe and elsewhere have been brought under the fold of commercial publishers, who promise a degree of professionalism in production and marketing, guaranteed to increase a journal's visibility. American societies, especially the larger ones, continue to publish highly regarded journals. As more and more of the world's scientific output is brought under the umbrella of commercial monopolies, difficult and uncomfortable questions arise regarding costs of access. The Open Access movement has gained in strength as researchers and institutions realize that publishers add little value to papers during the production process. The *Guardian* article quotes a Deutsche Bank analysis: '…if the process really were as complex, costly and value-added as the publishers protest that it is, 40% margins wouldn't be available'. Publications have been the lifeblood of science; progress at every critical step in the history of modern science has been marked by the appearance of a paper that, in retrospect, marks a turning point in the discipline. Today publications are a measure of scientific output; a tangible outcome of research carried out, invariably, with public funds. As the scientific enterprise has expanded, so too have the number of journals.

In the last few years the rise of scientometrics and the growing use of quantitative indices, necessarily derived by analysing publication output, have transformed the way scientists view the world of journals. There is intense pressure to publish and to 'target' journals based on impact factors. There is a growing desire amongst individuals to maximise their *h*-indices; a convenient and easily measurable parameter that may be likened to a personal impact factor. Citation counting lies at the heart of many quantitative assessments. Individuals, institutions and even journals have now become part of a game to boost citation statistics. Since rewards for individuals, rankings for institutions and sales figures for journals hinge on quantitative parameters, there are almost no options available to escape from avaricious publishers, who 'have rounded up the journals with the highest academic impacts factors'.

The tyranny of impact factors has begun to have an effect on 'scientists working in small under-researched non-lucrative fields'. In an editorial appearing in 2007 in the journal *Folia Phoniatrica et Logopaedica* (*FPL*, 2007, **59**, 281), H. K. Schutte and J. G. Svec cited 66 of the articles published in 2005–2006 in the same journal. They successfully drove up the impact factor from 0.655 in 2005 to 1.439 in 2007, demonstrating that strategies for doubling impact can be readily devised. As a journal catering to a specialized research community in the area of 'phoniatrics, speech therapy and communication pathology', the authors appear to be registering a protest. Their reward was swift in coming. Thomson Scientific, the publishers of *Journal Citation Reports*, removed *FPL* from its 2008 listing. A more recent example of a journal's meteoric rise in the rankings is the case of *Acta Crystallographica Sect. A*. The journal rose to the second position, with an impact factor of 49.9 in 2010, a phenomenon that appears to have surprised even the analysts at Thomson-Reuters. Writing on the website of the Citation Impact Centre, M. McVeigh traces this remarkable rise to a single paper by George Sheldrick, which amassed an extraordinary 6,436 citations in a two-year period. The conclusion: 'Without another important article in 2010 *Acta Crystallographica Sect. A* is likely to return in 2011 to its prior journal impact factor of between 1.5–2.5.' The champion article, entitled 'A short history of SHELX', describes the evolution of a widely used program for crystal structure determination. The paper attempts 'to understand how a program originally designed for photographic intensity data, punched cards and computers over 10,000 times slower than an average modern personal computer has managed to survive for so long' (Sheldrick, G. M., *Acta Cryst*., 2008, **A64**, 112–122). Curiously the article carries an invitation to cite: 'This paper could serve as a general literature citation when one or more of the open-source SHELX programs (and the Bruker AXS version SHELXTL) are employed in the course of a crystal structure determination.'

Most journals and the bibliometric databases like Thomson's *Web of Science* and Elsevier's *Scopus* are in the hands of commercial publishing houses. Scientists and administrators the world over, and India is no exception, have succumbed to the temptation of using single number parameters for assessment of individuals and institutions. A recent advertisement from a government funding agency asked applicants to provide 'their *h*-index for the past five years'; a sharp reminder that bibliometrics has spread like a cancer in the corridors of the science departments in Delhi. As the number of scientific institutions grows rapidly in India, there is an urgent need to review journal pricing policies of commercial publishers and to adopt a more informed and tempered view of bibliometric indices in assessments of scientific research.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Alice: Science and Common Sense in Wonderland

Scientists, especially those in academia are required to write scholarly papers; an activity that frequently exposes them to withering, anonymous criticism from their colleagues who serve as peer reviewers. Papers and research proposals, submitted for funding, can draw fire from snipers whose identity and positions remain unknown. Researchers who amass formidable lists of publications must be both resilient and thick skinned. Running repeatedly through the gauntlet of anonymous peer review requires a great deal of staying power. To be unfazed by trenchant criticism is indeed an attribute that is needed for success in science and many other fields. Writers, in general, are fair game for critics. In writing these columns, with a painful regularity, I have encountered my share of criticism. At times, the critics have taken issue with the opinions expressed; on other occasions with the content, sometimes asking why the columns seem to address topics of little relevance, especially when Indian science faces apparently insurmountable problems. My defense has always been that writers, particularly those with deadlines to meet, choose their topics primarily with the intention of producing a passable essay. The task of providing solutions to weighty national problems must inevitably be left to optimists with the imagination and energy to advance strategies that have not been considered before. There are also purists, quick to pounce upon stylistic shortcomings, amongst the tribe of critics. Their sensibilities are easily offended by lapses in grammar and punctuation. Foreign authors who submit papers to American and British journals are often advised to have their manuscripts scrutinized by a 'native English speaker'; tacitly assuming that superior writing skills are possessed by the 'natives'. I have encountered my share of carping critics. A scientific paper of mine once drew a stinging rebuke from a referee who disliked 'split infinitives'; sending me scurrying to find a long forgotten textbook of grammar, in the days before the internet made information searches so comfortable (*Curr. Sci*., 2004, **86**, 239). More recently, glaring lapses of punctuation were pointed out by a perceptive reader: 'If the commas are not simple typos, I can only think of Thoreau: If a man does not keep pace with his companions, perhaps it is because he hears a different drummer.' Suitably chastised, I consoled myself by recalling the rest of the Thoreau quote: 'Let him step to the music which he hears, however measured or far away.' My weakness for quotations drew a rather plaintive question from a friendly and often, encouraging colleague: 'Why do you have to quote so frequently from *Alice's Adventures in Wonderland*? Must I go and read the book again?'. My fondness for *Alice* is an inherited trait; presumably a combination of both 'nature and nurture', a model favoured by many post-genome geneticists. My father, a bureaucrat, was justly proud, till the very end, that he had successfully quoted from Lewis Carroll's book on the margins of a government file. That was, of course, in the days before the Right to Information Act (RTI) gave everyone the chance to educate themselves by reading notings on government files. Are *Alice* and Carroll's sequel *Through the Looking Glass* truly books for children? Do they only fascinate adults who have refused to grow up or are Carroll's classics educational in a more general sense?

Alice first appeared in 1865 and *Looking Glass* in 1872. Carroll fans will undoubtedly look forward to a grand celebration in 2015, the 150th anniversary of the appearance of a book for children, which has stimulated a large number of scholarly studies seeking to decipher the hidden meaning in a classic that has endured for so long. In 1960, Martin Gardner, the great popularizer of mathematics who died last year, produced the *Annotated Alice*, an invaluable aid to anyone who seeks to uncover the hidden sense, amidst the nonsense of *Wonderland*. Charles Dodgson the Oxford mathematician, immortalized by his pseudonym Lewis Carroll, the creator of *Alice*, lived and wrote at a time when science was on the brink of a major upheaval. Darwin's ideas on the origins of diversity in biology and the imprint of natural selection were beginning to filter through academia in England. Chemistry was stirring restlessly in Europe; atoms and molecules were in the air. Pasteur had announced the resolution of racemic tartaric acid. Stereochemistry had been conceived. Symmetry or the lack of it seemed critical for life. Van't Hoff and La Bel would midwife the tetrahedral carbon atom into existence in 1874, barely three years after Carroll produced *Looking Glass*. Physics and physicists viewed with some unease the imagery of 19th century chemistry. Atoms, valencies and chemical formulae seemed artificial devices to rationalize a craft that seemed to have descended from the alchemy of the earlier centuries. The structure of matter was shrouded in mystery, although the properties of matter seemed so wonderfully useful. This sense of unease pervaded to the

remarkable attributes of space, empty space, a mystical aether through which natural phenomena seemed linked. Faraday and Maxwell had united electricity and magnetism; light seemed to emerge from this union. Boltzmann, 'an early follower of Darwin' in the words of Bronowski, had begun to grasp the reality of the atom and to link heat and disorderly motion. Entropy and chaos would have been welcome in Alice's wonderland. Physics in Carroll's time was teetering on the brink of a new order in which space and time would meet in a manner that most of us find decidedly strange, even a century later. Did physics, chemistry and biology intrude into Carroll's thinking; was the mathematician and logician Charles Dodgson speaking of strange, new views of the world through the enduring characters in the mad world of wonderland?

In musing about my colleague's question about *Alice*, I was delighted to chance upon Gillian Beer's essay, *Alice in Time* that appeared recently in *Nature* (2011, **479**, 38). The author, an emeritus professor of English literature and an academic of great distinction, explores 'the passage of time in its many manifestations through Lewis Carroll's enduring classics. Time enters *Wonderland* when the White Rabbit pulls a stopwatch out of his waistcoat pocket and hurries on muttering to himself "*Oh dear! Oh dear! I shall be too late!*" Alice follows him down the rabbit hole falling endlessly, in a way that gravity and Newton seemed to misbehave, as she tumbled into *Wonderland*. Beer notes that photography, invented in Carroll's time 'froze and made portable a moment and a place'. She adds that from the 1850s Hermann von Helmholtz, physicist and physiologist, was interested in 'the measurement of the nerve impulse', in attempting to understand 'the speed of thought and reaction time'. Beer points out that Helmholtz 'summarized many non-Euclidean insights of the previous two decades', in 1870. Beer notes that Helmholtz concluded by citing German mathematician Georg Riemann's 'somewhat startling conclusion, that the axioms of Euclid may be, perhaps, only approximately true'. Life in worlds of different dimensions seemed to tease the imagination. Beer concludes that 'Carroll did not follow this non-Euclidean thinking professionally but let it loose in his fiction'. Alice's changing shape and sizes as she stumbles through *Wonderland* can fascinate children; in the many movie versions of *Alice* they can be frightening. In Beer's words: 'The child's everyday and helpless experience of growing, and of being always the wrong size in a world designed by adults, is meshed with mathematical speculation.' Time and space intrude in special ways in *Through the Looking Glass*. Beer suggests that Carroll seems to have conceived the story 'on the plane of a chessboard, later adding the optical reversing effects of the mirror'. He appears to have been influenced 'by his friend J. J. Sylvester' who in a 'presidential address to the 1866 British Association for the Advancement of Science' discussed reaching out to a fourth dimension. In Beer's analysis, 'Alice becomes aware that our mode of living in time is not the only pattern available'. She recalls the Red Queen's dismissal of Alice's world where days come one at a time: *That's a poor thin way of doing things. Now here, we mostly have days and nights two or three at a time, and sometimes in winter we take as many as five nights together – for warmth, you know*. The Mad Hatler's tea party, in Beer's essay, may have anticipated Georg Cantor's 'degrees of infinity as well as an infinite infinity'.

Alice scholars find not only allusions to mathematics and physics in Carroll's books; chemistry and biology also make an appearance. Jack Dunitz in presenting an overview of 'symmetry arguments in chemistry' notes that Carroll 'makes several references to the left/right dichotomy' in *Looking Glass*. Alice's musing to her kitten: 'Perhaps looking-glass milk isn't good to drink' is now quoted in most discussions of molecular chirality. Dunitz notes that *Looking Glass* 'was published in 1872, at a time when chemical structural theory was being challenged by the recent finding that a substance present in muscle appeared to be identical with lactic acid obtained by fermentation of milk, except that solutions of the two substances rotated plane polarized light in opposite senses' (Dunitz, J. D., *Proc. Natl. Acad. Sci. USA*, 1996, **93**, 14260). The Dodo, a bird that once lived in a predator-free island, Mauritius, appears as the master-of-ceremonies in the Caucus Race, where everyone gets a prize. The Portuguese arrived in Mauritius in 1513; by 1681 the Dodo was extinct. It had no known enemies until the European colonisers appeared (*Curr. Sci*., 2003, **85**, 5). Carroll had presumably read Darwin. Even researchers in linguistics turn to Alice. In *Looking Glass* Alice introduces herself to Humpty-Dumpty who is dismissive:

' "It's a stupid name, enough!" Humpty-Dumpty interrupted impatiently. "What does it mean?"
"Must a name mean something?" Alice asked doubtfully.
"Of course it must," Humpty-Dumpty said with a short laugh: "My name means the shape I am – and a handsome shape it is too. With a name like yours, you might be any shape, almost." '

This quote opens an article which draws attention to recent studies suggesting 'that we seemingly link certain sounds with particular sensory perceptions' (Robson, D., *New Scientist*, 2011, **2821**, 30).

The *Alice* books are a source of thoughts and original ideas camouflaged in seeming absurdity. There is a great deal of sound common sense in the words of the inimitable inhabitants of wonderland. Is there advice for readers who have not encountered *Alice* in adulthood. Carroll provides sage counsel:

' The White Rabbit put on his spectacles, "Where shall I begin, please your Majesty?" he asked.
"Begin at the beginning," the King said gravely, "and go on till you come to the end: then stop." '

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Infrastructure: The Price of Inaction

Doing nothing can be a very successful strategy in tiding over difficult situations. Seasoned administrators have often perfected the art of inaction. Sometimes doing nothing can be costly. Fires must be put out and the critically ill rushed to medical care. Even while pondering on the situations in which inaction seemed to be strategically advantageous, my attention was drawn to an editorial in *Science*. The author, Kathy Caldwell, president of the American Society of Civil Engineers (ASCE) titled her column 'The Cost of Doing Nothing' (*Science*, 2011, **334**, 289). She wrote her piece on the eve of an annual gathering of civil engineers, drawing attention to the critical importance of recognizing 'that rebuilding and modernizing infrastructure will be a key driver of economic growth' in the United States, even as the economic crisis continues, largely unabated. The *Science* editorial articulated a civil engineer's call for action.

Civil engineering is an unglamorous field in the world of academia. Yet, few fields impact everyday life in the manner that civil engineering does. The origins of the discipline's name go back to the time when engineering began to influence civilian life, almost as much as it affected military activity. Traditionally, civil engineers have been associated with building activity; townships, dams, bridges, roads and railroads, airports and waterways. Housing, transportation and water, indispensable to daily life depend critically on civil engineers. Indeed the 'infrastructure' for modern life requires the foundational strengths of civil engineering. Technological advances in construction activities have led to the chaotic, unplanned and, at times, frightening growth in our cities as high rise buildings sprout with increasing frequency. Civil engineering is also concerned with hazards; buildings and bridges, dams and nuclear power plants must be insulated from the cataclysmic consequences of earthquakes, floods and even tsunamis, in areas that adjoin the oceans. Efficient transportation is central to a modern economy, with highways and railways being the key elements of connectivity. Depsite its centrality, civil engineering is not the field of choice for most students who aspire to acquire engineering degrees. Most aspirants aim to enter 'computer science' courses, despite its rather curious name which seems to set it apart from the rough and tumble of engineering. Electrical and mechanical engineering courses also seem to win over civil engineering, in the statistics that are published each year after the annual admissions. In India, engineering degrees are often only a device to enter management courses or the information technology industry, whose glamour remains undimmed. Why have I turned to civil engineering, a subject far removed from my own areas of competence or interest? The subject has intrigued me in recent years as I have attempted to grasp the sharp differences that appear to be important between various sub-areas of a vast discipline. These differences can be like fault lines, that appear at the junctions between tectonic plates; largely dormant and silent but often preventing integration of a key subject of study and research. Disciplinary tensions between academics who work in the same broad area of science or engineering are not unusual. Chemistry, an old, traditional subject has always maintained sharp distinctions between organic, inorganic and physical chemists; well drawn and rarely breached boundaries in the Periodic Table. Computational chemistry, a relatively recent field, has served to integrate the sub-disciplines to some extent, creating in the process two new species of chemists; theorists and experimentalists. This is true of physics, too. Curiously, theorists and experimentalists cohabit comfortably; the former constantly on the look out for experimental data, while the latter turn to a theoretical calculation, which always seems to propel papers into print. Biology too has been a fractured field. The classical biologists have for long been trampled over by the frenetic pace of molecular biology. The tide appears to have turned as biology seems to be returning to its roots; the deluge of sequencing and structural information has catalyzed introspection. Do we need more data? Every field of science reaches a turning point. Tensions between subdisciplines are essential to the process of advance. Having digressed, I must return to my original theme; civil engineering and its central role in maintaining our infrastructure.

The *Science* editorial drew attention to the *2009 Report Card for America's Infrastructure* produced by the ASCE. This comprehensive analysis provides grades for 15 categories of infrastructure: aviation, bridges, dams, drinking water, energy, hazardous waste, inland waterways, levees (an embankment that protects crops from flooding), public parks and recreation, rail, roads, schools, solid waste, transit and wastewater. The grading scheme would please teachers; a five-point scale that runs down from excellent (A), good (B), mediocre (C), poor (D) and failing (F). According to the ASCE report, the United States scored 11 D's and 4 C's. Particularly noteworthy

was the fact that drinking water and roads scored low D's (D–). The overall grade point average over the 15 categories was D. The estimated investment according to the ASCE for raising the infrastructure to a 'good condition' over a five-year period would be $2.2 trillion, a number that appears too large to comprehend, for those unaccustomed to global projections (and I am one). What are the parameters on which the grades have been assigned? The ASCE's advisory council considered 'capacity, condition, operations and maintenance, current and future funding, public safety and resilience'. The final grades were determined using 'publicly available data and the subjective judgements of the engineers serving on the council'. Earlier this year the ASCE produced another bleak report on the state of America's 'surface transportation infrastructure … that enable people and goods to access markets, services and inputs of production essential to America's economic vitality'. The analysis suggested that 'the continued deterioration leaves a significant and mounting burden on the US economy' (*Failure to Act: The Economic Impact of Current Investment Trends in Surface Transportation Infrastructure*, ASCE, 2011).

For Indians who have visited America or for the countless others who would like to do so, grim reports of decaying infrastructure are hard to believe. An enviable highway system, that underpins an oil guzzling economy, the absence of power cuts and water shortages is really the enduring image of America in Indian minds. The standards of infrastructure are clearly superior in America. The ASCE report underscores the concern of a community to repair, maintain and enhance the quality of infrastructure. Two issues highlighted in the *2009 Report Card* seemed particularly relevant in the Indian context. The ASCE report notes that 'as dams age and downstream development increases, the number of deficient dams has risen to more than 4000, including 1819 high hazard dams. Over the past six years, for every deficient, high hazard dam repaired, nearly two more were declared deficient'. In India the Mullaperiyar dam is now a bone of contention between neighbouring states. Technical discussion appears to be drowned in the din of political posturing. How many dams need to be evaluated? Is there an independent professional body that can make such assessments? Would concerns arise about the hundreds of bridges, road and rail, that are dotted across the length and breadth of India? Curiously, the area of drinking water secured a low grade, D–, in the ASCE report. After acknowledging that 'Americans still enjoy some of the best tap water in the world', the report notes that 'the costs of treating and delivering that water where it is needed continue to outpace the funds available to sustain the system'. According to ASCE, America's systems are aging, with leaking pipes resulting in an estimated loss of 'seven billion gallons of clean drinking water a day'. The ASCE report articulates very clearly the concerns of a professional body of civil engineers for the apparent decay of, arguably, the best infrastructure available in a large country. A column that appeared a few months ago raised another bogey: 'China vs America: Which is the Developing Country?' (Herbold, R. J., *Wall Street Journal*, July 9, 2011). The column is a paen of praise to the Chinese model of development, with recently built infrastructure in China shining in comparison to America. The author asks: 'Can you imagine the US Congress and President emerging with a unified five-year plan *that they actually achieve* (like China typically does)?' (italics mine). The Chinese 12th five-year plan began in March 2011. India's 12th Plan will, hopefully be launched in April 2012 and it will be instructive to compare investments and commitment to key infrastructure areas.

The word 'infrastructure' is an all encompassing term. The design, construction, maintenance and repair of major public utilities, from power plants to dams, bridges to water treatment plants, railways to airlines, requires a large well trained and experienced workforce. As the level of sophistication increases, the knowledge and skills of the technical personnel required increases. If a report along the lines of the ASCE's America Report Card were to be produced in India, it is likely that we would do very poorly on all counts. One additional parameter that needs to be graded in India is the quality and level of training of the available technical manpower. There appears to be a looming shortage of 'real engineers and technologists', in the most important areas relating to essential infrastructure. If nuclear power turns out to be the only means of addressing power needs, we need to ask if future manpower requirements will be met by the higher education system. Or is the solution that each sector develop an appropriate educational program the alternative? When a crisis develops experts are needed; experts whose judgements inspire public confidence. The current controversies that hinge around safety concerns at Kudankulam or Mullaperiyar appear to need the reassurances of expert groups. In many areas the shortage of 'real experts' is alarming. A report in the press highlighted the problem with an eye catching title: 'SOS: Scarcity of Specialists' (Saxena, S., *Times of India*, 25 September 2011). The report focused on areas where experts are suddenly needed in a crisis. The author argues that '…be it geologists, meteorologists, archaeologists or even vets, we just don't have the experts who matter in a crisis'. As India moves to upgrade infrastructure in a big way, the future requirements for trained manpower to build and maintain utilities need to be addressed. Civil engineers, reinvented as a new interdisciplinary breed, may be among the most needed. This is a problem to be considered and debated even in the cloistered and at times, dangerously comfortable environment of academia. In her commentary, Kathy Caldwell noted that 'although repairing and modernizing the country's infrastructure may seem daunting in lean times, the cost of doing nothing will be exponentially greater'. In India, the consequences of 'doing nothing' will be immeasurably more damaging.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science in India: New Year Reflections

The new year is a time when the optimists rejoice and even pessimists look forward to a brighter future. For scientists in India, the year begins with the opening of the Science Congress, invariably on January 3, with a speech by the Prime Minister. This year's congress is the 99th in a long series. Undoubtedly, next year's congress will be special; marking a century of gatherings. In recent years, the science congress has always been long on ceremony and short on science. Public interest is limited and even scientists show little enthusiasm for a meeting which long ago lost its relevance. The Prime Minister's speech this year held out, as always, promises for a better future for science. Investments in science, which have risen steadily over the years, are slated to grow. The projection that science's share of the GDP, which is about 0.9% at present, will double over the next five years or so must surely be good news for researchers. Unfortunately, promises and projections are sometimes hard to realise and even the best of intentions can disappear without a trace in the quicksands of officialdom. Scientists in India have long bemoaned the vice like grip of bureaucracy. In many previous congresses prime ministers have promised 'debureaucratisation'; a task that is almost impossible to accomplish. This year's speech avoided the issue; tacitly accepting the fact that it is probably easier to work within an entrenched and immutable system, rather than to dissipate energies on attempting radical reform. The promise of substantial investments in scientific research will undoubtedly please the community of researchers. It remains to be seen how quickly and nimbly agencies and institutions can move in funding and executing projects. Often the process of reform and change is subverted by the glacial pace of implementation of an approved course of action. The Science and Engineering Research Board (SERB) is a good example. Announced in 2005, the SERB has emerged in 2012; the long gestation period is testimony to the remarkably inhibitory processes that set to work, whenever the status quo appears to be disturbed.

In keeping with the times, the China bogey was raised again at the Science Congress. The popular press finds a race for more Ph Ds, more publications, more money, more institutions, indeed more of everything easy to understand. Science appears now like a game, where beating an opponent or competitor is indeed a clearly definable goal. India appears to be doing much poorer than its competitors among the rapidly developing economies, if the statistics of scientometrics are a fair measure of the health of science in India. A recent *Elsevier* report (Kolman, M., January 2012) summarises a great deal of data, captured and analysed imaginatively (I shy away from the term 'innovatively'). Using the growing *Scopus* database and an expanding set of online tools, the publishing giant seeks to inform decision makers. The new mission statement bears repetition: 'Provide information and workflow solutions that help institutional decision makers and researchers create significant value by building insights, enabling advancement and *improving research-driven returns on investment*'. This move to provide a data driven base for science policy will undoubtedly please economists and planners. Old fashioned observers of science, and I count myself amongst them, will be left mildly disturbed to see glimpses of a future where decision makers, with little feel for the uncertainties of science and its intrinsic value, pass judgements based on modern measures of science.

The *Elsevier* report merits careful reading; there is a great deal of useful and interesting information. The statistics (2010 data) that have attracted the press are of course the China–India comparisons. In terms of total published articles China (969,315) lies in second position behind the United States (2,095,934), while India (233,027) lies in tenth place. In terms of growth rate of publications (a sometimes risky parameter, which may be influenced by the growth rate in journal coverage by databases), China (22.8%) outstrips India (14.2%). Both countries show a much higher growth rate in published papers than the developed world (US 2.1%; UK 3.54%, Germany 4.71%; Japan 1.58%); a feature that is easily anticipated. There is one parameter in which India appears to edge out China. This is a number that quantitative analysts fondly believe is a reflection of 'quality', viz. citations per article (CPA). India scores 2.71, while China is close behind at 2.21, leading *The Hindu* to proclaim, 'India ahead of China in quality of scientific papers' (January 6, 2012). Using this parameter, small countries like Switzerland (7.6), The Netherlands (6.98) and Sweden (6.57) come out on top, with the United States (6.45) not very far behind. *The Hindu*'s sub-editors may have been optimistic with their headline. It would be dangerous, for example, to use overall statistics to compare the performance of Indian batting line-ups with that of their opponents on recent overseas tours to England and Australia. There is

another curious parameter that reflects the publication dates of papers cited in the reference lists of articles published from different countries. India does surprisingly poorly on this count, with only Russia doing worse. Clearly, this finding merits a closer look and may be an indicator of the nature of subjects in which papers are written. The *Elsevier* report provides an interesting view of international collaboration, a subject that merits careful strategic consideration in India. It may be critical to forge purposeful research linkages at institutional level. Little benefit may be forthcoming from the many visits of government delegations abroad, if ground level working relationships are not established by institutions. Financial resources are limited for pursuing meaningful collaboration; when they are available the impregnable wall of bureaucracy can prove inhibitory. Collaborations, both within the country and abroad, appear to be important in enhancing the quality and quantity of scientific output. Are papers and citations the most important measure of the health of the research enterprise or are we measuring what is easy to measure? Are the fields where papers accumulate quickly and journals are characterized by higher impact factors necessarily more important for investment? Will increasing the number of Ph Ds produced, by lowering and relaxing the norms of academic judgement, contribute positively to the growth of science in India? These and many other questions arise whenever a barrage of data is thrown into the arena of science policy. The *Elsevier* report begins with investment statistics that are revealing. 'India spends 1% of its GDP on research compared to 3.4% in the US and Japan.' Even more provocative for the academic science community is the fact that 'only 4% of the total R&D expenditure in India is from higher educational institutions, putting India lowest among its global peers – China (10%), Japan (14%), the US (17%) and Canada (35%)'. This statistic reflects the priorities in India where there is a great emphasis on R&D spending in the strategic sectors of atomic energy, space and defence. What is less appreciated is that areas like energy, environment and infrastructure come within the ambit of large ministries with substantial budgets, but with little or no commitment to research.

With the 12th Five Year Plan just around the corner, one might ask whether academia (and I include here the many national laboratories that do academic research, albeit in an environment distinct from an academic institution) in India can in fact digest and effectively utilise a sudden infusion of funds? Do these institutions have the administrative and financial mechanisms to benefit from the increases in budgets? Do they have the internal academic processes that permit hard judgements on areas where the money is best invested? Is the general health of the Indian academic system good enough to respond to a sudden stimulus for change? The great universities of the West, driven by an emphasis on both research and education, have been models over the past fifty years for institutions the world over. The recent fashion of world ranking of institutions always finds the dominant universities of the West at the very top. While nostalgia might make us long for Nalanda, now an almost mythical institution, reality suggests that we might be more pragmatic and learn from more recent experiences elsewhere in the world. An observer of the Indian scene provides the view of a well-intentioned outsider, in this issue (p. 9). Richard Zare an eminent physical chemist, an ardent advocate of science education and a frequent visitor to India writes about his impressions after attending two major conferences in India in December, celebrating the International Year of Chemistry. His perceptive comment that 'there may be too much emphasis on the amount of publications as opposed to the quality' will undoubtedly be echoed by many Indian scientists. Zare outlines three criteria which young faculty need to meet in his department at Stanford in order to secure tenure. Firstly, they must be good departmental citizens. Secondly, they must be good teachers. While great teachers are most desirable, Zare notes that everyone who wants to achieve the status of a good teacher can do so. Finally, of course, faculty must be great researchers. Judgements about research are difficult to make but Zare is clear that quantitative metrics and local impressions are insufficient as a reliable guide.

The Indian academic system differs dramatically from the American model in that, with very, very few exceptions, tenure is not an issue. Faculty are invariably recruited to 'permanent positions'. Even in cases where the initial appointment is a contract of fixed term, renewals are practically automatic. Academic performance is not demanded and academic freedom often degenerates into a licence to legitimise non-performance. While the best of institutions hire faculty with some care, the majority of institutions operate under severe constraints while recruiting new faculty. There is also a vast difference between the state and central institutions, with the former being subject to considerable political pressure. Despite the many constraints and inhibitory influences, research output from India is indeed increasing both in quantity and average quality. While both the carrot and stick are used equally effectively in the West to enhance academic performance, neither is available to most institutions in India. Administrations must follow the policy of benign neglect with respect to high performers, even while turning a blind eye to the significant dead wood accumulating in our institutions. Attempts to provide incentives by the many national award schemes are unlikely to have any impact in an environment where institutions have a limited judgemental role in assessing faculty performance. One can only hope that the new year and the near future will add more good citizens, good teachers and great researchers to our institutions.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Virtual World: Problems Without Solutions

Heads of academic institutions are the beneficiaries of a great deal of advice. At times, advice pours in from all quarters especially on matters on which everyone seems to have opinions and solutions. In recent months, I have had the good fortune to have received detailed suggestions on subjects as diverse as strategies to raise my institution to 'world class' status to plans for decongesting campus roads. As an editor, I have always received many interesting suggestions for improving the journal's impact factor. As the, often silent, recipient of a great deal of well meaning advice, I have invariably preferred those who advise to those who complain. Over time, I have learnt to reflect on advice, including a great deal which, curiously enough, is anonymous. Advice (and complaints) has become easier to dispense, with e-mail having become both ubiquitous and, at times, annoyingly efficient. A few days ago I received intriguing advice from a student. A characteristic of youth, one that is undoubtedly engaging, is the unshakeable conviction that every problem has a satisfactory solution. The student, apparently one with a driving interest in institutional quality, suggested that 'access to *Facebook* and *Twitter* should be barred on the campus network'. He argued that this course of action would result in improving the quality of the research output of the institution. Having struggled with the digital revolution for several years, I have only read about *Facebook* and *Twitter*, both of which seem to be gathering strength with each passing day. Celebrities and revolutionaries seem to have converged in equal measure on these sites. The student's mail left me slightly puzzled.

Social networking, both virtual and real, is something I have only observed from a distance. What could be the connection between internet access to these sites and research output? My first guess was that my advisor was implying that several students (and possibly, faculty) spent excessive time on social networking sites rather than on productive academic pursuits. Was there any other connection? I turned for help, inevitably to a student, who suggested that the institutional network connectivity was becoming slower and slower as larger and larger segments of the community became addicted to *Facebook* and *Twitter*. Coincidentally, even as I write this the *Times of India* (18 January 2012) reports that IIT Madras has blocked access to *Facebook* and *Twitter*, as the local information highway appears to have become completely clogged with traffic to those sites. I was left wondering how students had spent their time a few years ago, in the pre-historic era when the Internet, *Google*, *Facebook* and *Twitter* had not made major inroads into public consciousness.

Modern information technologies have been truly transformative on the way in which academic communities function. Scientists have always communicated by writing papers. Journals have been central to the enterprise of science, since the appearance of the *Philosophical Transactions of the Royal Society* in 1665. Little changed in the manner in which manuscripts were prepared or journals produced for nearly three centuries. Dramatic changes occurred only in the last decade of the 20th century. In the 1970s, when I first began to read and write scientific papers, the first version of manuscripts was invariably written in long hand. Transformation into a typescript required the authors to write legibly or employ typists, who had to correctly decipher scribbles, through long years of experience. The draft manuscript was then typed on a 'stencil sheet' using a manual typewriter. Copies were then rolled off on a cyclostyling machine. Figures were drawn by a draughtsman using tracing sheets and 'India ink'. Copies were produced by the process of 'ammonia printing'. Blue copies were carefully trimmed to size. Manuscripts were sent by airmail to journals, which insisted on multiple copies driving up postal expenses. The postman was a popular and eagerly awaited individual. Acceptance and rejection letters from journals (often more of the latter) were always delivered with unfailing regularity by the local post office. The new technologies began to intrude into India in the late 1970s; some inventions had a very short life. The electric typewriter made a brief entry only to be swept away by computers and printers. The photocopier, always known as the 'Xerox machine', became a fixture in institutions. Manual typewriters, cyclostyling machines and ammonia printing devices disappeared. Draughtsmen, gestetner operators and typists approached extinction. Word processors appeared briefly. By the late 1980s PCs were on the horizon. Changes were fast and furious in the 1990s. Manuscripts could be produced in their entirety by authors, with little pretention to typing or drawing skills. The *Microsoft* world was becoming commonplace. Electronic

submission of manuscripts became the norm in the new century as the speed and power of the internet grew dramatically.

In less than 25 years the process of science publishing has transformed in a manner that could hardly have been anticipated in the 1970s. The traditional printed journal is approaching extinction, even as libraries seek to reinvent themselves. The manner in which scientific lectures are given has changed spectacularly. PowerPoint ensures that slides have a uniformly attractive quality, that often compensates for deficiencies of presentation. In reflecting on the transformative role of technology in communicating science, I could not but help concluding that fearsomely efficient worldwide communication has been a mixed blessing. E-mails demand instant responses. Left unattended, they are drowned by the perennial stream of messages that pour in. Texting on phones is now so common that addicts have forgotten how to spell. *Twitter* appears to encourage brevity (and at times, inanity) to the point where a return to normal(?) communication becomes difficult. The rise of the internet has made free expression an art form, with blogs and comments complementing the power of social networking sites in shaping opinion. Undiscerning browsers on the internet can sometimes find it hard to distinguish between fact and fiction. Governments, both democratic and otherwise, can scarcely conceal their desire to police the internet.

Prompted to think about the new and ever expanding virtual world by a student's e-mail, I was pleasantly surprised to receive a book that provides a historical account of 'the ideas that shaped a century and a company'. The company, of course, is IBM (International Business Machines). Celebrating a century of existence, IBM was for a long time synonymous with mainframe computers. *Making the World Better* is authored by three journalists who examine distinct aspects of 'how the world has changed', while exploring 'IBMs role in that change' (Maney, K., Hamm, S. and O'Brien, J. M., IBM Press, 2011). The three sections, authored independently, provide an engaging view of the growth of the technology and businesses that dominate the world today. IBM entered India in the 1960s. Multinationals were then viewed with deep suspicion; computerization was projected as a device that would result in a loss of jobs. The railways in India were the first to realize the enormous enhancement in operational efficiency by embracing computers. Fifty years later, the computerization of administration in all public departments is slowly becoming a reality. To a great extent, it is the internet with its enormous entertainment value which has served to make computers acceptable in all offices. Kevin Maney's overview of the growth of computers is compelling. Machines that deal with information must be able to 'sense the world'. Humans do it all the time using 'sight, hearing, touch, smell and taste'. From the punched cards of the 1960s to the touch technology on the most recent devices, progress has been rapid. Computers may soon rival their creators in being able to 'sense the world' in everyway. Storage is, of course, the key element in processing information. The brain does it well, but can be fickle. How often have we been unable to recall a fact which we surely stored away? Computer memories are becoming so vast and efficient that Maney suggests 'we are now approaching total recall'. As storage devices become smaller and smaller, we encounter a new problem – the need to deal with a deluge of data. Maney has an interesting view: 'The human brain has a mechanism for dealing with data overload. It forgets. If indeed we're on a path to building machines that think like us, how ironic if the next great invention in computer memory turns out to be forgetting.' Processing, computing faster and faster, defines the field in a way no other parameter does. Maney describes the road from individual transistors to chips, densely packed with transistors. Moore's law appeared in 1965, which predicted 'that the density of transistors would double roughly every 18 months'. Remarkably the law has held up for nearly half a century. Hardware obsolescence may be traced to the success of Moore's law. Computers have moved to mimic many attributes of the human brain and Maney chronicles the transition from chess playing 'Deep Blue' to the quiz champion 'Watson'. While the former triumphed in a brutal, machine-like manner, the latter defeated human opponents in a more subtle fashion; 'Watson learns from its mistakes.' Towards the end of his overview, Maney turns to networking. He notes that 'when people, computers and devices interconnect, computing's impact is multiplied exponentially'. The internet is now entrenched; cloud computing is a term increasingly heard; hand held devices can operate in the most unlikely places. Computers and communication have merged seamlessly.

Having digressed, I must return to the issue with which I began. Is it sensible to regulate internet usage, especially social networking sites as my advisor suggested? I suspect it would be as effective as 'prohibition' was in curbing alcohol consumption, with cleverer and cleverer ways of circumventing restrictions being invented. Bans often spur creativity.

The internet has evolved as a formless entity that recognizes no political or geographic boundary. Exercising control over the internet may, ironically, be 'a problem with no technical solution', a phrase coined by Jerome Wiesner and famously resurrected by Garret Hardin in his now classic analysis of 'the tragedy of the commons'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Virus Research: Fuelling New Fears

Oswald Avery's identification of DNA, in the 1940s, as the 'pneumococcal transforming principle' may be justly regarded as the first step in establishing the chemical nature of genes. Genetics, midwifed into existence by Mendel in the 19th century, suddenly seemed connected to chemistry. Almost exactly half a century has passed since the Nobel prize recognized the unprecedented insights into the chemistry of heredity, provided by the Watson–Crick double helical structure of DNA. DNA's monotonous chemistry revealed a robust and efficient mechanism for storage and transmission of information over generations. Molecular biology's advance, in the years that followed the postulation of the DNA structure, has been blinding, at times. Even twenty five years ago few would have imagined that the entire sequence of the human genome would be available by the turn of the century. The methodologies of sequencing and synthesising DNA have reached a level of sophistication and automation that these operations are best done by companies for a price. Cloning is a term that is now commonly used. A new discipline of 'synthetic biology' is rearing its head. Techniques imported from physics and chemistry, available on superbly engineered instrumental platforms, have provided a near atomic level view of molecules and their interactions, which define cellular chemistry. The cell, with its thousands of chemical constituents, is now the unit of discussion in biology. Chemicals and chemistry allow cells, and indeed organisms, to communicate and interact. Biology's explosive growth has held out great promise for revolutions in areas central to the human condition – health and agriculture. Biology's transformation over the last half a century has propelled the discipline to the forefront in public perception of science; rivalled only by the attention bestowed on information and communication technologies, which have also exploded over the past few decades. It is fashionable today to envision a brave new world developing at the confluence of biotechnology, information technology and the emerging field of nanotechnology.

The 'gene revolution' in biology quickly brought to the fore the possibility of 'genetic engineering'. Many questions, once the domain of imaginative authors of science fiction, could be legitimately asked. Can new organisms, with new characteristics, be created? Can long extinct organisms be resurrected? Michael Crichton's *Jurassic Park* captured the imagination of dinosaur fans (and there are undoubtedly many). Purists were quick to pounce on the scientific inaccuracies that render Crichton's scenario implausible. In the early days of recombinant DNA technology and genetic engineering, fears about the new advances in biology were widespread, even amongst molecular biologists. The 1975 Asilomar meeting, initiated and organized by leading researchers in the emerging field of recombinant DNA technology, was critical in setting standards and allaying fears, 'allowing geneticists to push research to its limits without endangering public health'. Paul Berg, the 1980 Nobel laureate in chemistry, describes the background to the 1975 meeting in California and its outcome which opened up the new field of genetic engineering (*Nature*, 2008, **455**, 290). Berg notes that 'the conference marked the beginning of an exceptional era for science and the public discussion of science policy'.

The experiences of the Asilomar conference will undoubtedly be recalled repeatedly in the aftermath of a recent move to remove key experimental details from two papers on the H5N1 avian influenza virus, which are currently being considered for publication in *Science* and *Nature*. The papers establish that viruses that possess a specific protein, a hemagglutinin, from the H5N1 influenza virus can become transmissible in an animal model. These studies conducted using ferrets may help further understanding of how viruses found in nature become capable of transmission amongst humans. The authors based in The Netherlands and the United States have taken the unprecedented step of 'a voluntary pause of 60 days on any research involving highly pathogenic avian influenza H5N1 viruses, leading to the generation of new viruses that are more transmissible in mammals'. The self-imposed moratorium has been advanced to provide time for discussions on the issue as 'organizations and governments around the world need time to find the best solution for opportunities and challenges that stem from the work' (Fouchier, R. A. M., García-Sastre, A. and Kawaoka, Y., *Nature*, 2012, **481**, 443).

The move to review and possibly 'censor' scientific papers as a result of biosecurity concerns is unprecedented. The National Science Advisory Board for Biosecurity (NSABB), apparently a body with no regulatory authority, suggested to *Science* that key details necessary for reproducing the results be omitted. The journals are apparently agreeable 'if the government delivered a "transparent plan" for sharing the details' (Enserink, M. and Malakoff, D., *Science*, 2012, **335**, 22). Thus far, all discussions have involved the US government and its

agencies. A feature in *Nature* (January 19, 2012) provides views of ten experts, not all of whom agree on the issue. One of the authors of the controversial papers, Ron Fouchier, asks 'whether it is appropriate to have one country dominate a discussion that has an impact on scientists and public health officials worldwide'. He notes that 'there is no global equivalent of NSABB' and concludes that 'an issue this big should not be decided by one country, but by all of us' (Fouchier, R. and Osterhaus, A., *Nature*, 2012, **481**, 257). The major fear about the H5N1 papers appears to be that publication of detailed protocols for engineering a more transmissible influenza virus would provide a new avenue for bioterrorists. While procedures for producing incendiaries, explosives and even nuclear devices are accessible, the prospect of biological weapons in the wrong hands appears to engender greater fear. This is undoubtedly due to the transmissibility of pathogens in human populations, a process that occurs silently and insidiously. Are there other concerns? It would appear that an even more immediate worry is the danger of laboratory infections. A brief and sharply worded point of view suggests the probabilities of infections accidentally acquired in the laboratory are significant and that the probabilities of chance escape will increase as the number of investigating labs increases. Indeed, 'regulators should not be sitting idly by, while the threat of a man-made pandemic looms' (Klotz, L. and Sylvester, E., *Nature*, 2012, **481**, 258). The importance of the new papers on H5N1 for understanding how viruses move from one host to another is evident to most commentators in the field.

For specialists in the area of influenza virus research, the H5N1 episode must evoke memories of an earlier discussion when the virus that caused the 1918 Spanish influenza pandemic was resurrected (Tumpey, T. M. *et al.*, *Science*, 2005, **310**, 77; Kaiser, J., *ibid*, 2005, **310**, 28). The viral genome was reconstructed based on complete coding sequences of viral RNA determined from genomic RNA 'recovered from frozen, unfixed lung tissues from an Alaskan influenza victim who was buried in permafrost in November 1918'. The 1918 virus was extraordinarily virulent and has been held responsible for 'up to 50 million deaths worldwide, including an estimated 675,000 deaths in the United States'. By contrast, contemporary H1N1 viruses are relatively benign. Resurrecting the 1918 virus allowed Tumpey *et al.* to identify the mechanisms underlying the high virulence. Most importantly, the resurrected virus strains could be killed by currently available antiviral drugs, oseltamivir (*Tamiflu*) and amantadine (*Symmetrel*) and were sensitive to the seasonal flu vaccine. One of the authors of the 1918 Spanish Flu virus paper in a strongly worded comment on the H5N1 controversy argues that 'life-saving science' should not be censored. In recounting the story of the resurrection of the 1918 virus he notes: 'Had we not reconstructed the virus and shared our results with the community, we would still be in fear that a nefarious scientist would recreate the Spanish flu and release it on an unprotected world. We now know such a worst-case scenario is no longer possible'. Curiously, the 2005 paper on the Spanish flu virus was also considered by NSABB and eventually published in full. In considering what is euphemistically called 'dual use research', the perspectives of researchers immersed in biology can differ sharply from those interested solely in biosecurity. Critics of the H5N1 research have raised grim possibilities: 'It is a virus that is capable of killing half its victims, a proportion greater than that for any endemic disease. Were that coupled with the transmissibility of a pandemic flu virus, it would have characteristics of an ultimate biological weapon unknown even in science fiction. We should not publish a blueprint for constructing such an organism' (Henderson, D. A., *Nature*, 2012, **481**, 258).

In many ways, the discussion that has begun is reminiscent of the mood that preceded the Asilomar conference in 1975. Writing over three decades later, Paul Berg asks: 'Apart from laying the foundations for an effective safety regime, what else did Asilomar achieve?' His answer: 'I feel that scientists were able to gain the public's trust – something that is now much more difficult for researchers working in biotechnology'. He concludes: '…there is a lesson in Asilomar for all of science: the best way to respond to concerns created by emerging knowledge or early stage technologies is for scientists from publicly funded institutions to find common cause with the wider public about the best way to regulate – as early as possible. Once scientists from corporations begin to dominate the research enterprise, it will simply be too late' (*Nature*, 2008, **455**, 290).

In the mid-1970s terrorism was not yet the major concern that it is today. However, under the impregnable cover of 'national security' a great deal of research that would have raised public alarm was indeed carried out. Berg noted the difficulties of regulation once corporate interests begun to dominate. This is also true of research that has for decades been protected by the security umbrella of advanced countries. Although biology's advances sometimes raise concerns, most often the new technologies provide fascinating insights into our past, DNA extracts from bones and teeth recovered from cemeteries near London where victims of the 'Black Death' of 1347–51 were interred, have yielded information to reconstruct the genome of the bacterium *Yersinia pestis* responsible for the medieval plague (Schuenemann, V. J. *et al.*, *Proc. Natl. Acad. Sci. USA*, 2011, **108**, E746). Characterising ancient pathogens will undoubtedly provide insights into the evolution and epidemiology of human infectious disease. Will the bacterium itself be resurrected? Time alone will tell. But a commentary in *Nature* (October 27, 2011, p. 444) notes that while 'this may sound alarming … such a strain could be easily treated with modern antibiotics'. There is a great deal of history embedded in DNA waiting to be read. Avery, one of modern science's unsung heroes, would be surprised at the turn of events.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Human Experimentation: Historical Lessons

Medical research directed towards the development of new therapeutics and vaccines is a long drawn out complex affair, in which the chances of success are relatively slim. The regulatory process, that seeks to ensure the safety of vaccines and drugs intended for human use, involves different phases of clinical trials, which require the participation of volunteers. 'Informed consent' is a stated necessity, implying that all participants in a clinical trial are fully aware of the risks that they may be exposed to in the course of the study. Major pharmaceutical companies are increasingly fanning out to developing or underdeveloped countries to conduct clinical trials, most often drawing participants from poverty stricken populations, with limited understanding of the risks that may be involved. In attempting to secure the hundreds of millions of dollars invested in research on new therapeutics or vaccines, there is a strong desire to push ahead with clinical trials. The mushrooming of 'clinical research organizations' in India, sometimes a euphemism for entities that undertake and facilitate clinical trials on a contractual basis, is a clear indication that major multinational pharmaceutical companies are indeed eyeing India's large population as a fertile prospect for testing new products. Several recent controversies raise concerns about the manner in which trials have been planned and conducted. While reports in the press highlight and sometimes sensationalize ethical lapses, there is a clear sense of unease in the many commentaries on clinical trials that have appeared over the last year. Medical research involving human subjects has always been an ethical minefield. Indeed, much of the internationally accepted guidelines emerged after the Nuremberg trials, which convicted as war criminals German doctors who carried out the most brutal experiments on prisoners in the name of medical research. The Nuremberg Code (1947) provides today a starting point for considering experiments involving humans.

Discussions of ethics in human experimentation need to be informed by the lessons of history. A recent editorial in *Nature* entitled 'Hypocritical oaths' comes to the conclusion that 'history judges some research as unethical, despite approval at the time' (*Nature*, 2012, **482**, 132). Indeed, hindsight can be a powerful weapon of judgement on most human matters. The *Nature* editorial quotes William Osler's prescription that appeared over a century ago: 'For man absolute safety and full consent are the conditions which make such tests allowable.' The provocation for *Nature*'s editorial concern is a news feature on human experiments carried out by US doctors in Guatemala beginning in the 1940s. The article attracts immediate attention with the headline, 'First, Do Harm' (Walter, M., *Nature*, 2012, **482**, 148). The Guatemala venereal disease experiments were exposed only in 2010, when Susan Reverby, a historian, published an account of a horrific series of studies carried out with human subjects, with the 'full knowledge of US health officials' in the late 1940s (*J. Policy History*, 2011, **23**, 6–28). Her discovery of the Guatemala papers in 2009 was accidental. Reverby had just completed her book *Examining Tuskegee: The Infamous Syphilis Study and its Legacy* (University of North Carolina Press, Chapel Hill, 2009). One of the researchers in the Tuskegee study John Cutler had left his papers at the University of Pittsburgh. In searching for additional material on Tuskegee, Reverby chanced upon details of Cutler's experiments in Guatemala. The Guatemala studies began in 1946 when penicillin had become available as an effective treatment for syphilis. Yet in these experiments uninformed subjects were deliberately infected and treatment withheld (Semeniuk, I., *Nature*, 2010, **467**, 2010). The extraordinary cruelty of the experiments and the decades long suffering of the subjects is described in the news feature in the 9 February 2012 issue of *Nature*. The author provides a historical rationale for what appears in retrospect, as an abhorrent study: 'In the early decades of the twentieth century, US health officials were consumed by the battle against sexually transmitted diseases, much as subsequent generations of researchers have fought cancer and HIV.' In 1943 an estimate was made that the US army would be challenged by '350,000 new infections of gonorrhea annually'. This in turn was termed as 'the equivalent of putting out of action for a full year the entire strength of two full armored divisions or of ten aircraft carriers'. The end of the war in 1945 and the availability of penicillin by then suggest that the Guatemala experiments were ill-conceived and appear to have been devised and approved by researchers and administrators 'who cut their teeth with venereal diseases and were interested in them'. The studies conducted by Cutler in Guatemala had the support of local public health officials and the political leadership. Indeed over six decades later local governments in poor countries often facilitate 'clinical trials' carried out with little oversight by pharmaceutical companies and international agencies. In the aftermath of Reverby's account of Cutler's experiments, a US bioethics commission

produced the grim statistics, over sixty years later. 'Cutler's team exposed 558 soldiers, 486 patients at the psychiatric hospital, 219 prisoners, 6 prostitutes and 39 other people to gonorrhea, syphilis or chancroid. But the commission was unable to determine how many people actually developed infections or how many of the participants were treated. Researchers also measured the accuracy of diagnostic tests in experiments that involved orphans and people with leprosy, as well as people from the psychiatric hospital, prison and the army.' Some descriptions of Cutler's experiments, described in the *Nature* news feature are reminiscent of the bizarre and brutal details that emerged at Nuremberg. Even in retrospect it appears difficult to provide a historical rationale for the Guatemala study.

The lead researcher in this bizarre episode, John Cutler, began his career in another controversial project initiated by the US Public Service in 1942. Driven by the desire of the military to develop prophylactic methods against sexually transmitted diseases, Cutler participated in an experimental program using 'volunteers' at a prison in Terre Haute, Indiana. The *Nature* news feature notes that this short-lived study which lasted only ten months 'established several practices that Cutler would go on to use in Guatemala'. The experiments in Guatemala between 1945 and 1948 have been termed 'not only an unconscionable violation of ethics' but 'also poorly conceived and executed'. Curiously, failure and poor practice did little to retard the progression of Cutler's career. Interestingly 'the World Health Organization sent Cutler to India to lead a team demonstrating how to diagnose and treat venereal disease'. A decade or more after his Guatemala experiments, Cutler arrived in Alabama, then a racist bastion, to become 'a lead researcher in the infamous Tuskegee experiment….in which hundreds of black men with syphilis were studied for decades without receiving treatment'. The *Nature* feature notes that 'he flourished in the Public Health Service and later became professor of international health at the University of Pittsburgh'. The Guatemala papers were unearthed several years after his death in 2003. There will undoubtedly be many analyses in future which will attempt to understand the man and his actions in the context of the times he lived in and the environments in which he worked.

Tuskegee, where Cutler found another location for his experiments in the 1960s, is now synonymous with unacceptable protocols in human experimentation. The website of the Centers for Disease Control and Prevention provides a timeline for the 'Tuskegee Study of Untreated Syphilis in the Negro Male'. The experiment, which began in 1932, involved as many as '600 black men – 399 with syphilis, 201 who did not have the disease'. The subjects did not know that they were part of an experiment designed to 'record the natural history of syphilis'. The ostensible purpose was to study disease progression in untreated patients. The study ran for forty long years until a news report in 1972 led to termination of the experiment. Although penicillin became available and was the preferred treatment by the early 1950s, 'the men did not receive therapy'. A scholarly account of the Tuskegee experiment entitled 'Racism and research: The case of the Tuskegee syphilis study' appeared in 1978 (Brandt, A. M., *The Hastings Center*, December 1978, pp. 21–29). The investigations that followed the public disclosure of the details of the experiment did not, in Brandt's words, address 'the most basic questions of *how* the study was undertaken in the first place and *why* it continued for forty years'. Brandt goes on to suggest that the origins of the Tuskegee experiments were rooted in the then prevailing medical opinion that viewed black Americans as 'essentially primitive peoples', in the early years of the 20th century. 'By the turn of the century Darwinism had provided a new rationale for American racism…. Social Darwinists analyzed census data to predict the virtual extinction of the Negro in the twentieth century.' A major assumption that provides a 'backdrop for examining the Tuskegee Syphilis Study' is that doctors 'generally discounted socioeconomic explanations of the state of black health, arguing that better medical care could not alter the evolutionary scheme'. Brandt cites a great deal of correspondence in the run up to the Tuskegee study, which provides a disturbing insight into the attitudes of the men who planned the study. Why did the Tuskegee study continue for so long?; throughout the decade of the 1960s even after the many discussions of ethics in research that followed the Nuremberg trials. As late as 1969 physicians at the Center for Disease Control met, deliberated and endorsed continuation of an experiment that denied treatment to subjects infected with completely susceptible organisms. It is quite likely that the study may have continued for the lifetime of the subjects, if a press report had not drawn attention to a programme designed in a manner clearly violative of all accepted canons of medical ethics.

Brandt's conclusion is disturbing: 'In retrospect the Tuskegee study revealed more about the pathology of racism than it did about the pathology of syphilis; more about the nature of scientific inquiry than the nature of the disease process.' He goes on to add that 'the notion that science is a value free discipline must be rejected'. A final note of caution, that is particularly relevant even today, argues that 'the need for greater vigilance in assessing the specific ways in which social values and attitudes affect professional behaviour is clearly indicated'. The Guatemala and Tuskegee studies were clearly unethical, even by the standards of the times. In the former, subjects were brutally infected and, at times, left untreated. In the latter, proven treatments were deliberately withheld, for a scientific purpose which must have appeared completely unacceptable even at the time the study was conceived. Human arrogance prevailed in both cases. Presidential apologies eventually appeared; Clinton in 1997 for Tuskegee and Obama in 2010 for Guatemala. In today's context the urge to breach ethical boundaries can be strongly fuelled by commercial interest. Learning from history may be prudent.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Communicating Science: Candles, Flames and Faraday

American university campuses in the late 1960s and early 1970s were hugely influenced by the war in Vietnam; a conflict that seemed to be perceived as a monumental misadventure. The war appeared to immediately impact campuses as young men (often little more than boys) were likely to be drafted into military service, unless deferments were obtained. As a Ph D student on an American campus at that time, the concerns and fears of my contemporaries left a lasting impression. This was also a time when technologies were evolving rapidly. The *Apollo* moon missions were events that merited near continuous coverage on television, with viewership peaking during the near disastrous *Apollo 13* mission in 1970, which provided cliff hanging suspense for several days. Colour television had arrived in America; a medium that students from India had never encountered before. In 1972, a new TV serial appeared based on happenings in an army medical unit serving on the frontlines in the Korean war; a setting that seemed particularly appropriate in a country consumed by another far-off Asian conflict. *M.A.S.H*. was a serial that was often funny, although the dark humour seemed to be a defense mechanism that enabled the characters to maintain their sanity, in a maddening environment. *M.A.S.H*. turned out to be a long running serial, and as I just discovered, using the long arm of *Google*, one that ran for over a decade. Its lead character, Hawkeye Pierce, played by Alan Alda was appealing in many ways, especially in an environment where anti-establishment sentiment was running very high. He was cynical, disrespectful of authority, casual but surprisingly competent, resourceful with a wicked sense of humour. My memory, now partly fading, of a forty-year-old TV serial was jogged when I found Alan Alda's photograph on the Editorial page of the 2 March 2012 issue of *Science*. What indeed was a TV actor of yesteryear doing on the pages of a science journal? Alda was promoting the increasingly important area of communicating science to a general audience. He begins his essay 'The flame challenge', by recalling a childhood question posed to his teacher: ' "What's a flame?" I asked her. "What's going on in there?" There was a slight pause and she said, "It's oxidation". She didn't seem to think there was much else to say. Deflated, I knew there had to be more to the mystery of a flame than just giving the mystery another name." ' (*Science*, 2012, **335**, 1019.)

Alda's question was asked and answered in dramatic fashion just over a century and a half ago by Michael Faraday. The six lectures he delivered at the Royal Institution in London, during the period December 1860 and January 1861, on 'The chemical history of a candle', later published as a book, are now a part of the folklore of science. A sesquicentenary edition has just appeared, edited by Frank James (Oxford University Press, 2011), reminding us that masterpieces endure the ravages of time. A recent review of the book notes that James points out that the continuing relevance of Faraday's lectures stems from the fact that 'they remain almost entirely scientifically valid (The only shaky sections are those that touch on the sources of light emission; these would have benefited from a quantum mechanical understanding that was still decades in the future)'. The reviewer draws attention to James' suggestion that Faraday's religious beliefs 'influenced his deep appreciation of the natural world'. He goes on to add: 'There's some irony in the fact that Faraday's most famous studies – on electromagnetism – laid the groundwork for the mass electrification that ultimately supplanted candles as practical sources of light. It seems likely he would have appreciated their sustained current role in spiritual settings, as people continue to contemplate the strange beauty of their flickering flames.' (Yeston, J., *Science*, 2011, **334**, 1644.)

The classic account of Faraday's discourse on the candle was transcribed and edited by William Crookes and is available in India as a reprint produced by Vigyan Prasar in 1995. In a letter to *Nature* (1997, **388**, 710), J. M. Thomas pointed out that 'Japanese translations of…Faraday's Christmas Lectures (1859–60) have gone through more than 70 editions and are required reading for Japanese schoolchildren'. Faraday addresses his lectures to the young: 'I claim the privilege of speaking to juveniles as a juvenile myself'. Like the young Alan Alda nearly a century later Faraday is fascinated by the flame, 'where you have such beauty and brightness as nothing but combustion or flame can produce. You have the glittering beauty of gold and silver, and the still higher lustre of jewels like ruby and diamond; but none of these rival the brilliance and beauty of flame. What diamond can shine like a flame? It owes its lustre at night-time to the very flame shining upon it. The flame shines in darkness, but the light which the diamond has is as nothing until the flame shines upon it, when it is brilliant again. The candle alone shines by itself and for itself…'. Faraday's use of a candle to illustrate a wide range of natural phenomena remains a timeless classic in communicating sci-

ence. Towards the end he draws the parallel between combustion and respiration, both of which utilize oxygen and produce carbon dioxide ('what a wonderful change of carbon must take place under these circumstances'). Faraday urges his young audience to consider the consequences of respiration: 'A man in twentyfour hours converts as much as seven ounces of carbon into carbonic acid; a milch cow will convert seventy ounces and a horse seventynine ounces solely by act of respiration… And what an extraordinary notion this gives us of the alterations going on in our atmosphere. As much as 5,000,000 pounds or 548 tonnes of carbonic acid is formed by respiration in London alone in twenty four hours. And where does all this go? Up into the air.' Faraday goes on to discuss the absorption of carbon dioxide by plants. Remarkably, Faraday wonders about the effects on the atmosphere, anticipating the many contemporary discussions of the effects of burning fossil fuel. He concludes with a wish for his young audience: '…a wish that you may, in your generation, be fit to compare to a candle; that you may like it, shine as lights to those about you; that, in all your actions, you may justify the beauty of the taper by making your deeds honourable and effectual in the discharge of your duty to your fellow-men.'

Faraday used the candle and its beautiful, flickering flame to illustrate the chemical nature of combustion, the carbon cycle, the two parts of water – hydrogen and oxygen – and the profound connections between respiration and photosynthesis. Physics and biology hovered in the background when Faraday engaged his audience on a discussion of the chemical history of a candle. Over a century later in a review titled 'From candles to chemistry', of a book by P. W. Atkins (*Atoms, Electrons and Charge*, W.H. Freeman, 1991), J. P. Simons describes 'the task that Faraday set himself'. Even with the insights of the 20th century the task is formidable: '…to see the complexity (and underlying simplicity) of chemical change through the light of a candle's flame' (*Nature*, 1991, **353**, 805). In Faraday's time atoms and molecules were ideas just beginning to form. Chemical thermodynamics was still over a decade away with Willard Gibbs producing his work only after Faraday's death. Yet much of Faraday's discourse is completely valid today. In 1991, the second centenary of Faraday's birth was an occasion for celebration and nostalgia at the Royal Institution. John Maddox in an account of the seasons main event describes the lectures of the two past Directors of the Royal Institution, George Porter and John Thomas. The former 'acting as if relieved that the constraints of being President of the Royal Society had been sloughed off…sat inside an electrically conducting cage' in a demonstration to establish 'that the electric charge of a conducting body lies on the surface – as Faraday had done at the same spot in 1836'. Thomas' discourse fittingly evoked memories of the lectures on the 'candle' reminding 'the audience that Faraday was first a chemist and only afterwards the founder of electromagnetism'. Did the public lectures, 'discourses', at the Royal Institution, founded by Count Rumford in 1799 as 'a centre for the popularization of the "mechanical arts" ' have a larger purpose in later years? According to Maddox, Humphry Davy, who succeeded Rumford and was Faraday's predecessor, realized that 'the Royal Institution was a means of making propaganda for the mechanical arts among the well-to-do of Mayfair and thus of raising money for research' (*Nature*, 1991, **353**, 599).

Writing and speaking about science in a manner that engages a large audience of the general public is a talent that few possess. In his *Science* editorial, Alda wonders 'if written and oral communication skills could be taught systematically throughout the entire length of a student's science education'. In teaching students 'to distill their message and write without jargon', Faraday's classic would undoubtedly be required reading. Faraday's great contemporary, Charles Dickens whose bicentennial is currently being celebrated believed that science 'does immense good – moral, social and intellectual – but only when it works hand in hand with imagination and reverence' (Jenkins, A., *Nature*, 2012, **482**, 32). In her essay, Alice Jenkins notes that this was a Victorian view and one shared by Faraday, who believed that 'in the pursuit of physical science, the imagination should be taught to present the subject investigated in all possible, and even in impossible views'. Dickens commissioned Percival Leigh in 1850 to 'rework Faraday's notes for his lectures on the candle' for a magazine, *Household Words*, that he edited. 'Leigh turned Faraday's notes into a narrative', involving family conversations at teatime, where a young boy enthusiastically describes Faraday's insights. Ironically, Dickens turned his back on science and Faraday when he vapourised a character in *Bleak House* by 'spontaneous combustion'. Victorian England seems to have been a stage in which science and storytelling coexisted comfortably, at times learning from the other. In her retrospective on Dickens' attitudes towards science, Jenkins concludes: '…when science conflicted with a good story – he combusted it'. I suspect Faraday (and possibly Alan Alda) would have secretly approved.

Flame chemistry continues to be a subject of research and Faraday is still cited in studies of 'hydrocarbon oxidation' (cf Taatjes, C. A. *et al*., *Science*, 2005, **308**, 1887). Candles even appear on journal covers, most recently in the 6 January 2012 issue of *Science*, which reports a new use of candle soot 'as a template for a transparent robust superamphiphobic coating' (Deng, X. *et al.*, *Science*, 2012, **335**, 67). Not only Alda's 11-year olds, but even serious researchers may continue to ask: 'What is a flame? What's going on in there?' In introducing Faraday's Christmas lectures one hundred and fifty years ago, William Crookes concluded: 'the lamp of science must burn'. Indeed, it must.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

History and Entomology: Insects and War

History and entomology have little in common; subjects that appear to a disinterested observer as different as chalk and cheese. Children are exposed to both, a little history and a little zoology at school. A vast majority of students heave a sigh of relief when they leave these subjects behind them, their memories no longer taxed by the need to remember a great deal of factual information, which appears to have little bearing on their future careers. History is sometimes taught as a succession of battles and wars that have often been decisive in shaping the world as we know it. Entomology, the study of insects, is an esoteric activity. For most people insects seem an unglamorous subject of study. Having shed history at school and having no recollection of any classes that dealt with insects, I was drawn irresistibly to an article that appeared in the latest edition of the *Annual Review of Entomology*. The volume contains scholarly reviews of the current status of subjects as diverse as 'Insect nuclear receptors', '*Plasmodium knowlesi:* A malaria parasite of monkeys and humans' and 'Evolutionary ecology of Odonata'. A glance at a formidably technical table of contents should have been sufficient to dissuade me from further exploration of the book. However, the title of one review captured my attention. 'Insects as weapons of war, terror and torture' (Lockwood, J. A., *Annu. Rev. Entomol*., 2012, **57**, 205) is a gripping account which weaves together history and entomology, chronicling major human conflicts where insects became weapons in the hands of man.

The diversity of living organisms has always fascinated observers of the natural world. More than a century before Darwin, Carl Linnaeus began to describe and classify the observable world around him, laying the foundations of taxonomy. In his initial classification three major kingdoms emerged, Animal, Vegetable and Mineral. Of these, the last would disappear from the living world of biology, to be consigned to geology and chemistry, subjects that dealt primarily with inanimate matter. The animal kingdom, so evident around us even in the midst of the most crowded urban environments, is dominated by an abundance of insects. Cockroaches and spiders, flies and mosquitoes, ants and grasshoppers, bees and wasps are easy to find. A casual survey of any mildly open area will reveal a host of insects, many of which are invariably viewed as pests. For farmers, waging war on insects that attack crops, is an occupational chore. Insecticides, chemicals toxic to both insects and, at times, man, are invariably pressed into service. Some estimates suggest that insects make up about 90% of the animal kingdom. Entomology may indeed be one of the most important areas of biology. Honey and silk are produced by insects, examples where insect biochemistry has been harnessed to serve human needs. Amongst insects, beetles of the most varied appearances can be readily seen. J. B. S. Haldane in his 1949 book, *What is Life, The Layman's View of Nature* wrote: 'The Creator would appear endowed with a passion for stars, on the one hand and for beetles on the other, for the simple reason that 300,000 species of beetles are known, and perhaps more, as compared with somewhat less than 9000 species of birds and a little over 10,000 species of mammals. Beetles are actually more numerous than the species of any other insect order. That kind of thing is characteristic of nature.' Haldane, of course, is credited with the more memorable turn of phrase, where he is reported to have said that the Creator, if he exists, 'has an inordinate fondness for beetles'. An oft told story, probably apocryphal, suggests that this was Haldane's response in a conversation with theologians who asked, 'what can one conclude as to the nature of the Creator from a study of his creations?' The phrase 'inordinate fondness for beetles' appears to have acquired near immortality, despite doubts about its origins. As always, the internet provides a marvelous resource for those who like to check the origins of their favourite quotes – *Quote Investigator* (dedicated to the investigation and tracing of quotations, http://quote-investigator.com).

Insects are small and light; their size conferring on them characteristics that are absent in large animals. In his classic essay 'On being the right size', Haldane explains why 'an insect … is not afraid of gravity'. Insects always seem to be defying falls and gravity with a disdain that can hardly be emulated by larger animals and certainly not by man. Haldane notes: 'You can drop a mouse down a thousand-yard mine shaft; and, on arriving at the bottom it gets a slight shock and walks away. A rat is killed, a man is broken… For the resistance presented to movement by the air is proportional to the surface of the moving object. Divide an animal's length, breadth and height each by ten; its weight is reduced to a

thousandth but its surface only to a hundredth. So the resistance to falling in the case of the small animal is relatively ten times greater than the driving force.' In his essay 'Size and shape', Stephen Gould was, as always, eloquent: 'We are prisoners of the perceptions of our size and rarely recognize how different the world must appear to small animals. Since our relative surface area is so small at our larger size, we are ruled by gravitational forces acting upon our weight. But gravity is negligible to very small animals with high surface to volume ratios; they live in a world dominated by surface forces and judge the pleasures and dangers of their surroundings in ways foreign to our experience' (McGarr, P. and Rose, S. (eds), *The Richness of Life: The Essential Stephen Jay Gould*, Vintage Books, London, 2006, p. 320). Even idle, untrained watchers of insect colonies always come away with a feeling of wonder for the diversity of biology.

Having rarely thought about insects, except when confronted with pests at home, I found a current review on the use of insects as weapons of war quite intriguing. Lockwood's article in the *Annual Review of Entomology* traces the uses of insects in war from pre-historic times to the present day. He notes that 'for thousands of years insects have been incorporated into human conflict with the goals of inflicting pain, destroying food and transmitting pathogens'. Clearly, insects can be 'weaponized' in many ways. The use of potent toxins extracted from insects to coat spears and arrows dates back to times before recorded history. Lockwood draws attention to Biblical accounts and Mayan texts which detail the use of insects. Tossing beehives at enemies appears to have been an early but possibly hazardous tactic. He describes a Greek account of a poison from India, now known as pederin, 'a toxin secreted by *Paederus* beetles (Staphylinidae)'. Curiously, he cites a book published in 2003 (Mayor, A., Greek fire, poison arrows and scorpion bombs. *Biological and Chemical Warfare in the Ancient World*, Overlook, Duckworth, New York) to add that pederin 'continues to be of interest to the Indian Defense Ministry'. Insects often transmit disease, serving as vectors that shelter and transmit pathogenic microorganisms to animals and humans. While the mechanisms by which contagious diseases spread have become well understood only in recent times, plague was transmitted by fleas in a victim's clothing transported into enemy lines. Lockwood notes that 'fleas can survive for hundreds of days between blood meals, so they could have functioned as an entomological booby trap'.

Military strategy in ancient wars was sometimes based on an intuitive understanding of the connections between insects and disease. Even in the fifth century BC, in an era long before 'explicit entomological knowledge' became available, an 'Athenian army was decimated by malaria when Sicilian commanders maneuvered their defensive lines to force the invaders to establish summer camps in the marshes or perhaps drew the enemy into the wetlands through the ruse of negotiating surrender'. Lockwood's sweep through history moves from the Middle Ages to the Napoleonic Wars. 'The tactic of launching biological materials into a besieged city is rooted in the fourteenth century.' Lockwood summarises scholarly studies that suggest that the plague which decimated Europe in the first half of the 14th century, killing 'more than one quarter of the continent's population' may have its origins in the Mongol siege of 'the Genoese seaport of Kaffa on the Crimean peninsula'. The spread of disease may have been hastened by the Mongol act of catapulting infected 'corpses into the city'.

Napoleon has never ceased to attract military historians. The influence of insects and disease on the Napoleonic campaigns is the subject of a fascinating account by R. K. D. Peterson (*American Entomologist*, 1995, **41**, 147). War and disease are intimately linked; 'deadly comrades' in Peterson's words. In Napoleon's time the connection between microbes, their insect hosts and disease was unknown. The insights of Pasteur, Koch and Ross were to come many decades later. Napoleon's campaigns were derailed by 'bubonic plague in Jaffa, yellow fever in Haiti and typhus in Russia'. Typhus in the trenches was probably more decisive in determining military outcomes in World War I than battlefield heroics, leading to the comment that many famed battles 'are only the terminal operations engaged in by those remnants of the armies which have survived the camp epidemics' (Zinsser, H., *Rats, Lice and History*, Transaction Publ., Edison NJ cited by Lockwood, 2007). The modern era where entomology meets war includes examples from World War II, the Korean conflict and Vietnam.

Insecticides, vaccines, antibiotics and a host of other pharmaceuticals now provide a defense against insect borne disease. But, there is an ever present danger of deliberate threats to agriculture which cannot be ignored. Lockwood raises the spectre of insects being potential weapons in 'asymmetrical conflicts'. In his words: '…the arc of history may suggest the future of entomological armaments'. Human history has been shaped by a myriad factors. It is a sobering thought that insects are among them.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Teaching Research: Addressing a New Generation

The romance of revolution inspired William Wordsworth to write his immortal lines:

'Bliss was it in that dawn to be alive,
But to be young was very heaven!'

The poet had just returned from France, where the winds of change were blowing rather hard. The French revolution, like all other revolutions since, was just about to enter its darker and more turbulent phase. Wordsworth's lines celebrate the advantages of being young when times change rapidly. The verse sprang to my mind when I received an invitation to speak at a gathering of mostly young scientists, all of whom were recipients of the Department of Biotechnology's Ramalingaswami Fellowships, which seek to attract biologists to research careers in India. Times are indeed changing and changing rather rapidly in Indian science. A growing number of schemes seeking to attract the young into science have been introduced in recent years. The now venerable Kishore Vaigyanik Protsahan Yojana (KVPY) scheme, which provides scholarships for undergraduates in science, is quickly being dwarfed by the INSPIRE fellowship scheme of the Department of Science and Technology (DST). For young scientists just coming out of a Ph D program there are multiple postdoctoral opportunities, with the D. S. Kothari fellowships of the University Grants Commission (UGC) being an attractive prospect. A little more experience (a stint overseas may help) is enough to qualify researchers, at the beginning of their careers, for a plethora of new schemes; the Ramanujan fellowship and Inspire Faculty fellowship schemes of DST and UGC's recently announced 'faculty recharge' program. Research grants have increased in number and the quantum of funding has increased steadily over the last few years. Many new academic institutions, some devoted exclusively to science, have been created. There is a growing perception that jobs in academia are significantly more attractive than in the past. In a bid to attract new faculty to man the growing enterprise of higher education and research, biologists have invented a new device. Meetings of 'young investigators' with institutional heads, seeking to spot fresh talent, are now held periodically in India and the United States. The locations are always attractive, ensuring good participation. The research job market appears to be expanding with institutions focusing on young researchers hoping to return to India from western laboratories.

The invitation I received to speak at the gathering of Ramalingaswami fellows was precise and explicit. I was asked to speak about 'doing science in India, then and now'; more precisely on the 'status of Indian biology, then and now'. Clearly, the organizers were aware that I had been around for a long time trying to do research in areas in which I had a moderate degree of expertise. Since I had observed the changing face of science in India for nearly forty years, I was expected to reflect on the past and the present, ending undoubtedly on an optimistic view of the future. I approached my assigned task with some element of regret. It is not entirely a comfortable transition from the position of a player to that of a commentator, for both cricketers and scientists. I was also charged with the difficult task of providing 'an insightful perspective'; an assignment that I was clearly incapable of completing. Even while I was wondering about the wisdom of talking about Indian biology's progress through the decades, I was fortunate to receive an unexpected gift of a book: *Secrets* by Ruskin Bond (Penguin Books, New Delhi, 2011). In his engagingly direct and simple prose, Ruskin Bond points to the advantages of aging: 'One of the nice things about growing old is that we have so much to look back upon – a stream of memories that never runs dry; people who have been dear to us; the friends of one's youth; faces from the past; adventures; memorable events; dreams lost and found; the good, the bad, the beautiful….' Heartened by Ruskin Bond's words, I gathered some courage to look back at Indian biology, as I saw it as a young man and as I see it now, tempered by age.

Biology in India has been the subject of many recent commentaries. Not too long ago the *Journal of Cell Biology* featured the Taj Mahal together with the DNA double helix on its cover (2009, **184**, 342). A little later *Science* carried an editorial entitled 'Young leaders for biology and India' (Tole, S. and Vale, R. D., *Science*, 2010, **329**, 1441). In preparing for my task of reflecting on the past and present of Indian biology, I re-read these two commentaries that presented a perspective that seemed unfamiliar to me. The future, undoubtedly, belongs to the young. The *Science* editorial concluded with sound advice: 'The best guarantee for future success is to imbue young scientists with a sense of mission, to nurture

not just the science but a culture that fuels the will to succeed in countries long thought of as second choice options for research careers.' Reading the last few words I could not but wonder: Was India of the 1960s, 1970s and 1980s a 'second choice option' for those exposed to Western laboratories? Or was it indeed a 'first choice option' for those who were committed to research, but also deeply attached to home turf? Like Ruskin Bond, memories suddenly seemed to flood the mind. The last four decades have been marked by political, economic and technological changes. The Emergency, the era of import licences and cyclostyling machines are images from the 1970s. The first of India's major funding agencies, DST was born in the 1970s and began taking the first steps towards transforming the way in which research was encouraged and supported. The 1980s began with DST's Baroda seminars, which led to the formation of program advisory committees and introduced peer-review of individual investigator generated projects. The M Sc Biotechnology programs appeared in the 1980s and photocopying machines and word processors entered offices. Politics was turbulent and as the decade ended the economy teetered on the brink of a crisis. The 1990s began ominously with an imminent payment default, but the transformation happened soon afterwards, as the economic liberalization coincided with the information technology revolution. The new century, one in which many of the most promising researchers in India have begun their careers, has seen a dramatic growth in the scientific enterprise. In thinking about biology's past I turned inevitably to the institution builders who conceived their ideas in the 1970s and executed them in the 1980s. Two of India's most prominent biological laboratories, the Centre for Cellular and Molecular Biology (CCMB) at Hyderabad and the National Institute of Immunology (NII) at Delhi were built in the 1980s. Molecular biology in India grew out of the strong tradition of biochemistry and genetics in many institutions, pushed forward by scientists working in difficult conditions across the country. Structural biology, an old discipline with a new name, grew out of modest laboratories at Madras University. The past can sometimes be overwhelming and as I attempted to gather my thoughts it appeared wise to break into the present.

What advice can an aging scientist, limited by only a nodding acquaintance with biology as it is practiced now, give an audience of ambitious and focused young researchers. The old clichés, work hard, follow your instincts, choose your problems carefully, do what is important not what is doable, sprang immediately to mind. Brushing aside the temptation to tread familiar ground I decided to address two issues that are matters of concern to many young researchers, beginning careers as independent investigators in India – research facilities and teaching. Everyone I have met (or almost everyone) seems to want more of the former and less of the latter. For those with an insatiable desire to accumulate sophisticated research facilities, I turned to Richard Hamming: 'It is a poor workman who blames his tools. The good man gets on with the job, given what he has got and gets the best answer he can.' This old quote from a computer scientist dates back to the 1980s and was hardly what my audience wished to hear. I pressed on to my pet topic; the need for researchers to teach. Indeed with the creation of the IISERs, there is a purposeful move to integrate undergraduate science education with research. Teaching can greatly benefit young researchers. Richard Bach (author of an old 1970s favourite, *Jonathan Livingston Seagull*) said it well: 'You teach best what you need to learn.' Einstein, quotable on most subjects, said: 'Teaching should be such that what is offered is perceived as a valuable gift and not as a hard duty.'

The future of science in India depends substantially on those who are starting out as researchers today. It will depend critically on the students who enter science, hoping to build careers in their chosen disciplines. Science, as taught today, can hardly enthuse a new generation, which is fortunate enough to encounter an ever expanding range of career options. The excitement of science and the pleasures of research can only be communicated by exposing students at the earliest opportunity to laboratory work, in which outcomes are not always anticipated. A recent editorial in *Science*, which appeared after my encounter with the audience of young researchers, argues that it is important to 'advocate and provide support for replacing standard laboratory courses with discovery-based research courses' at undergraduate level. In a critique that is undoubtedly relevant, even for the best of our programs, the authors add: 'Too often, even the active learning elements of today's teaching regimens – laboratory courses – simply repeat classical experiments rather than engage students in compelling experiments with the possibility and excitement of true discovery' (Gates, Jr S. J. and Mirkin, C., *Science*, 2012, **335**, 1545). The issue of *Science* also carries two essays written by winners of the IBI *Science Prize* for Inquiry-Based Instruction. Both essays deal with experiments in biology. The essay, 'Investigating Arabia Mountain: A molecular approach' describes the involvement of a group of undergraduates to the problem of investigating microbial diversity in the local environment using DNA analysis (Jacob, N. P., *Science*, 2012, **335**, 1588). The second essay, 'Adapting to osmotic stress and the process of science' highlights an exercise in bacterial genetics providing insights into evolution, resulting in a publication (Gasper, B. J., *Science*, 2012, **335**, 1590).

Teaching students in the classroom and in the research laboratory may be an investment which will ensure that the future of biology in India is secure and bright. This will undoubtedly be true for the other disciplines of science. For young researchers, teaching must be a joy, not a chore to be avoided.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Academic Science: Facing the Glare of Public Scrutiny

'Academics do live very sheltered lives by today's standards – how many careers offer tenure or employment for life? And, frequently, an individual's insistence on working as free from tethers as possible can be taken too far, forcing everyone and everything around them to accommodate their needs. This becomes a waste of time and effort – in other words of money. …Has the concept of absolute academic autonomy become a luxury that the scientific enterprise can no longer afford?' These are words that might be considered as blasphemous in academic circles. Yet they appear in an editorial entitled 'Creative tensions' in the April 5 issue of *Nature* (2012, **484**, 5). This critique of the ways of the world of academia begins on an ominous note: 'Scientists must find ways to improve academic efficiency if they are to keep their independence.' *Nature*'s call for scientists to take a hard look at practices in the community appears to have been stimulated by 'this age of economic austerity'. Having spent considerable time moving uneasily between the freedom of the laboratory and the more constrained world of the academic administrator, the *Nature* editorial left me thinking about responses which seemed distinctly conflicting. To those charged with running and managing academic institutions, the freedom enjoyed by faculty can sometimes appear to be fostering a state of anarchy. To academics pursuing research goals with a single minded devotion, administrators are inhibitory forces impeding progress. The *Nature* editorial draws attention to a column in the issue, 'Clean up the waste', written by a management consultant, which is bound to spark considerable discussion (Marty, T., *Nature*, 2012, **484**, 27). A key conclusion of the analysis, one that will undoubtedly be challenged, is that the 'main sources of inefficiencies are a wrong understanding of autonomy, weak leadership and a lack of strategic thinking when selecting research areas'. Writing in the background of the economic downturn in the West, the author argues that 'efficiency is largely about saving time and effort, not reducing expenditures'. In a section titled 'reduce autonomy', the author notes (and many administrators will silently agree, even as academic faculty disagree) 'that autonomy can be taken too far'. He argues that 'academics have a tendency to set their own priorities; administrative matters are regarded as unimportant and managerial decisions are usually taken at the last minute with little consideration for the consequences'. His conclusion, that 'although such behaviour might be favourable to the individual, it represents a heavy burden for the institution as a whole', is one that merits attention. I was reassured to read that Western institutions also suffer from governance deficits. The author notes that 'decisions are often taken at the wrong level of hierarchy, involving too many people or too great a focus on details. This is evident in the overuse of large-membership committees which leads to decisional paralysis and takes professors away from research and teaching'. Ponderous committees, and the unending search for consensus often facilitate 'decisional paralysis' in our own institutions.

Marty notes that a 'major source of inefficiency at academic institutions is weak leadership'. The short duration of many management posts, especially in Europe, appear to arise from faculty distrust of leaders. In India, rapid rotation of heads of departments and other functionaries often ensures that progressive reform rarely happens. There are some pieces of advice that may well be worth thinking about. The author, undoubtedly influenced by the corporate world, emphasizes the need to think strategically when building research capabilities. There is some sound advice in the midst of uncomfortable criticism. He reiterates what is common sense: 'Notably, doing more research in a given area increases the efficiency of the whole institution.' Shared infrastructure, especially major facilities, cuts costs and promotes more discussions amongst researchers, especially when inter-personal relationships are cordial and professional. A view that must strike a chord in many managers of science is voiced: 'Departments also tend to hire academics who are studying hot topics, such as stem cells. But not everyone can bring together the critical mass of scientists required to become a prominent and successful stem-cell institute.' The suggested prescription is to 'identify and expand areas…which are already doing well rather than starting small institutes from scratch'. This is a route that has been pointedly and deliberately ignored in India, especially in 'hot funding areas' like biotechnology and nanotechnology. Indeed we have, over the last few years,

created many new institutions in highly specialized, but glamorous, areas at considerable cost. The obvious is also stated, but nevertheless merits repetition: 'The bigger the centre for a particular subject, the more likely it is to attract brilliant minds. For example, an institution with a strong backbone in physical chemistry, experimental physics and engineering might bring those fields together to build a world class materials science department.'

Nature's critique of Western science models is three pronged. Two more columns in the April 5 issue address problem areas. The critics are from areas outside the 'hard sciences' – economics and management science. A commentary intriguingly entitled 'Perverse incentives' argues that 'counter productive financial incentives divert time and resources from the scientific enterprise. We should spend the money more wisely' (Stephan, P., *Nature*, 2012, **484**, 29). A telling example is the use of cash incentives to promote submissions to high impact journals; a strategy adopted in China, South Korea and Turkey. The author notes caustically: 'These payments have achieved little more than overloading reviewers, taking them away from their work, and have increased submissions by the three countries to the journal *Science* by 46% in recent years, with no corresponding increase in the number of publications.' Stephan adds: 'Sadly, science is full of incentives gone awry. Look no further than expanding Ph D programmes that produce graduates with almost no career prospects or the growth of lab space with no apparent increase in productivity.' The commentary highlights the growth of laboratory space for the biological and biomedical sciences over the past decade or so. 'Biology's research footprint' has grown to a staggering level in the West, leading the author to suggest: 'Perhaps it is time for deans in the biomedical sciences to rent some of their excess space to their colleagues in chemistry and physics.'

A third commentary addresses the issue of whether current funding models used in the West need serious restructuring. Pierre Azoulay, an academic in the Sloan School at MIT, argues that it is time to 'turn the scientific method on ourselves'. He suggests that funding mechanisms need to be evaluated using the very same methods used in assessing research results. He is critical of 'retrospective analyses using selected samples' which often turn out to be 'little more than a veiled attempt to justify past choices' (*Nature*, 2012, **484**, 31). His suggestions, clearly directed towards funding by the National Institutes of Health (NIH) in the United States, merit some attention in India, where new experiments in funding modes may be needed to address the demands of an expanding research community, working in a large number of exceedingly diverse institutions. These critiques of academic science must be viewed in the background of the prolonged and unremitting economic crisis in the West, which has a strained a vastly bloated research enterprise. Are these concerns relevant in India, where the warm glow of expanding research budgets has dominated thinking at the science departments of the central government, over the past few years? If this year's budget projections are an indicator, the era of rapid growth in research spending may be drawing to a close. Governance issues at funding agencies often neutralize the advantage of larger budgets. Institutions in India also struggle with internal constraints, historical inheritances which show no signs of disappearing, that contribute substantially to widespread inefficiency in coping with the volume of work generated by greater spending. The need for a dispassionate relook at the strategies for promoting and expanding the base of science in India is emphasized in a recent commentary. Gautam Desiraju advances a provocative thesis that science in India 'needs clear and honest leadership, not more money'. He is forthright in remarking that 'more money is better, but if there are deep cultural and social problems, it will simply drain away'. The prevailing emphasis on quantitative metrics comes in for criticism. 'Impact factors and *h*-indices become the sole arbiters of scientific excellence' in an environment 'that lacks innovation and creativity' (Desiraju, G., *Nature*, 2012, **484**, 159). He is concerned, and rightly so, of an increasingly perverse system of continuing monetary rewards, that has resulted in creating a new generation of scientists, singularly obsessed with awards and fellowships which have been made financially very attractive. Money, he notes, 'is neither the cause nor the solution to our problems, although it can facilitate progress in an otherwise healthy climate'. Critical voices are being heard with increasing frequency in India. In a letter published in *Science* (2012, **335**, 1440), P. M. Bhargava castigates the leaders of science: 'The scientific leadership in the country, with notable exceptions, rewards sycophancy and punishes independence, integrity, effective communication, scientific competence, and credibility'. This is a harsh assessment but one that should provoke introspection.

A news report in *Science* (2012, **335**, 904) titled 'India Rising' presents a positive view of growing opportunities for science in India: 'As India's economy roars and Western nations limp along, the trickle of talented expatriates returning home may turn into a flood'. Curiously, there has been little discussion on the increasing number of Ph Ds produced by our own institutions who struggle to find academic positions. The expansion in Indian science is driven by experiences of the successes in the West. Whether these models will prove fruitful in a new century in a vastly different environment remains to be seen. As public investments and expectations increase, scientists in India may have to learn to function in the harsh glare of public scrutiny.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Innocence and Sophistication: Users and Equipment

About twenty years ago, as the era of the tightly controlled economy began to slowly fade away, universities and research laboratories in India were poorly equipped. Major facilities for research were to be found in very few national institutions. Even the best of our laboratories could only boast of equipment which were far from being comparable to that available in most laboratories in the West. Changes have happened rather dramatically in the last fifteen years. Science funding has grown steadily, foreign exchange is not a limiting factor and customs duty exemptions and clearances for publicly funded institutions are available. Most importantly, aspirations amongst a new generation of scientists are significantly higher than those of their predecessors; an inevitable corollary of globalization and liberalization. A host of government schemes now provide grants to equip laboratories in universities and national institutions. Internal funding, in the laboratories supported by large organizations like the Department of Atomic Energy (DAE) and the Council of Scientific and Industrial Research (CSIR), has grown substantially. In the last twenty years, government departments, originally conceived as funding agencies promoting research, have created and acquired institutions of their own, which then receive significant core funding. Both the Department of Science and Technology (DST) and the Department of Biotechnology (DBT) oversee a growing number of dedicated research laboratories, which proudly display state-of-the-art facilities. Once impoverished University science departments are now in the fortunate, but unfamiliar, position of being able to seek support for major equipment from a multiplicity of agencies. New schemes introduced by the University Grants Commission (UGC) are available to equip research laboratories, even in colleges. UGC's new initiatives have been characterized by a departure from the traditionally slow decision making process; an empowered committee cuts through many of the delays in fund disbursement. The drive to infuse more money into research has not always been accompanied by the creation of new administrative and financial mechanisms, both at the funding agencies and the recipient institutions. Grants may be sanctioned and received, but utilization can be a bottleneck as many institutions struggle with non-functional administrative structures. The first instalment of grants is usually received. Subsequent instalments are uncertain, as finance department at institutions and the agencies engage in a war of attrition. The situation is compounded by financial rules written for another day and age. The sudden spurt in the number of grants and grantees strains administrative structures, which were created long ago in a more leisurely era.

A consequence of the rapid increase in funding levels is that India has quickly become an important market for manufacturers of sophisticated scientific equipment. Institutions, old and new, acquire major research facilities with a frequency that was unheard of some years ago. Instrumentation centers and centers of 'excellence', a much abused word, are emerging across the country. Many years ago when a 'sophisticated instruments facility' was set up at the Indian Institute of Science in the late 1970s, it boasted of a single NMR spectrometer operating at 270 MHz. Upgradation and expansion was a very slow and arduous process until the late 1990s. In the 1970s, biochemistry and biology departments used relatively few pieces of major equipment. Even spectrophotometers and refrigerated centrifuges were prized departmental possessions, to be used in a disciplined manner. Ultracentrifuges were looked upon with respect, rotors jealously guarded and users carefully instructed before they were permitted to begin their work. Money was scarce, equipment scarcer and spares and services were difficult to obtain. The revolutions in biology and materials characterization occurred in the late 1970s and throughout the 1980s, leading to an explosion in the range of techniques used to characterize and analyse molecules, materials, cells and tissues. Microscopy suddenly transformed our view of materials and biology. Electron microscopes of ever increasing resolution, with accessories that probe chemical composition raised the bar on materials characterization. Confocal microscopes, exploiting the remarkable power of lasers and the ability of molecular biologists to introduce optical probes into cells, quickly became indispensable tools in cell biology. The human genome project spawned a host of new technologies. DNA sequencing is being achieved in ever shorter times at rapidly falling costs, with a reliability that was hardly anticipated in the days when the genome appeared an almost impossible goal. The 'omics' revolu-

tion in biology has been fuelled by the ever increasing resolution and sensitivity of mass spectrometers. It is hard to believe that mass spectrometers capable of analysing biological macromolecules were unavailable in our laboratories even in the late 1990s. In the span of less than a decade Indian institutions, public and private, have become major buyers of mass spectrometers. High performance computing is another area which has exploded, with 'clusters' proliferating in many institutions; a dramatic change from the past when computing was always centralised in the 'computer centre'. Every area of science has seen an explosive growth in the sophisticated tools necessary for modern day research. Unsurprisingly, the many new institutions that are being set up begin with formidable shopping lists for equipment, even as they struggle to recruit competent faculty to exploit the facilities being created. Older institutions replace and upgrade instrumentation with a frequency that was unimaginable a decade ago. Individual laboratories now possess equipment which not too long ago were prized as central facilities. Despite this sudden improvement in the facilities available for research, it is hard to visit an institution without hearing complaints about inadequate supporting infrastructure, which limits efficient utilization of the available equipment. Complaints about uncertain power supply are commonplace. A more disturbing grouse is the absence of trained technicians, with a high level of competence in operating and maintaining facilities.

A great deal of the most sophisticated analytical facilities used today can indeed be directly operated by trained users. Microscopes and spectrometers of all kinds should, in principle, be accessible to direct use by researchers, primarily students, provided they are adequately trained and supervised. In most, if not all, institutions 'operators', with a minimum knowledge of the methods that they use, stand as custodians of sophisticated facilities. Users, who often display a degree of technical innocence that is almost touching, turn to these operators for solutions to problems that occupy their attention. Very often it is a case of the 'blind leading the blind'. Few major facilities in India are staffed by technically competent professionals who can indeed address problems posed by their clients. Most major facilities are sub-optimally used and sophisticated instruments are rarely exploited to their full potential. Senior researchers who preside over large groups of students and assistants generally have a limited 'feel' for the wide range of analytical methods that are required for much of the characterization that is central to modern research in biology and materials science. Ph D students sometimes use a bewildering range of methods in their research, but profess ignorance of the principles behind the techniques used in their studies. Unfamiliarity with instrumentation results in a quick demise of user operated facilities, as a consequence of casual and cavalier treatment of spectrometers, chromatographs and other pieces of relatively widely used equipment. In the better funded institutions and laboratories replacing equipment appears more attractive than resurrecting instruments that have suffered from misuse. Poor training of students in the methods essential for their research is often the root cause of the many mishaps that occur in research laboratories. The failure to solve problems that are amenable, despite the availability of analytical facilities, is a direct result of a lack of appreciation of the intricacies of interpretation. Problem solving in many areas requires both good experiments and careful interpretation.

In thinking about the new problems created by the funding boom, I was drawn to a recent commentary in *Nature* which argues that 'over-reliance on automated tools is hurting science'. In a column entitled 'Understand how it works', the head of Vanderbilt University's core microscopy laboratory emphasizes the 'need to do a better job of teaching students how techniques work before they start using them (Piston, D. W., *Nature*, 2012, **484**, 440). He illustrates the pitfalls that may be encountered in microscopy using automated image analysis. Describing a specific example of a mis-interpretation he concludes: 'In this case it wasn't inspiration that was lacking – it was instruction. The researchers had used a proven and validated tool, but in a way inappropriate for the problem at hand.' Piston voices a concern that research supervisors might do well to heed: 'Unfortunately, this scenario is becoming all too common in many fields of science; researchers, particularly those in training, use commercial or even lab-built automated tools inappropriately because they have never been taught the details about how they work.' He notes correctly that 'the interdisciplinarity of modern biomedical research makes it almost impossible for one person to understand the subtleties of all the procedures on which they rely'. He goes on to argue for creating an instructional programme in training research students which will 'emphasize better the fundamental concepts and practice of experimental techniques. This would necessarily include hand-on-labs involving state-of-the-art equipment and instruction from experts with proven success in using these techniques'. His conclusion, that students must not only be taught 'how to formulate hypotheses, design research approaches and write manuscripts, but also how to build, implement and troubleshoot their experiments', merits the most serious attention.

Indian institutions need to enhance the level of technical competence in their core facilities, examine ways of recognizing and rewarding those who play an important role in sustaining facilities and promoting enlightened usage, and plan on organizing instructional workshops centred around major research equipment. Technical innocence, in an age when experimental facilities are rapidly expanding, may no longer be acceptable.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Interdisciplinary Research: Silos and Walls

Newly appointed heads of institutions, especially old and traditional ones, are usually enthusiastic about changing and influencing their environment. I was no exception, when some years ago circumstances suddenly pitchforked me into the job of the steering a course for the Indian Institute of Science (IISc). This is an old institution, tracing its evolutionary history over the course of a dramatic and turbulent century. Its calm and serene surroundings are in complete contrast to the hustle and bustle outside its boundaries. In 2005, when I began my new task, the new century seemed to be in its infancy, full of promise for the future. The first decade still had a long way to go. A new government was in place and the economy was growing rapidly. Change seemed imminent. Academia, worldwide, was just beginning to learn the use of new tools to assess institutional performance. Grading research performance and producing ranked lists of universities and academic institutions was a new phenomenon. The Shanghai rankings, so widely dissected in subsequent years, emerged in 2003. The *h*-index, first introduced unobtrusively in a paper posted on the physics arXiv appeared in 2005, and was largely unknown in Indian academic circles. Few could have predicted then that this single number index of research performance would spread like a cancer through academia across the world. I was an early follower of the Shanghai ranking system; quickly seduced by the simplicity of quantitative analysis based on easily measurable parameters. The Hirsch index, first introduced to measure individual performance, also seemed irresistible. It was clear that in an intensely competitive global environment, Indian institutions needed to understand the rules of a new game in which peer perceptions of institutions were based on quantitative measures of performance. Increasing institutional productivity in a significant way was, undoubtedly, the way forward. Since academic institutions are invariably judged by the number of publications produced by the faculty and students, it appeared sensible to urge everyone to do more. While a boost in the quantity of output was clearly essential in making a move up the rankings chart, enhancing the quality of publications and their 'impact' was also an imperative. In the current 'era of impact factors', publication quality is judged by the number of citations a paper receives or, somewhat more simply and quickly, by examining the impact factor of the journal in which it appears. The former is limited to judging papers published years earlier, while the latter is full of infirmities, but is a favourite of those with little time to worry about the niceties of academic evaluation.

'Impact' is a word pregnant with meaning. Presumably, proponents of the impact factor mode of evaluation believe that papers published in the best of journals have a higher probability of advancing the field. It is also generally true that when scientific advances are made, the fall out is felt by the environment in a positive manner. In considering ways of enhancing 'institutional impact', I became aware that for some time the best of institutions, the world over, had been experimenting with strategies to enhance interdisciplinary research. Science in the 21st century was moving rapidly in a direction, where the borders between traditional subjects were turning porous. The revolution in biology in the latter half of the 20th century created a 'new world', welcoming immigrants from every discipline of science. Chemists, physicists, mathematicians, computer scientists and engineers of all persuasions were suddenly interested in problems of biological interest; making inroads into an area that had long been the province of an insular discipline. Materials science, the precursor to the currently glamorous area of nanoscience and technology, exploded in the 1980s and 1990s. Research in biology and materials, almost necessarily, requires an interdisciplinary approach, if the promised breakthroughs in the areas of medicine, agriculture and energy are to be realized in the future. The most visible institutions in the West had been engaged for sometime in creating interdisciplinary centres; often focusing on the creation of new buildings and laboratories designed to foster interactions between researchers specialising in different areas of science. Proximity in an artfully designed ambience was advanced as a catalyst for promoting interdisciplinary research. A new breed of architects seemed to be becoming popular on academic campuses in the West; specialists in designing laboratories that facilitated collaborative interactions.

In my first surge of enthusiasm for stimulating an atmosphere of change, I began to envision the creation of an interdisciplinary research centre at IISc. The naivete of a newly minted institutional head led me to believe that

support, both academic and financial, would be readily forthcoming. The idea of building a physical facility that would house and seamlessly integrate scientists and engineers from various disciplines, nudging them towards the solution of problems that were clearly challenging, appeared attractive. The idea received a mixed response. Traditionalists and serious specialists dismissed the proposal as a short lived flight of fantasy. Dangerous optimists, who surprisingly can still be found in our institutions, were enthusiastic; convinced that it would indeed be possible to raise the resources needed to embark on a major new venture. I quickly realized any such proposal was a magnet for sharp criticism. Some critics argued that an interdisciplinary centre would merely be an expensive façade, behind which academics did whatever they pleased; with little by way of interdisciplinarity. These critics suggested that a specific theme needed to be identified, which would draw researchers with diverse specializations to work for a common goal. 'Focus' is a popular piece of advice when diffuse proposals are to be recast. Too sharp a focus can also attract criticism, especially when an interdisciplinary centre is intended to bring together a broad coalition of interests. To critics of academia, scientists always seem to function in isolation, pursuing esoteric interests. Individualism appears to take precedence over teamwork in academic institutions. A perceptive critic noted that the greatest danger in the projected centre would be the tendency of academics to work in 'silos'.

'Silos' is an uncommon word. I had heard of the term in connection with the granaries in America and the fortified launching pads for missiles, so often talked about at the height of the Cold War. The internet and *Google* quickly revealed that I was not alone in thinking of the term 'silos' as unfamiliar. A site that helps with English language and usage provided a precise meaning: 'A tower or a pit used in a farm to store grain'. A dictionary offered an alternative: 'An underground installation constructed of concrete and steel, designed to house a ballistic missile and the equipment for firing it.' There was also a metaphorical meaning: 'A system or process or department that operates in isolation from others.' The term seemed quite popular in a corporate context, where 'functional silos' emerged by the 'creation of barriers that do not serve any business purpose'. Having spent a lifetime in academia, I had to admit that 'silos' was an accurate descriptor of most departments in most academic institutions. Even as I surveyed the remnants of a stillborn project, a thoughtful friend drew my attention to a column entitled 'Mending walls', which reflected on the issues confronting the state of interdisciplinary research in the biomedical sciences.

Greg Petsko, a structural biologist of distinction and a prolific and highly readable columnist borrows his title from a poem by Robert Frost and begins with a quote:

'*Before I built a wall I'd ask to know*
What I was walling in or walling out.'

In talking about the difficulties of interdisciplinary research carried out with 'the aim to bridge the gap between discovery and translation to the clinic'. Petsko notes: 'One often hears the word "silos" used to describe the separateness of cultures. Silos is an evocative word, conjuring images of isolated white cylinders against the flat horizon of a Midwestern farm state.' He prefers 'to talk in terms of walls'. His argument in favour of walls over silos is compelling: 'Silos seem inherently separate. Walls can be knocked down' (Petsko, G., *BMC Biology*, 2012, **10**, 41). Petsko's concerns are primarily about the 'Balkanization of biomedical research funding by disease phenotype and organ system', which make 'connections between diseases … hard to find'. Genomics and all the other 'omics' that have followed demonstrate the unity of biology. Petsko argues that we need to 'think more about pathways and processes within the cell and organism'. What is true of biology and biomedicine is indeed true of most other disciplines. No major problem of our time in the areas of health, environment or energy is likely to be solved by a narrow, specialist approach. Teams whose members possess complementary skills need to be built and fostered. There is indeed a great need to experiment with new approaches to building structures for scientific research. The importance of interdisciplinary approaches to courses, degrees and training needs to be emphasized. Petsko asks: 'And shouldn't NIH (and its foreign counterparts) also develop specific mechanisms to promote cross-disciplinary training? Not just for basic researchers, where the idea is already fashionable, but especially for physician scientists.' In India, cross-disciplinary training is yet to be recognized as crucial for basic scientists, while 'physician-scientists' are an extremely rare breed.

Some years ago I was shown a building at one of our major universities. The founders of the institution and the architects had designed a structure which housed both the life science faculty and social scientists. A corridor connected the two wings. On both sides, the doors were shut, permanently. C. P. Snow's 'two cultures' had separated themselves. In thinking about interdisciplinary research, I could not help wondering if the critics of my proposal were indeed right. Petsko's column suggests that there is room for optimism. He returns to Robert Frost:

'*Something there is that dosen't love a wall*
That wants it down.'

Academic disciplines are not isolated in impregnable silos. They are separated by walls that can be broken.

P. Balaram

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EDITORIAL

Higher Education: Tilting at Windmills

The Joint Entrance Exam (JEE) of the IITs has for long been the flagship of the elaborate system of entrance examinations, that need to be faced by students attempting to enter universities and national institutions. JEE has, over the years, acquired an image that ensures a great sense of accomplishment to students who appear high on its merit lists. A high JEE rank is a badge of honour in the days and weeks that lead up to the admissions to the IITs. Many successful students have spent three to four years, of early adolescence, training for the examinations. Coaches and coaching schools have mastered the art of preparing students for the examinations; a no mean task given the widely diverse social, economic and educational backgrounds of those who emerge successful. Discipline and intensive exposure to problems mimicking those that appear in the JEE exams are a hallmark of the coaching centres. Residential centres provide a new dimension in regimentation. Many students focus on entrance tests, paying only cursory attention to their school board examinations. Of the hundreds of thousands of students who appear in the JEE, a small fraction are successful. The others must necessarily turn to other exams; and there are many. This intense competition for limited opportunities has led to many public debates on whether students are being over-stressed, in attempting to negotiate a bewildering range of hurdles in seeking admission to higher education institutions. While the pressure is the highest for the national, centrally funded institutions, admission is not always easy even in the best of state supported and private institutions. The Ministry of Human Resource Development (MHRD) embarked some time ago on a campaign to limit the entrance exam burden and to restore, at least to a limited extent, the importance of school board exams. A new formula has emerged for the JEE which has engendered considerable controversy. Charges that the 'brand equity' of the IITs is being diluted are being loudly voiced; suggesting that the JEE is the major contributor to the strength of an institutional reputation which is indeed high. Do institutional reputations depend predominantly on the rigour of their procedures to select students or is it also a function of their ability to act as a key transformative agent in the education of students? A long time ago post-graduate students who entered one of our more prestigious IITs, the institute at Kanpur, were poorly prepared, with limited exposure to their chosen disciplines. An institutional ethos transformed many of them, opening windows of opportunity that had never been sighted before. I was one of those who was seduced by science in an environment that was challenging, formidable, at times, and stimulating. This was true in the 1950s and 1960s of many of our universities, then staffed with faculty deeply committed to teaching, mentoring and furthering their discipline. The current drive to mend something that does not seem to be irretrievably broken, the JEE, appears to be focussing on a diversionary issue; ignoring the main problem of enhancing the quality of a large number of existing institutions, which in turn will provide stimulating educational opportunities for the growing number of students enrolling in higher education.

The JEE debate has been fuelled by a single slogan: 'Reduce the number of entrance examinations and move towards a single national test'. The stated objective is to reduce student stress levels and to provide an adequate weightage for school board exams, thereby enhancing their importance, while diminishing the influence of coaching centres. Stress arises from parental and peer pressure, which pushes students to aspire for entry to the best institutions. If the number of institutions is miniscule and the number of aspirants keeps rising, it is hard to imagine a new avatar of JEE being a 'stress-buster'. There is a clear and repeatedly recognized need for increasing the number of high quality institutions, expanding the number of windows of opportunity for students. Government, egged on by commissions, committees and diverse political compulsions, has indeed announced the creation of a slew of new IITs, IISERs and Central Universities. Many are up and running, but it will take some years before all the new institutions begin to be counted in the same breath as the old IITs. In this phase of expansion there was little public debate on our old institutions; hundreds of universities across the country. Was there a possibility of attempting to restore the sheen on the best of our old universities? After all, in the first quarter of a century after Independence, the Universities at Delhi, Calcutta, Madras, Bombay, Poona, Benares, Aligarh, Mysore and many others boasted of an academic ambience that was created by accomplished and enlightened

faculty, supported by a largely non-intrusive political environment. The decline that began in the 1970s has continued unabated and indeed accelerated in recent years.

A recent column highlights a specific example in the field in which I have been an avid watcher for many years. M. Vijayan, a structural biologist of considerable distinction, a former President of the Indian National Science Academy and a long term colleague, writes with feeling about the decline of an academic science department which once produced some of the most original contributions to emanate from post-Independence India. He chronicles the slow sinking into oblivion of the Department of Crystallography and Biophysics at the University of Madras (*The Hindu*, 20 May 2012). The centres origins may be traced to 1952, when G. N. Ramachandran, then barely thirty, moved from Bangalore to head the new department of physics, a position offered to C. V. Raman, who suggested instead that one of his most promising students be chosen. Ramachandran went on to build a vibrant school in a new area emerging at the confluence of biology, physics and chemistry. Two remarkable contributions are still discussed in contemporary research. In the early 1950s, Ramachandran produced the triple helical structure of collagen, close on the heels of biology's other famous helices; Pauling's α-helix in proteins and the Watson–Crick double helix for DNA. This was a feat which firmly announced that Madras University now had a rising star in its ranks. A decade later, the now famous Ramachandran Map emerged, a stellar contribution to structural biology, that has found its way into textbooks of biochemistry. By the late 1960s, one of India's most visible research centres was located at Madras, in the ambience of a state university. The decline began in the new decade, with Ramachandran and many of his colleagues migrating to Bangalore. A once proud department, which boasted of one of India's first computers and a fine tradition in crystallography was on the way downhill. The local conditions turned inhospitable for true academic work and even more importantly, slowly allowed retirements to whittle down the faculty to an insignificant size. The ability to slowly starve departments to death has been practiced effectively in many universities. 'Inappropriate appointments', a phrase used by Vijayan, is another factor that has hastened the decline of university departments across the country. State universities face constraints imposed by their environments. Central universities, to some extent, have a greater degree of autonomy from government. The current drive to expand higher education focusses on the creation of new institutions; the large, old universities are usually left alone, with an occasional display of financial largesse when the annual budget is presented.

A critical problem facing the new institutions, and indeed old ones which attempt renewal, is the apparent shortage of good, well trained faculty. Many new institutions seek to have a predominantly research focus, sometimes paying little attention to the ability of newly recruited faculty to develop into competent, if not inspired, teachers. The government and Parliament have readily acquiesced in effecting a feat of alchemy, by which research organizations with specific mandates are transformed into deemed universities and 'academies'. The very notion of a university is sometimes subverted in the headlong desire to acquire a status that provides a legal sanction to award degrees. Many young, potential academics would prefer to work in an environment devoid of the demands of teaching, which also labels them as 'professors'. The rise of a professoriate which does not 'profess' is a sign that is ominous for the future of higher education. The benefits of teaching in research are often recognized only at a relatively late stage in academic careers. If faculty are hard to come by how will new institutions fulfil their goals and the aspirations of an ever increasing number of students? How do old institutions cope with faculty attrition? Even as India's economy stutters, austerity and resource constraints are terms that will be heard more frequently. Hard times will be familiar to all those old enough to remember. 'Sanctioned posts' will become more difficult to obtain further constraining faculty induction.

The attempt to reform the sector of higher education in India began several years ago with a slew of recommendations from the National Knowledge Commission, the Yashpal Committee and many other committees focussing on specific disciplines. The key issues of institutional governance and autonomy were the focus of many recommendations. The number of Bills pending before Parliament is testimony to a desire for change. Unfortunately, reform in its truest sense cannot be centrally legislated. It must come from within institutions, exercising the limits of autonomy that they possess. It must come from a perceived need within institutions for change. Government and the relevant ministries both at the Centre and the States can only act to facilitate, catalyse and promote the process of change. Sadly, there is little real interest in higher education reform in these circles. Reforming the JEE by a central diktat may be the first salvo in a battle for change, that is beginning to resemble one of Don Quixote's famous campaigns. Tilting at windmills may be entertaining, but hardly effective.

P. Balaram

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EDITORIAL

Impactitis and Predatory Open Access

'Impactitis' is an infection that appears to have spread like wildfire through India's scientific community. The disease may have first been identified in Europe (Zwahlen, M. *et al.*, *Soz. Praventivmed.*, 2004, **49**, 19). Government departments routinely request (and often insist) that publication lists indicate the impact factors of the journals in which the listed papers appear. The more demanding may ask for citation counts. Scientists, in turn, have become increasingly self conscious. It is not uncommon to find researchers who spend significant effort to add impact factors to their publication lists. 'Average impact factors', a wonderfully misleading index, popularised by a science agency some years ago, are proudly displayed as a vital statistic on curriculum vitae. A variant of the disease compels researchers to display '*h*-indices', another single number criterion which permits ready ranking of individuals. Science can be assessed, even by the ignorant, by simple minded comparisons of average impact factors and the *h*-index. For administrators in science departments of government and academic institutions, these tools of measurement allow judgements to be made without any knowledge of the subjects being considered. The ability to apparently rank scientists and institutions using a single number often fuels the arrogance of science administrators. A combination of ignorance and arrogance can be invincible. Senior scientists, who serve on advisory bodies and peer review committees quickly get infected by the 'impactitis' virus and its numerous variants. A new breed of 'information scientists', with little understanding of the practice of science is proliferating; producing mind numbing figures and statistics, continually inventing new metrics which quantitatively 'measure' science. Thomson Scientific (*Web of Science*), Elsevier (*Scopus*) and Google (*Google Scholar*) market the databases and tools for analysis. A quick view of your competitors *h*-index is only a mouse click away. Age and seniority (and presumably, experience) do not provide protection from this virus. Otherwise reasonable academics, who should know better, quickly succumb to 'impactitis'. The young are not immune. It is commonplace to find students, on the verge of submitting their first paper, choose journals on the basis of impact factors; often using even the second decimal to discriminate. The reservoir of this virus is undoubtedly well nourished in the organisations that are charged with funding science and recognising and rewarding scientists – government departments, academies and institutions.

I have had a love–hate relationship with scientometrics over the years. As an early follower and admirer of Eugene Garfield's seminal work in the analysis of science and its development, by inventing tools to mine the scientific literature, I was fascinated by the use of citation networks to map the intellectual origins of new fields. The *Web of Science* seemed the key to mapping the sociology of science. The journal impact factor appeared indispensable to librarians managing tight budgets and a demanding clientele. Garfield's essays uncovered the importance of scientific work unknown to a wider community. I recall reading in the 1980s, with rising excitement, the work of Sambhu Nath De who discovered cholera enterotoxin in Calcutta in the 1950s. In analysing the work of S. Chandrasekhar, the legendary astrophysicist, who was to later receive the Nobel Prize, Garfield discovered his cousin S. Chandrasekhar, whose work contributed greatly to the development of liquid crystals. Citation analysis in the early days was a tool that seemed to uncover fundamental contributions, with little need for a deep knowledge of the subject involved. In the 1980s computers and networks were rare and the internet was still in gestation. The information technology revolution that followed in the 1990s transformed the practice of scientometrics. Over the last decade, the tools of scientometrics have been readily accessible at many centres in India. Citation counts and impact factors now have uses far removed from those envisaged in the early days. They are, today, mundane metrics used thoughtlessly to rank scientists and institutions; dangerous weapons in the hands of administrators and politicians. My distaste for constant emphasis on impact factors of journals prompted a mischievous colleague to draw my attention to an editorial in a professional journal, with the stated intention of providing 'ammunition' for this column. I have been quick to succumb to the bait.

In an article aimed at 'dispelling the myths surrounding the Research Excellence Framework', P. Trevorrow and D. A. Volmer criticise the use of journal metrics in determining the outcome of the institutional funding

exercises in the UK. The British appeared for some time to be the last bastion surviving the onslaught of 'impactitis' in Europe. Their Research Assessment exercises in the period 1986–2008 were based on conventional peer review panels. Most importantly, 'the use of journal metrics (citation counts, impact factors, etc.) was explicitly ruled out' (Treverrow, P. and Volmer, D. A., *Rapid Commun. Mass Spectrometry*, 2012, **26**, 399). According to the authors, it was really 'a report from the UK Treasury' which 'suggested a more economical approach for the future of funding: the use of metrics'. Clearly, the bureaucrats who held the purse strings had sensed an opportunity to measure that one parameter so dear to the hearts of many economists and policy makers – the cost–benefit ratio. The UK's assessment panels continue to be advised that 'metrics' should not be indiscriminately used, but the virus has begun to spread. Further, as the authors point out, 'peer panelists in this epoch of metricisation are not blind to the mechanics of publishing or the shortcomings of ranking mechanisms'. While this may be true of UK panels, peer review committees in India are generally innocent of the dangers of blindly using journal metrics. A curious point to emerge in the UK analysis is that impact factor rankings 'play a definite role in the selection of publications by researchers and department heads', which are presented to the Research Assessment Exercise committees.

Treverrow and Volmer are concerned about the effects of 'impactitis' on professional journals which cater to distinct fields of science. Authors necessarily target high impact journals, which appear to be excessively weighted in assessment exercises. The same article appears to be of different quality if it were published in *Nature* as compared to any specialist journal. This misperception is based on the wrong notion that all articles contribute equally to determining a journal's impact factor. They cite a published study which provided an example of *Nature* for the year 2004, where 'a quarter of the articles resulted in 89% of the citations'. The inescapable conclusion which holds good in almost all cases is that a vast majority of the articles received fewer citations than reflected by the impact factor. Another illustrative example is provided by 'an analysis of all journals in the spectroscopy category contributing to the 2008 impact factor of the host journals'. In this case, '50% of journal citations are attributed to 11% of articles, 96% of the citations are from 50% of the articles, with 44% of the articles remaining uncited'. These distributions were repeatedly emphasised in the early discussions of impact factors, with Garfield himself sounding many a warning on the uncritical use of journal metrics in assessment of scientific output. The impact factor has proved resilient and acquired a special importance as the virus of 'impactitis' spreads worldwide. Even as authors rush to submit their work to high impact general journals, *Nature* and *Science* pre-eminent among them, the journals themselves often do a poorer job of peer review as compared to the 'community' or specialist journals. A provocative commentary entitled 'Bibliometrics as weapons of mass citation' notes that 'it is not rare to read papers in *Science* that are as muddled in their argumentation as spectacular in their claims' (Molinie, A. and Bodenhausen, G., *Chimia*, 2010, **64**, 78). The recent controversy over the 'arsenic bacteria' is a clear case in point.

The race to publish is becoming increasingly more competitive as the worldwide community of scientists grows rapidly. Publication counts are routinely used to compare the state of science in different countries. The pressure on scientists to publish their work has been recognized as a clear business opportunity by publishing houses. Scientific publishers constantly expand their stable of journals. As library costs escalate, the pressure to ensure easy access to the results of publicly funded research has grown. The 'open access' movement, which began by promoting self archiving and institutional repositories is now suddenly dominated by a growing number of journals which transfer the costs of publication to authors, while allowing readers free access. These costs can be considerable. For example, the *PLoS ONE* charge of $1350 per paper might put the journal out of reach for a majority of authors in Indian institutions. Papers in journals of this type are peer reviewed and judged not on potential impact or importance, but on scientific validity. The result is a growing volume of published papers. New open access business models are emerging. *Peer J*, a new journal, offers authors a deal that many may find hard to refuse; pay $299 for unlimited open access publications and submissions. For more limited numbers and to cater to impoverished authors, packages are available for $199 or $99. While each author on a multi-author paper must be a paying member there are concessions. Papers with more than a dozen authors need only 12 paying members (Van Noorden, R., *Nature*, 2012, **486**, 166). The drive to 'open access' has spawned a new breed of publishers who appear to be exploiting the author pay model. Jeffrey Beall of the University of Colorado has compiled a list of 'Predatory Open-Access Publishers' for whom gullible authors, attempting to publish articles rejected by mainstream journals, are easy prey (*The Charleston Advisor*, April 2010, pp. 10–16). Beall's 2012 compilation, available on the internet, lists among others Bentham Open which is described as a 'scholarly vanity press'. There is also a watchlist which includes Hindawi. Both these enterprises bombard scientists with e-mails to contribute as authors or editors for a rapidly growing stable of journals.

Impact factors and publication counts are the dominant metrics used for assessing individual scientists and their institutions. The assessors may do well to understand the limitations of journal metrics and the dynamics of scientific publications.

P. Balaram

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EDITORIAL

Animal Spirits: A Powerful Prescription

Economists have a way with words. Adam Smith's 'invisible hand' and John Maynard Keynes' 'animal spirits' seem to guide economies. Science can lay claim to few phrases which are comparable in their imagery. Even 'survival of the fittest', a phrase apparently first used by Herbert Spencer to explain Darwinian natural selection, has been used (and misused) more in discussions of economics. Prime Minister Manmohan Singh's recent exhortation to the guardians of government finance, to infuse the 'animal spirits' to revive a comatose and sinking economy, was a Keynsian call to action. For a naïve reader of the daily news, and I am one, the phrase 'animal spirits' immediately conjured up visions of a cocktail containing unmentionable ingredients which when administered to an ailing patient results in a miraculous restoration to full health and vigour. The Prime Minister was, of course, going back to his academic roots in recalling one of Keynes insights: '…there is the instability due to the characteristic of human nature that a large proportion of our positive activities depend on spontaneous optimism rather than mathematical expectations, whether moral or hedonistic or economic. Most … of our decisions to do something positive, the full consequence of which will be drawn out over many days to come, can only be taken as the result of animal spirits—a spontaneous urge to action rather than inaction…' (Keynes, J. M., *The General Theory of Employment, Interest and Money*, Macmillan, 1936, pp. 161–162). Economics is rarely a mood elevating subject; in today's context it can be distinctly depressing. Ironically, the way out of both an economic and psychological depression may be to use one's last reserve to summon up the Keynesian 'animal spirits'.

The prolonged economic slowdown in the West and the runaway growth in China have engendered a great deal of discussion in recent times. America's long unchallenged preeminence in science and technology may be under threat as Chinese science expands dramatically. The rise of global competition and its effects on America's unrivalled position as the driver of world science and technology was first addressed in a National Academies of Science report in 2005. Five years later, as the economic crisis deepened, a second warning was sounded by the US Academies (*Rising Above the Gathering Storm Revisited*, National Academies Press, 2010; *Current Science*, 2011, **100**, 443). This report and its predecessor made a powerful case for enhancing government support for the scientific enterprise in the United States, even as the recession deepened. The bogey of US leadership in the arena of science and technology being eroded, as other countries, most specifically China, enhanced their investments in science, was raised effectively. The Keynsian approach to stimulating an economy in recession would argue for infusion of public investments in key areas; science and technology which are the drivers of innovation and growth were projected by the US Academies. *Is American Science in Decline*? is a book, that crossed my desk, which answers its title question with 'a qualified no' (Xie, Y. and Killewald, A., Harvard University Press, Cambridge, Massachusetts, 2012). The authors are sociologists at the University of Michigan and Harvard University. The authors' thesis, that the health of American science is largely satisfactory, is based on an analysis of hard statistical data. The Academies' report and others that criticised them have led to a public debate that has proved instructive. Xie and Killewald marshall their facts well, producing a thesis which should draw the attention of policy makers worldwide.

The 20th century began with all the major centres of science being located in Europe. By the end of the century, which could well be labeled an 'American Century', the leadership of science had shifted decisively to the United States. Xie and Killewald recall a Japanese historian's prediction in 1962 that 'the scientific prosperity of the USA begun in 1920 will end in 2000'. They note, with some satisfaction, that 'American science is still going strong today'. The figures they present to buttress their assertion are striking. The United States with about 5% of the world's population accounts for '45 per cent of the world's Nobel Prize winners in physics, chemistry and physiology or medicine, through year 2009'. The US also accounts for '35, 49 and 63 per cent, respectively of the world's scientific publications, citations and highly cited publications'. For a very long time the US university system has been the envy of its competitors, accounting for '85 per cent of the world's top universities and 54 per cent of the world's top 100 universities'. The US presence as a leader in developing new technologies is substantiated by its share of '38 per cent of patented new technology

inventions by the industrialized nations of the Organization for Economic Cooperation and Development (OECD)'. This enviable position has been reached with '40 per cent of total research and development spending'. Despite this apparently impregnable position, a sense of unease seems to have pervaded the science policy establishment in the US. The China factor is now an obsession with science policy makers everywhere; India is no exception.

In considering the present and imagining the future, the past can sometimes provide insights. Xie and Killewald provide an absorbing account of the evolution of science in America. Science as it is practiced today traces its origins to Europe beginning with Copernicus in Poland in the 16th century, Galileo and Kepler in the 17th and Newton in the 18th century. France was a hub in the early 19th century, while Germany decisively dominated science until the misadventures of the 20th century. Britain fostered the tradition of the amateur scientist, with the transition to professional science becoming evident in the 20th century. 'The integration of science into universities' was first achieved in Germany, when 'research became not only legitimate but indeed a vital activity of a university professor'. American science developed, in Xie and Killewald's words, as 'two separate but interwoven strands: "pure science"… and "applied science" '. The latter's origins are traced to the fact that 'colonists and settlers struggling to survive in harsh new wilderness environments often had to find creative ways of making do with whatever materials were at hand… Rather than working to develop models of how the universe worked, American inventors such as Franklin, Bell and Edison worked independently to solve practical problems including fireproofing buildings from lightning, communicating over long distances and illuminating houses after dark.' The demands of colonial inventors for patents resulted in the American Constitution, framed in 1789, empowering the federal government 'to create a patent system' that 'allowed inventors to make handsome profits from their creative work and contributed to the unprecedented level of innovation in the late eighteenth and nineteenth centuries'. The founding of the Johns Hopkins University in 1876 and the University of Chicago in 1891 marked the beginning of the professionalization of science. 'Both schools explicitly incorporated the German model, but with one important difference: they contained research oriented graduate schools.' The authors note that these schools served as models for both private universities like Harvard and Yale and, 'somewhat later, large public universities such as the University of Michigan and the University of California'. Intriguingly, the Johns Hopkins model of a research oriented graduate university attracted the attention of J. N. Tata when he conceived of setting up the Indian Institute of Science, in the last decade of the 19th century.

The American university system has not really lost its edge in the 21st century. It remains as a model that many countries envy, but are unable to replicate. Xie and Killewald do not subscribe to the many alarmist reports that ominously forecast a loss of American supremacy in science. They quote the White House Science Advisor John Holdren: 'We can't expect to be number one in everything indefinitely.' Their conclusion is sensible: 'If America loses its status as the dominant leader of the scientific world, this could merely mean that science in other countries is getting better, not necessarily that American science is getting worse.' The many recent analyses of scientometric indicators across countries often fail to recognize that upward mobility cannot be indefinitely sustained and, at times, may provide a misleading indicator of the health of a country's science and technology enterprise. The frequently expressed concern that American school education is deficient in mathematics and science instruction is misplaced, according to Xie and Killewald: '…there is no evidence that today's American school children are less prepared than their counterparts of three decades ago to enter advanced training in scientific fields.' They address another oft debated issue. Is there an impending shortage of scientists and engineers in America or is there an oversupply of Ph Ds, 'indeed a glut of scientists'? They reach an 'overarching conclusion' that while 'the labor market situation for scientists in the United States has remained healthy in recent decades, especially at the bachelor's and master's levels … there may be reasons for concern among young doctoral scientists and engineers who aspire to be academic researchers.' The alarmist Academy reports on the decline of science have been influential, resulting in a 'large infusion of federal funds into scientific research as part of the American Recovery and Reinvestment Act of 2009'.

Reading Xie and Killewald's fact filled analysis, I could not help wondering whether there are lessons to be learnt and issues to be considered as the educational and research enterprise expands in India, even as the economy shows signs of fragility. There is little by way of informed debate based on reliable statistics. Planning and implementation are widely separated activities, with little guarantee that adequate fund flows will indeed be available over several years. Institutions are being recklessly created with populism and emotion being the driving forces. Growing institutions have been quickly shackled by restrictions on faculty recruitment, as the bureaucracy begins to apply the brakes on an expansion plan that may be financially unsustainable. The mindless calls to increase the number of Ph Ds produced do not address the issues of growing unemployment and underemployment of doctoral candidates emerging from our institutions. Recent downsizing of multinational R&D establishments in India will accentuate this problem. The science funding mechanisms show little signs of changing with the times. Science policy and governance may also be in need of a strong dose of 'animal spirits'.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Men and Microbes: War and Peace

At the turn of the century and the millennium, *Science* carried a series of essays titled *Pathways of Discovery*. An extremely readable and characteristically eloquent essay on *Infectious History* still holds a place, albeit indistinct and receding, in my memory. The author, Joshua Lederberg, was one of the most influential figures of 20th century biology and a writer of rare scholarship. In his essay, Lederberg traces the history of the relationships between men and microbes; a long history of conflict, with microbes invariably being associated with 'germs' that spread contagious disease. The 'Black Death' of the 14th century, when bubonic plague swept through Europe, and the rising tide of syphilis in the 16th century provide Lederberg the starting point for his reflections on our evolving understanding of the relationships between microbes and the environment (Lederberg, J., *Science*, 2000, **288**, 287). Viruses, bacteria, fungi and several unicellular parasites, plasmodia, the causative agents of malaria, among them, have for long been associated with disease. The war on infectious disease has had a simple objective; eliminate, by any means, the organisms that prey upon man. Microbes are, in Lederberg's words, 'moving targets'; natural variation, mutation and selection resulting in an ever changing enemy. But variation and adaptation are also at work in human hosts, over the long timescales of evolution. Many studies, most notably on malaria and tuberculosis and more recently on AIDS, illustrate the connection between infection and evolution. Lederberg is quick to point out that 'one lesson to be gleaned from this coevolutionary dynamic is how fitful and sporadic human evolution is when our slow and plodding genetic change is pitted against the far more rapidly changing genomes of microbial pathogens'.

My thoughts turned to microbiology when I was struggling to recover from a severe stomach infection. I was told cheerily, by an unsympathetic and irreverent gastroenterologist, 'Don't worry, bear it. The microbiology of your gut is changing. You will soon adjust to a new microbiome.' Having viewed with some distaste the spread of the suffixes 'ome' and 'omics' in biology, I was less than pleased that the contents of my gut now included a 'microbiome', a term describing the diverse population of organisms that inhabit the surfaces and recesses of our bodies. The 'omics' contagion began with 'genomics' in the 1990s, quickly spreading to include 'proteomics', 'metabolomics' and 'lipidomics' amongst others. In each case the term is intended to encompass all the constituents in a biological system; genes, proteins, metabolites or lipids. The 'microbiome' then evokes an image of a teeming multitude of organisms, too small to be seen, diverse in their chemistry and biology, that have colonised us with impunity. Peaceful coexistence of animals (humans amongst them) and microbes is a hallmark of biology. Our preoccupation with infectious organisms (pathogens) has often clouded our perceptions about microorganisms. Indeed Lederberg notes that 'during the early acme of microbe hunting, from about 1880 to 1940, however, microbes were all but ignored by mainstream biologists. Medical microbiology had a life of its own, but it was totally divorced from general biological studies. Pasteur and Koch were scarcely mentioned by the founders of cell biology and genetics. Instead bacteriology was taught as a speciality in medicine, outside the schools of basic zoology and botany. Conversely, bacteriologists scarcely heard of the conceptual revolutions in genetic and evolutionary theory'. Bacteriology had indeed captured the public imagination in the early years of the 20th century as it became clear that the enemy in the war on infectious disease had been identified and that a counter attack with vaccines and chemotherapeutics could now be mounted. I might add, parenthetically, that when my own institution, the Indian Institute of Science, was conceived over a century ago, a 'department of bacteriology' was envisioned and a 'school of tropical medicine' appeared a distant dream. Both did not materialize for reasons that future historians may find intriguing, despite their obvious relevance to India, then and now.

The early decades of the 20th century were a witness to the growing importance of microbiology in medicine. Generations of researchers grew up reading Paul de Kruif's 1926 classic, *Microbe Hunters*, which remains a compellingly romantic account of the golden age of bacteriology and microbiology. On the pages of this book, one encounters with a bewildering swiftness the dramatic transformation of microbiology, the birth of immunology and vaccinology and the first triumphs of medicinal chemistry as the author traces the work of Leeuwenhoek, Pasteur, Koch, Metchnikoff, Roux and Behring, Ross and Ehrlich. Should a book on science written in 1926 be recommended nearly ninety years later? Excerpts from an

introduction to a reissue of the book in 1996 provide an answer. 'It is thus no small praise to say of Paul de Kruif's *Microbe Hunters* that, well over half a century after it first saw the light (1926), it manages to delight, and frequently to entrance, old and new readers – those who have kept a more or less blurry recollection of its pages from adolescence … and those for whom its vivid images and portrayals are a fresh experience…. Survival of this magnitude is uncommon for any book, but especially for one aptly regarded as a work of "scientific popularization" and that deals with facts and personalities whose description has been reiterated ad nauseam' (Gonzalez-Crussi, F., Introduction to the 1996 reissue of *Microbe Hunters*, Harcourt Brace & Company, Florida). De Kruif's narrative style may be a part of the magic which has sometimes drawn student readers to the practice of science. Gonzalez-Crussi quotes de Kruif: 'A scientist, a really original investigator of nature, is like a writer, or a painter or a musician. He is part artist, part cool searcher.' Is science, especially a subject of such practical utility as microbiology, a product of the creative imagination of those who have advanced the field? De Kruif provides a simple answer; one which merits some reflection: 'Great advances of science so often start from prejudice, on ideas got not from science, but straight out of the scientist's head, on notions that are only the opposites of the prevailing superstitious nonsense of the day.'

Modern genomics has unified biology. The founders of bacteriology, who so vividly come to life in de Kruif's book, may not have been really surprised by the resurgence of their disciple, albeit in a vastly altered form, with the recent excitement on the analysis of 'human microbiomes'. Attention has been focused on an entirely different aspect of the relationship between microbes and man; their necessity to coexist. The *Human Microbiome Project* has subjected the microbial populations found in different niches in the human body to a detailed scrutiny using DNA analysis, specifically a taxonomic marker the 16s rRNA gene, employing the recently developed techniques of 'next generation' sequencing to probe the 'structure, function and diversity of the human microbiome' (The Human Microbiome Project Consortium, *Nature*, 2012, **486**, 207 and 215). The methodology used permits a sampling of organisms 'from the skin, nose, mouth, throat, vagina and faeces (to represent the gastrointestinal tract)' using DNA samples from '242 healthy adults in the United States'. The analysis of the marker molecule in 5177 samples permits a broad characterization of the nature of the organisms that colonise specific sites in the human body. David Relman in a News & Views piece notes that 'studying the human microbiome has so far been an exercise in humility' (*Nature*, 2012, **486**, 194). Microbiome analysis will be confronted with many issues in the future as individual habitats vary widely and the selective pressures of the environment can be vastly different. The microbiome will be a dynamically changing, evolving community of invisible colonisers. Relman notes that 'many areas of human microbiome research warrant further investigation, but viruses and small non-bacterial organisms such as fungi deserve special attention, as do questions regarding the functions of the microbiome. We are essentially blind to many of the services that our microbial ecosystems provide – and on which our health depends – and investigators desperately need new approaches for studying interactions between members of the microbial community and their human hosts'. Indeed, the *Nature* reports suggest that 'some of the microbiome's functions are likely to be performed by rare community members or to involve genes that are expressed at low levels', a feature that 'will complicate attempts to decipher their influence' (Relman, D. A., *Nature*, 2012, **486**, 194). The gut microbiome is rapidly emerging as the community which is being most extensively investigated, with microbial ecology and evolution of the human–microbe symbiosis attracting great attention (*Science*, Special Section, 8 June 2012). At birth, the human gastrointestinal tract is sterile, slowly acquiring a stable and valuable microbial community that vastly enhances the gene complement that produce the protein catalysts that perform the necessary chemistry. This process appears analogous to the strategy adopted by corporations which enhance their competitive fitness by mergers and acquisitions. A recent review concludes, rather sombrely, drawing a connection between the evolution of human lifestyles and chronic disease: 'It is almost inevitable that the characteristics of the modern lifestyle, such as antibiotics, Caesarian sections, hygiene, refined diets, formula feeding and small households, would introduce hurdles into symbiont transmission with consequences for the functional development of the gut microbiota. This is one way in which the modern lifestyle might contribute to the rise in chronic diseases recently associated with the gut microbiome in westernized societies' (Walter, J. and Ley, R., *Annu. Rev. Microbiol*., 2011, **65**, 411).

The human microbiome may soon be dwarfed by an attempt to map the *Earth Microbiome*; an effort to understand the most populous, but invisible, inhabitants of our planet. This enterprise may turn out to be a new mega project for biology which will generate vast amounts of data. Turning that flood of information to useful knowledge will be a major challenge (Hood, L., *Science*, 2012, **336**, 1209). Lederberg anticipated a great deal of current discussions on the microbiome when he argued that we must abandon 'our manichaen view of microbes – we good, they evil'. He underscored the need for abandoning the 'metaphor of war' in favour of 'a more ecologically informed metaphor'. The human microbiome project is a major step in transforming our view of the relationships between men and microbes. The metaphor of war may soon be replaced by the more informed metaphor of peaceful coexistence.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Cancer Research: Collaboration and Collegiality

Cancer biology is a complex field. Despite decades of intense research, the war on cancer may well extend far into the 21st century and, maybe, even beyond. The search for new targets for attack by chemotherapy is currently an intensely competitive field of research. An increasingly sophisticated range of tools are being deployed, in an area where the view from the trenches appears grim. Issues of toxicity, efficacy and resistance seem to quickly dull the promise of new drug leads. Evolution by natural selection can be a double edged sword, which favours the hardier, more adaptable tumour cells to score over their normal counterparts. The handful of molecules that emerge into the clinic, after years of development, are often priced so steeply that few can afford the exorbitant costs of treatment, that often offers only a temporary reprieve. The problems of new drug development are encountered in all human disease. As regulatory processes become more rigorous, new molecular entities that appear promising often fall by the wayside. The pharmaceutical industry is research intensive, with few companies having the deep pockets necessary for long term investment. Molecules often fail in the final steps of the drug development process, when a significant proportion of the research and development costs have already been incurred. Unsurprisingly, companies factor in the continually rising costs of research while pricing any successful molecular entity that emerges as a marketable drug.

Having often been urged by naïve peer review committees at government agencies to push basic, academic research in chemistry and biology towards 'drug design', I received, with some enthusiasm, a notice for a public lecture arranged by the Indian Academy of Sciences. The talk, delivered with great elegance, by Ashok Venkitaraman, a distinguished cancer biologist from Cambridge, described the promise of a broad interdisciplinary approach towards identification of specific targets, which would open the road towards a new generation of cancer chemotherapeutics. Molecular oncology, genetics, cellular and structural biology appeared to merge seamlessly, in this strategy to subdue cancer. The elegance and sophistication of new approaches that integrate chemistry and biology, 'chemical biology', sometimes obscures an old cautionary note that was once familiar in the research laboratories of pharmaceutical companies: 'Success in the laboratory does not always translate into success in the marketplace.' Drug research requires a hugely organized effort, with effective collaboration between scientists with widely differing backgrounds. Venkitaraman highlighted this essential feature by first quoting Andrew Grove, a cofounder of Intel and later its CEO: 'Success breeds complacency. Complacency breeds failure. Only the *paranoid* survive.' While this may be true in the harshly competitive world of high technology, Venkitaraman suggested that Grove's dictum might be modified for the arena of drug research: 'Only the *collegiate* survive'. I was struck by the word 'collegiate'. It seemed to convey, eloquently, the need for cooperation and collaboration within an interdisciplinary team. Shared goals, complementary skills and collective responsibility seemed to be described by a single word. I could not help wondering if collegiality might indeed be the single most important quality necessary for success, in a modern, interdisciplinary research environment in almost any field of science.

Thinking about collegiality, I turned, almost inevitably, to sports and the London 2012 Olympics. As I write it is clear that the Indian contingent will return with a handful of medals, all won in individual events. Our performance in the team events, notable among them tennis and hockey, was singularly disappointing. Most regrettably, the tennis stars displayed a remarkable lack of collegiality, while newspaper reports suggested that the hockey team had several individualists who had scant regard for team strategies. While individual brilliance and commitment served lone performers well, the team sports demanded a collaborative effort, which floundered in the absence of collegiality. Science is increasingly resembling a team sport. In most areas of modern science teams of researchers with diverse specializations are required to address the major issues of interest. 'Big science', exemplified by the hunt for the Higgs boson by the physicists or the large genomics programs in biology, requires collaborative efforts on a scale never seen before. Hundreds of authors and institutional affiliations are listed on published papers in the areas of particle physics and genomics. These are, of course, extreme examples of collaborative efforts. However, even a cursory glance at any of the major journals of science will reveal very few papers which do not list many authors; testimony to the fact that modern science requires organized teams in order to succeed in a complex and competitive environment. Multiauthor

papers, on an average, appear to attract greater attention than the contributions of lone scientists, suggesting that multidisciplinary articles may have a greater impact as measured by the metrics commonly used to quantitatively assess scientific output. Successful collaboration requires a great deal of understanding and mutual respect between the researchers involved. If these conditions are not met, unpleasantness and controversy follow. Sharing of credit also implies a sharing of responsibility for the published results of a scientific study. This is often rendered difficult by the nature of research, where highly focused specialists contribute to a specific aspect of a multidisciplinary study and invariably rely on the expertise of collaborators to ensure the correctness of results and interpretations, that lie outside their areas of competence. Collegiality and trust become essential ingredients of a successful collaboration. In concluding his overview of the new approaches to finding the next generation of cancer chemotherapeutics, Venkitaraman emphasized the virtues of a specific target driven approach that might then be attacked by the proverbial 'magic bullet'.

Cancer research, in recent times, has provided an excellent example of a target driven strategy in the development of Gleevec (Glivec), as a means of treating a relatively common form of blood cancer, chronic myeloid leukemia (CML). The drug is in the news in India, where non-governmental organizations have led a legal battle against the patent protection sought by Novartis on its best selling product. The issue has been in the courts since 2007 and is expected to be decided by the Supreme Court in the very near future. A judgement against Novartis would legitimize the production of cheap generics by Indian companies, dramatically reducing treatment costs which may run as high as $40,000 to $98,000 annually (**swissinfo.ch**, 12 March 2012). With estimated annual sales revenues of $4.7 billion from Gleevec, Novartis is indeed keen to avoid competition from cheap generics. The original rejection of a Gleevec patent in 2006 has been challenged. The Supreme Court's verdict is likely to have far reaching implications for the pharmaceutical industry in India and for the difficult arena of affordable health care. Pharmaceutical companies which invest heavily on research and development jealously guard successful molecules by evolving protection strategies beyond the date of expiry of the original patent. 'Evergreening' is an accepted industry strategy, whereby minor modifications of an existing molecule or formulation are used to file secondary patents, thereby extending the lifetime of monopoly protection. Gleevec's primary patent was never filed in India in the pre-TRIPS era and the secondary patent is the subject of the present dispute. The pharmaceutical industry, more than any other, illustrates the difficulty of finding means 'to balance innovation and access' (Sampat, B. N. *et al.*, *Science*, 2012, **337**, 414).

The science behind Gleevec highlights the pivotal role that basic biological understanding of a disease process may play in developing therapeutics. The key events in understanding the underlying molecular aberrations in CML may be traced to cytogenetic work in the 1950s, that established chromosomal abnormalities, eventually leading to the identification of the 'Philadelphia chromosome' in the 1970s. Over a decade was to pass before a gene was identified as a 'likely molecular culprit' in CML. Not long afterword the gene product was identified as a tyrosine kinase, an enzyme which initiates a signalling cascade, promoting cell proliferation, a characteristic of cancer. By 1990 a target for inhibition, a specific tyrosine kinase, was at hand. By this time, a growing body of structural information suggested that, contrary to prevailing opinion in the pharmaceutical industry, these enzymes may indeed be good targets for attack. The 2009 Lasker–DeBakey Award for Clinical Medical Research recognized the work of Brian Druker, Nicholas Lydon and Charles Sawyers for 'converting a fatal cancer into a manageable chronic condition'. Gleevec was the agent of management. Having developed a cell based laboratory assay for assessing the enzyme activity, Druker turned to Lydon – a researcher at Ciba-Geigy (later Novartis) – who dug into the company's vaults of compounds. Together with Sawyers, clinical trials were soon conceived for the most promising molecule, finally christened as Gleevec (Pray, L. A., *Nature Education*, 2008, **1**(1)). The Gleevec story, like many others in drug research, includes the development of resistance to a successful product, as cancer cells learn to evade attack by presenting an altered target. A structural understanding of the new target has already permitted development of a modified molecule, this time by Britsol Myers Squibb, *Sprycel*, which targets Gleevec resistant cancers. The story of Gleevec is an example of a rare success in the war on cancer (http://www.lasker-foundation.org/awards/2009). Drug development to combat CML has required contributions from many diverse disciplines and has been made possible by a fundamental understanding of the molecular basis of the disease. This success is a consequence of a readily identifiable target in the case of CML. Unfortunately, targets are hard to identify and attacking the wrong targets can be a fruitless exercise.

Listening to the Academy lecture on cancer research and reading about Gleevec, reinforced a growing feeling that only a broad interdisciplinary assault on the many problems of biomedical research can lead to results that can be effective in improving health care. Such an approach may require academic laboratories in India to examine ways of creating a new ethos that promotes collaborative research. Academic institutions and partners in industry may do well to ponder on another of Andrew Grove's prescriptions: 'A corporation is a living organism; it has to continue to shed its skin. Methods have to change. Focus has to change. Values have to change. The sum total of these changes is transformation.' Replacing paranoia by collegiality may indeed be the way forward.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Slowing Down

Age dulls the reflexes; slowing down seems inevitable. Nowhere is this more evident than on Indian roads, where both driving and walking seem hazardous, especially as the years advance. Nimbleness and lightning fast reflexes, invariably attributes of the young, seem necessary for survival on the roads of our cities and towns. Even placid academic campuses are rapidly being overrun by speeding vehicles, which hurtle along narrow roads. On my own campus I have often suppressed an urge to shout 'slow down' to drivers who seem to be in a great hurry in their apparently mindless desire to rush from place to place. There was a time when the pace of life was distinctly slower and a career in academic science proceeded at a leisurely pace, which seemed conducive to both study and reflection. Over the last few decades the pace has distinctly quickened, competition is more intense and the international scientific scene appears immensely crowded; at times resembling our streets where people appear to be rushing in every direction, often oblivious of their surroundings. The internet and instant connectivity have contributed greatly to speeding up communication and transmission of information, resulting in a situation where the average scientist can barely cope with the volume of information available in every discipline of science. Even as I have struggled to keep abreast of areas of science that interest me, the realization has dawned that the battle will eventually be lost. At times, I have wondered whether this feeling of inevitable defeat is only a result of aging or whether it is a feeling shared by others. I was, therefore, greatly heartened to stumble across an essay entitled 'Slow science', which asks 'whether chemists should slow down' (Lutz, J.-F., *Nature Chemistry*, 2012, **4**, 588).

Lutz's essay is bound to strike a chord in older scientists who are uncomfortable with the headlong rush of modern science. Using Charles Goodyear's painstaking researches of the 1840s, which led to vulcanized rubber, as an example, Lutz notes that 'many breakthroughs of the nineteenth and twentieth centuries were made by one researcher – or a small group of dedicated researchers – after many years of patient investigation punctuated by rare, but meaningful publications'. The author draws attention to 'the recently founded Academy of Slow Science', which 'emphasizes that time, failure, reading and thought are needed to conduct relevant research'. Encouraged by the fact that I had time on my hands and adequate experience of failure, I moved to the website of the Slow Science Academy, a task made easy by the power of *Google*. The Academy's 'manifesto' (a grand word that reminded me of Marx and Engels) begins captivatingly: 'We are scientists. We don't blog. We don't twitter. We take our time.' As one who does not blog or twitter, I read on. Was this an emerging group of European Luddites, striving hard to deny the power of modern communication technology? Or was this a group of aging scientists, like myself; naïve innocents cast adrift in the turbulent sea of technological change? Reading further, I realized that the new Academy was not espousing the cause of returning to a technologically simpler age. Rather, they seemed to say '*yes* to the accelerated science of the early 21st century; … yes to the constant flow of peer reviewed publications and their impact; … yes to science blogs and media and PR necessities; … yes to increasing specialization and diversification in all disciplines'. What cause was this new Academy espousing? They are in fact arguing for a 'slowing down' of the pace at which much of science is proceeding, even as the race for results, publications, rewards and recognition intensifies. They appear to hark back to an earlier era: 'Slow science was pretty much the only science conceivable for hundreds of years; we argue, it deserves revival and needs protection'. Their arguments may find an echo in the minds of those who are mildly uncomfortable with the changing ethos of science: 'Science needs time to think, Science needs time to read, and time to fail. Science does not always know what it might be at right now. Science develops unsteadily with jerky moves and unpredictable leaps forward – at the same time, however, it creeps about on a very slow timescale, for which there must be room and to which justice must be done'. The new Academy reiterated its commitment to the world of social media by asking readers to express their 'sympathy on *Facebook*'.

Why is there this concern about the accelerating pace of science; a worry about overspeeding, leading to this call to slow down? In his column in the August issue of *Nature Chemistry*, Jean-Francois Lutz raises the provocative question whether chemists publish too much. He notes that 'scientists do not have anything close to the amount of free time that would be necessary to read all of

the literature in their field of research, even in specialized areas'. This is certainly true as the number of journals in chemistry and related areas has increased dramatically in the last few years, making it impossible to 'read' all the relevant literature. This situation is not unique to chemistry. Indeed the explosion of biological science literature, with biomedical research being the major contributor, has ensured that it is impossible even to scan the abstracts of all published paper even in a limited area. Lutz asks: 'Is … all of this literature … necessary.' The answer is clearly, no. However, there appears no way by which the flooding of the literature with repetitive, fragmentary and mediocre studies can be avoided. Scientists must publish in order to further careers. Institutions and countries are judged by the quantity of their scientific output. Quality and utility are harder to assess. It is indeed impossible to return to the more deliberate pace of the first half of the twentieth century. Lutz echoes an oft stated impression that 'progress is now much more incremental than it used to be 100 years ago'. Does the 'fast' culture of contemporary science raise other concerns? After all, with some effort it might be possible to separate the wheat from the chaff even in these days of an ever expanding scientific literature. Lutz suggests, and many others before him have implied, that the current acceleration in the pace of science 'may also be a factor in the prevalence of duplicate publication, plagiarism, irreproducible results and fraud'.

The drive to accelerate the pace at which research findings are published has been most noticeable in China. Indian policy drivers constantly cite the phenomenal growth of scientific output, measured by the number of publications, that emanate from China. In contrast, the rate of growth of scientific output from India has been modest; almost resembling a casually strolling pedestrian breaking into a gentle jog. Acceleration, however modest, is often accompanied by the problems of fraud, plagiarism and duplicate publication that appears to be becoming common place worldwide. A more serious problem, and one that is harder to address, is the irreproducibility of results described in hastily written papers. The biomedical literature is a specific example of an area where extravagant claims are not uncommon. Indeed, publications in 'very high impact' journals almost necessarily require authors to exaggerate the importance and applicability of their findings. A provocative column drew attention last year to 'an epidemic of false claims'. The author, John Ioannidis, argued that the problem of 'false positives and exaggerated results… is rampant in economics, the social sciences and even the natural sciences, but it is particularly egregious in biomedicine' (*Scientific American*, June 2011, p. 16). The author traces the origins of the crisis to 'the public's rising expectations of science'. He notes: 'Being human, scientists are tempted to show that they know more than they do. The number of investigators – and the number of experiments, observations and analyses they produce – has also increased exponentially in many fields, but adequate safeguards against bias are lacking. Research is fragmented, competition is fierce and emphasis is often given to single studies instead of the big picture.' Biomedicine is also beset with issues of conflicts of interest, where the dividing line between scientific integrity and commercial considerations can be exceedingly thin. Ioannidis is perceptive when he concludes that 'the oligopoly of high-impact journals also has a distorting effect on funding, academic careers and market shares'. Distorted presentation of scientific results, by 'pharmaceutical and biomedical device industries, which may sometimes design and report studies in ways most favourable to their products', is a matter of concern. More recently, the need to 'raise standards for preclinical cancer research' has been emphasized. 'Methods, publications and incentives must change if patients are to benefit' (Begley, C. G. and Ellis, L. M., *Nature*, 2012, **483**, 531). In an earlier essay, Ioannidis – writing in the context of clinical trials – asserts that 'most published research findings are false'. This is an area where both study design and analysis of results are likely to be influenced by prevailing bias (*PLoS Medicine*, 2005, **2**, 0696). A later analysis emphasizes the distortion of science by current publication practices (Young, N. S., Ioannidis, J. P. A. and Al-Ubaydli, O., *PLoS Medicine*, 2008, **5**, 1418). The study uses an economic model to analyse science; treating scientific information as an 'economic commodity' and journals as 'a medium for its dissemination and exchange'. It must be disturbing to many to think of knowledge as a commodity being transferred 'from its producers (scientists) to its consumers (other scientists, administrators, physicians, patients and funding agencies)'. The commentary by Young *et al.* must be read by all those concerned with the role of journals in shaping and influencing science. Indeed, science today is necessarily viewed through the distorting prism of a stable of high impact journals.

The frenetic pace of modern science, often ruthlessly competitive, leaves little time for reading, thinking and even enjoying the practice of science. The transformation from an earlier and gentler age is evident in India. The art of grantsmanship must first be mastered by new investigators even as they set timetables for fellowships, awards and recognition, that have now become an essential catalyst to induce scientists to do what they should have normally enjoyed doing. The race for publication in high impact journals, the competition among colleagues and collaborators for listing as 'first authors' and the constant trumpeting of scientometric indices are signs of a profound and, apparently, irreversible cultural change. Science, even in India, is beginning to accelerate. The roads are crowded and the destination is largely unknown. Navigation through the traffic will be difficult in the years to come. For some scientists the pedestrian path may permit a gentle stroll, enjoying the sights and reflecting on the wonders of nature. For the majority, slowing down may be as difficult as it is for the speeding and reckless drivers on our roads.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Medical Research and Education: Lessons from Osler

Conferences in India invariably begin with formal and flowery inaugurations. One of the hazards of holding a visible official position is that organisers, hard pressed to find 'chief guests' quickly narrow down their search to readily available functionaries. It is inadvisable for scientific conferences to invite politicians and excessively busy public figures, who often do not appear on time, throwing carefully choreographed programs into disarray. Academics are a safer bet. They are decidedly more punctual, often less demanding and almost as voluble as politicians; with the decided advantage of occasionally even having a nodding familiarity with the subject of the conference being inaugurated. I have, over the years, done more than my fair share of inaugurations; most fading rather quickly from memory. The opening formalities at medical conferences have, however, left a lasting impression. Medical practitioners and professionals usually gather for large conclaves in which the pharmaceutical industry and manufacturers of medical equipment hold impressive exhibitions. These are also occasions when many distinguished practitioners of medicine and surgery, with stellar reputations, congregate, albeit briefly. I share with most ordinary people a very healthy respect for the remarkable services rendered by the best amongst our physicians and surgeons. Patients in pain and distress, and I am sure every reader would have an experience to recall, can look back with gratitude on encounters with dedicated practitioners of medicine. This is indeed a profession whose origins bear a strong imprint of pioneers, driven by a desire to understand disease processes, develop strategies for control and prevention and to contribute in every way to the alleviation of human suffering. In recent times, the profession of medicine has lost some of its sheen in public perception. The rising costs of health care, the looming presence of insurance companies, and the influence of large pharmaceutical conglomerates in the promotion of new drugs and in maintaining high levels of pricing have been subjects of concern in the United States. In India too, the prohibitive costs of specialist health care, the rise of corporate hospitals and the perilous state of medical education, overseen by regulatory structures that do not inspire confidence, have all contributed in some measure to a growing unease about the future evolution of medical research and education in the country. Academically inclined clinicians seem to voice these concerns more frequently and I have heard thoughtful calls for reform in many conferences.

In the West biomedical research has exploded over the last few decades, fuelled in large part by the revolution in the biological sciences. In India biomedical research is hardly a presence, with clinical research practiced only in a few isolated institutions. There is little or no concerted collaboration between clinical researchers and basic scientists who invariably work in different institutions. The concept of a medical school within a university has never found favour in India. Attempts to foster and promote research within our medical institutions have been limited. Understandably, there is little or no discussion on the need to restructure medical education to emphasize the value of research. In what can only be described as a cry in the wilderness, M. G. Deo argued for 'developing research oriented educational programmes both at the undergraduate and postgraduate level' (*Indian J. Med. Res*., 2009, **130**, 105). Unfortunately, even promotional programs with financial incentives like the Kishore Vaigyanik Protsahan Yojana (KYPY) scheme of the Department of Science and Technology have met with a complete lack of interest amongst students studying medicine. The Medical Council of India (MCI) has always had a stranglehold on medical education policies; its chequered past only ensures an uncertain future. In the United States, students enter medical school after completing a four year undergraduate program, during which the first seeds of interest in research are sown. In India, students enter immediately after high school and are exposed to a curriculum in which even the basic sciences barely merit much attention. It is unsurprising that there is little feeling for the importance of research in medicine.

Is research really a necessary component of medical education? In attempting to think about this question, I was greatly helped by a scholarly colleague who drew my attention to a recent commentary in *The Lancet* (2011, **378**, 1984), which addresses the problems of postgraduate medical education in England. The authors, Pawel Ovseiko and Alastair Buchan of the University of Oxford, remind readers of an address, published a century ago in *The Lancet*, by William Osler 'on the need for hospitals and universities to work closely together'. The authors' principal concern is for the 'ailing postgraduate medical system' in the UK. They appear more comfortable with

the situation of undergraduate medical education, which appears to be 'based on the latest scientific discoveries in laboratories and bedside teaching in hospitals'. The commentary strikes a positive note when the authors remark: 'Universities and hospitals work closely together in the National Institutes of Health Research (NIHR) biomedical research centres and units to translate scientific discoveries from bench to bedside.' The authors are critical of health reform proposals being considered in the UK, which would distance postgraduate medical education from universities and threaten 'clinical academic programmes that combine specialty training with research and teaching'. The importance of linkages between academic institutions and hospitals was recognized by William Osler, whom the authors quote: 'All agree that a study of the problems of disease and the training of men and women in the technique of the art come within the sphere of the university. England has suffered sadly from an absence of great medical faculties such as exist on the continent; and nowhere is this more evident than in the dissociation of the hospital from the university' (Osler, W., *The Lancet*, 1911, **177**, 211). In their recent commentary, Ovseiko and Buchan note with some satisfaction that 'seven generations of Regius Professors later, Osler's ghost would be delighted to see that great advances have been made in the past 100 years. English universities now have some of the best medical faculties in the world'. No sense of satisfaction is possible when surveying the scene of medical research and education in India, where the distancing of medicine from the broad umbrella of universities was complete a long time ago.

William Osler's influence on medical education and research in the West was far reaching. Beginning his career in Canada, he was one of the earliest members of the faculty of the newly founded Johns Hopkins Medical School in 1889. Interestingly, Johns Hopkins University was one of the first in the United States to emphasize research and was a model for J. N. Tata's conception of a 'University of Research' in India, which eventually materialised as the Indian Institute of Science in 1909. Ironically, in the early plans for the Institute a Department of Bacteriology was envisaged; undoubtedly influenced by the dramatic successes of Pasteur and Koch in understanding the biological basis of infectious disease. The department never materialised. Shortly after its founding, a proposal to establish a unit to study tropical medicine was mooted but did not find favour. Medicine and biomedical research was thus firmly excluded from the first major experiment in India to establish a research university. Over the last century, the evolution of medical education and research in India would have hardly met with Osler's approval. Osler's influence in the West stemmed from his great erudition and from his immensely popular book, *The Principles and Practice of Medicine* published in 1892. An essay commemorating the centennial of its publication notes: 'The multiplicity of literary and historical allusions that embellishes the textbook bears testimony to Osler's firm grounding in the classics and his knowledge of, and passion for, the history of medicine' (Golden, R. L., *Ann. Int. Med.*, 1992, **116**, 255). In his historical account, Golden recounts the influence of Osler's book on Frederick T. Gates, 'a member of John T. Rockefeller's philanthropic staff', who read it 'from cover to cover during his summer vacation', in 1897. Influenced by Osler's 'scientific candor and literary style', Gates wrote a memorandum to Rockefeller in which he 'aimed to show him the actual condition of medicine in the United States and the world as disclosed by Dr Osler's book'. Golden quotes Gates: 'Eminent physicians were consulted … and out of the wide consultation the Rockefeller Institute of Medical Research came into being. It had its origins in Dr Osler's perfectly frank disclosure of the very narrow limitations of ascertained truth in medicine as it existed in 1897.' Reading Golden's retrospective on Osler and his textbook, it is clear that his focus on pathology and his 'emphasis on morbid anatomy' and his therapeutic conservatism clearly bore testimony to his accurate assessment of the state of medical knowledge at the end of the 19th century. He quotes Osler's famous aphorism: 'The desire to take medicine is one feature which distinguishes man, the animal, from his fellow creatures.' Ehrlich and chemotherapy were yet to appear on the horizon when Osler wrote his book. Osler's firm conviction on the need for research, integrated with the teaching of medicine, has influenced the generations that followed, leading to a century of unparalleled progress in medicine. Yet, a sense of unease prevails even though we are well launched into a new century.

The dramatic advances in science have yet to translate into the kind of dividends expected from the large public investments in biomedical research in the West. The escalating costs of pharmaceutical research and the thin pipeline of potential new drugs has been widely discussed. Gene and stem cell therapies continue to promise but the road to success appears uphill and rocky. The situation in Indian biomedical research is grim. There are few connections between science, engineering and medicine. There is little visible enthusiasm for reform of medical education and research amongst the academic community. The enormous contributions made by advances in science, over the last century, to the practice of medicine are hardly appreciated in undergraduate medical courses, where neither science nor the history of medicine merit more than a cursory mention. The need to emphasise research in the medical curriculum must be deeply felt if any reform is to be expected. If this is not done the gulf between the sciences and medicine will become unbridgeable in the future. The recent *Lancet* commentary noted that 'Osler's call for hospitals and universities to work closely together remains as poignant today as it was 100 years ago'. They go on to quote Osler: 'The truth is we need an active invasion of the hospitals by universities'. Maybe there are still lessons that we can learn from Osler.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

DNA: Finding Uses for Junk

'Junk' is a common word. As an inveterate accumulator of unusable, unserviceable and useless material, including books, papers, gadgets and odds and ends, I am usually surrounded at the workplace and at home by large, untidy piles of junk. Throwing away things that cannot be put to good use seems a painful act; sentiment invariably overwhelms common sense. In our laboratories, junk accumulates rather quickly, as computers and equipment are replaced with an alarming regularity. The discarded items stand forlorn and abandoned in dark corners. Junk is distinguishable from garbage. The former accumulates in our homes and offices; the latter is disposed off outside, often resulting in unsightly and smelly piles when clearance mechanisms fail; a not uncommon occurrence in our ever expanding cities. The reasons why junk is not as readily discarded as garbage are not easy to enumerate; most often it is sheer lethargy. The energetic (and financial) costs of clearing junk often overweigh the costs of storage. Junk is a word that caught my attention a few days ago in a news report in *Science*, headlined 'ENCODE Project Writes Eulogy for Junk DNA' (Pennisi, E., *Science*, 2012, **337**, 1160). The provocation for this story, and many others that have appeared in the popular press, is the publication last week of as many as thirty papers announcing the results of one of biology's mega-projects, to establish the function of vast amounts of DNA in the human genome, which at first glance appear to be largely 'non-coding'. The sheer scale of the project, succinctly summarized in the *Science* report, should attract the attention of any casual observer of the scientific scene. A financial outlay of $288 million supported 442 researchers at 32 institutions worldwide, to carry out a genome wide survey of functional attributes of the entire genome length of over 3 billion nucleotides. In common parlance, this amounts to deciphering a biochemical significance for long stretches of genomic DNA, which previously were thought to have little or no specific functional role; barren wastelands of DNA that were termed as 'junk DNA'.

The term 'junk DNA' appears to have first surfaced in two articles written by the geneticist Susumu Ohno in the early 1970s, at a time when molecular biology was a young discipline and DNA sequencing was a technology that had not appeared over the horizon. Curiously, Ohno may have used the term twice in the titles of two conference presentations, which later appeared in printed form (Ohno, S., So much 'junk' DNA in our genome. In *Evolution of Genetic Systems* (ed. Smith, H. H.), Gordon and Breach, New York, 1972, pp. 366–370; Ohno, S., Evolutional reason for having so much junk DNA. In *Modern Aspects of Cytogenetics: Constitutive Heterochromatin in Man* (ed. Pfeiffer, R. A.), F. K. Schattauer Verlag, Stuttgart, Germany, 1973, pp. 169–173). A thoughtful commentary on the internet draws attention to the fact 'that most people who cite these papers have not read them'; often drawing the incorrect conclusion that 'non-coding DNA was totally unimportant' (Gregory, R. T., 2008; http://www.genomicron.evolverzone.com). This web posting reproduces an interesting discussion which followed Ohno's 1973 presentation. One of the participants notes that 'the word "junk" is a powerful word' and appears uncomfortable with a notion that large, conserved segments of DNA have no function. This is emphasized by another participant who adds: '…this DNA must have a functional value since nothing is known so widespread and universal in nature that has proved so useless'. The final interjection by a perceptive participant may strike a chord: 'Well there is an exception to that rule. A lot of us have permanent positions at the university but are considered by others (mainly by students) meaningless and of no utility whatsoever.'

The internet, an almost infinite source of information, hosts Ohno's first paper. Appropriately, the site on which the paper is displayed lists the 'archives of the life and death of junk DNA 1972–2008' (www.junkdna.com). Junk DNA's demise may be traced to the appearance, in 2007, of the preliminary results of the ENCODE project, which examined about 1% of the human genome (The ENCODE Project Consortium, *Nature*, 2007, **447**, 799). The impending disintegration of a 'gene centric' approach to genome analysis was proclaimed, once the results established that a great deal of 'junk DNA' was indeed transcribed into RNA and such sequences were 'evolutionarily conserved across mice and humans' (Pennisi, E., *Science*, 2007, **316**, 1556). Did 'junk DNA' as a non-functional, apparently useless element of genomes become an accepted view immediately after Ohno's presentations of 1972 and 1973? Ohno derived his arguments from the traditional viewpoints of genetics: 'The mammalian genome (haploid chromosome complement) contains roughly 3.0×10^{-9} mg of DNA which represents 3.0×10^{9} base pairs. This is at least 750 times the genome

size of *E. coli*. If we take the simplistic assumption that the number of genes contained is proportional to the genome size, we would have to conclude that 3 million or so genes are contained in our genome. The falseness of such an assumption becomes clear when we realize that the genome of lowly lungfish and salamanders can be 36 times greater than our own' (Ohno, 1972). Using arguments based on the probability of acquiring deleterious mutations in functional genes, Ohno argued that 'there seems to be a strict upper limit for the number of gene loci which we can afford to keep in our genome'. Ohno noted that gene duplication created multiple copies, allowing 'redundant copies to accumulate formerly forbidden mutations and thereby to acquire new functions'. Ohno's concluding paragraph bears reproduction at a time when the ENCODE project has focused attention on what was once thought to be a vast stretch of desert in our genomes. There is eloquence in Ohno's writing: 'The chance of acquiring a new function by unrestricted accumulation of mutations, however, should be as small as that of an isolated population emerging triumphant as a new species. Degeneracy is the more likely fate. The creation of every new gene must have been accompanied by many other redundant copies joining the ranks of silent DNA sequences, and these silent DNA base sequences may now be serving the useful but negative function of spacing those that have succeeded. Triumphs as well as failures of nature's past experiments appear to be contained in our genome' (Ohno, 1972). Ohno's arguments may have been less than persuasive in convincing his audience, that the overwhelming majority of DNA bases in human genomes were contributing to 'junk'. A comment from the audience (Boyer) is illustrative: 'It thus seems to me that the permissible number of structural loci is – as yet – a somewhat suspect way to arrive at figures of 1% structural utility to 99% junk.'

The community of molecular geneticists in the 1970s was clearly grappling with the uncomfortable thought that higher organisms appeared to waste a great deal of effort in maintaining large genomes, even though much of the enormous length of DNA sequence had no ascribable function. Genomes seemed to resemble inefficient public institutions which harbour large numbers of staff with little by way of work. Nature's extravagance has attracted imaginative explanations, most notably Richard Dawkin's highly influential exposition of the concept of 'selfish genes' (Dawkins, R., *The Selfish Gene*, Oxford University Press, 1976). A 1980 review declared selfish DNA as 'the ultimate parasite' (Orgel, L. E. and Crick, F. H. C., *Nature*, 1980, **284**, 604). Selfish DNA, according to Orgel and Crick, is characterized by its ability to spread 'by forming additional copies of itself within the genome' and by making 'no specific contribution to the phenotype'. They go on to conclude that 'the conviction has been growing that much of this extra DNA is "junk", in other words, that it has little specificity and conveys little or no selective advantage to the organism…. There is a large amount of evidence which suggests but does not prove, that much DNA in higher organisms is little better than junk'. Orgel and Crick repeatedly use the word 'junk', undoubtedly influencing much of the later thinking on the human genome. Ohno's presentations of 1972 and 1973 are conspicuous by their absence in the list of cited references, while a 1972 paper which does not explicitly introduce the word 'junk' into the vocabulary of molecular biology is mentioned in passing.

In advancing the concept of selfish DNA and considering its implications for the role of Darwinian natural selection on genomes of higher organisms, Orgel and Crick range far and wide, but reach a cautious conclusion: 'While proper care should be exercised both in labelling as selfish DNA, every piece of DNA whose function is not immediately apparent and in invoking plausible but unproven hypotheses concerning the details of natural selection, the idea seems a useful one to bear in mind when exploring the complexities of the genomes of higher organisms.' Genome sequencing and the ENCODE project provide an unprecedented and bewilderingly large amount of information on DNA sequences and function, holding out the promise of providing illuminating insights on the manner in which evolution has shaped genomes.

The 1980 issue of *Nature* which carried the Orgel–Crick review also contained a perceptive overview of evolutionary selective pressures and their role in shaping genomes. W. F. Doolittle and C. Sapienza argued that 'natural selection operating within genomes will inevitably result in the appearance of DNAs with no phenotypic expression whose only "function" is survival within genomes'. The authors note that 'when all attempts to assign a given sequence or class of DNA functions of immediate phenotypic benefit to the organism fail we resort to evolutionary explanations'. Indeed one hypothesis often advanced is almost impossible to test – sequences of undefined function are 'yet-to-be eliminated by-products of past chromosomal rearrangements of evolutionary significance'. Doolittle and Sapienza note that 'the phenotype paradigm is almost tautological; natural selection operates on DNA through organismal phenotype, so DNA structure must be of immediate or long-term (evolutionary) phenotypic benefit, even when we cannot show how'. Molecular biologists have often turned to evolutionary imperatives as a crutch to hold up hypotheses that are hard to test. This has not always been viewed kindly by evolutionary biologists. Stephen Gould and Richard Lewontin are quoted by Doolittle and Sapienza: 'The rejection of one adaptive story usually leads to its replacement by another, rather than to a suspicion that a different kind of explanation might be required. Since the range of adaptive stories is as wide as our minds are fertile, new stories can always be postulated' (Gould, S. J. and Lewontin, R. C., *Proc. R. Soc. London*, 1979, **205**, 581). Even as the ENCODE results are analysed, new stories about genomes and evolution will surely emerge, even as 'junk DNA' is laid to rest.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Cognitive Psychology: Impulsive Decision Making and Choice Blindness

Psychology is a discipline that lies outside the borders of the traditional sciences. As the area of 'cognitive science' gathers strength, the borders that separate psychology and the sciences are beginning to become porous and permeable. Indeed, over the past half a century or more the blurring of boundaries, between the 'hard' physical and natural sciences on the one hand and the 'softer' social sciences on the other, has been most evident in economics, which has rapidly succumbed to the advance of mathematics, statistics and, at times, physics (or more accurately, physicists). It has become possible to hold mathematical modellers responsible for the woes of investment banks and other financial institutions in a turbulent economy. Even biology, with ideas dating back to Darwin, finds a place in the discourse of economics, which shares a very large border with the sciences. A subdiscipline, behavioural economics, is an area where psychology and cognitive science begin to intrude. Researchers who attempt to understand the processes of decision making need to cross and recross the borders between various disciplines with great facility. The common labels, 'scientist' or 'social scientist' do not seem to easily describe researchers, who patrol no-man's land, which acts as a buffer zone between the hard and soft sciences. Decision making is a subject that interests many people; after all every individual makes decisions, minor or important, almost every day. Some turn out to be correct; many are often wrong. Individuals vary widely in their abilities to take decisions quickly. In governance, the ability to postpone decision making has often proved valuable in India. Unresolved issues, sometimes, evaporate slowly; 'benign neglect' as a policy can occasionally be a powerful instrument. While decisiveness is a quality that is often sought in leaders, the processes by which decisions are reached vary widely. Consultation and consensus commonly guide decision making in organizations, especially in public institutions. Committee formation is a favoured and time tested strategy; collective decisions seem so much safer when contentious issues are involved, especially in an age where the words transparency and accountability dominate public discourse.

Years of dealing with large numbers of highly educated individuals, both as an academic and as an administrator, have left me with a healthy respect for any research that appears to be directed at an understanding of human behaviour. Decision making by the management in academic institutions, operating in a non-hierarchical manner in an unconstrained environment, is never easy, since choices have to be made between widely differing courses of action, suggested by academics with very strong opinions. A simplistic view of decision making would suggest that all options are carefully considered, pros and cons evaluated and a rational judgement reached. Most often this is not really what happens. There is a great deal of conflicting opinion, with coherent signals buried under a high level of noise, even while pressure mounts for a quick decision to be made. In such situations decision makers act on gut feelings, trusting intuition to be the best guide. Are impulsive and intuitive decisions more often right than wrong? Or is it the other way around? As one who favours intuitive judgements, a course often preferred by those with little detailed knowledge of the issues involved, I was struck by the first line of an abstract of a commentary in *Nature* which proclaimed 'Acting on a gut feeling can sometimes lead to poor decisions'. This somewhat disheartening statement was followed by a very intriguing assertion: 'But it will usually support the common good, according to a study showing that human intuition favours cooperative, rather than selfish behaviour' (Gächter, S., *Nature*, 2012, **489**, 375). My curiosity was further aroused when I noticed that the author was affiliated to an academic unit that was interestingly named as the 'Centre for Decision Research and Experimental Economics'. The commentary engagingly titled, 'A cooperative instinct' highlights a paper, 'Spontaneous giving and calculated greed' appearing in the same issue (Rand, D. G. *et al*., *Nature*, 2012, **489**, 427). The authors ask a question that must have engaged the attention of observers of human behaviour for a very long time: Why do human beings cooperate even when behaviour which benefits others requires that a personal cost must be incurred? The central issue of concern to Rand *et al.* is the 'cognitive basis of cooperative decision making in humans'. The problem addressed, simply put, is whether humans are intuitively self centred and cooperation is a result of rational reflection or whether cooperative behaviour is an instinctive response, with thought and analysis favouring selfish actions. Their conclusions, reached using 'economic games' suggest that 'intuition

supports cooperation in social dilemmas, and that reflections can undermine these cooperative impulses'. The study by Rand *et al.* raises interesting questions which the authors highlight: Is it possible to think of the 'design of institutions that promote cooperation'? Is cooperation 'innate and genetically hard wired, rather than the product of cultural transmission'? They draw attention to earlier studies that demonstrate 'spontaneous helping behaviour in children' (Warneken, F. and Tomasello, M., *Science*, 2006, **311**, 1301).

In his commentary, Simon Gächter draws attention to a recent book authored by Daniel Kahneman (*Thinking Fast and Slow*, 2011, Allan Lane) which, in his words, 'presents a wealth of evidence that much of human decision-making is governed by fast and automatic intuitions, rather than slow effortful thinking'. Kahneman, the 2002 Nobel Laureate in Economics, notes in his lecture following the award that he together with Amos Tversky (1937–1996), 'explored the psychology of intuitive beliefs and choices and examined their bounded rationality'. Kahneman's work 'distinguishes two generic modes of cognitive functions: an intuitive mode in which judgements and decisions are made automatically and rapidly, and a controlled mode which is deliberate and slower' (Nobel Lecture, December 2002). This analysis also suggested that while 'most behaviour is intuitive, skilled, unproblematic and successful', corrections to intuitive judgements sometimes have to be made. In such cases intuition 'will be acknowledged, but the intuitive impression will be the anchor for the judgement'. Everyone makes judgements in daily life. Administrators and editors make judgements, often on matters where their skill and expertise is limited. Juggling both roles, I realised how much one relies on instinct and intuition, rather than calm, deliberate and informed reflection. The paper by Rand *et al.* and Gächter's commentary suggest that impulsive responses may often be more generous than decisions reached after a great deal of thought. The 'economic games' approach used in this study allows participants to 'choose to either keep an allocation of resources for themselves or contribute all or a portion of their allocation to a collective pool, which is then distributed evenly among all players'. This is a commonly encountered situation in research institutions, like my own, where individually obtained resources may need to be shared for a common, public good. Time pressure in forcing quick decisions seems to promote cooperative behaviour, suggesting that in many difficult situations it might be wise to hasten decision making. It is comforting to think that the average individual is intuitively cooperative and generous, while the calculating pragmatists, who hopefully are a minority, are hard wired to be selfish. Reading about research at the confluence of economics, psychology and cognition can be instructive and even entertaining.

Human behaviour is greatly determined by the opinions that individuals form over time. Can opinions change over time? Undoubtedly, they can and very often do, as people respond to inputs of information. Indeed, in science opinions on poorly understood issues change constantly as new evidence accumulates. It is also a fact that stubborn individuals often hold on to pet hypotheses even as the rug is being pulled from under their feet. Moral issues are more perplexing than the debates in science. A recent study titled, 'Lifting the veil of morality: Choice blindness and attitude reversals on a self transforming survey' describes a research strategy, which reveals that moral attitudes are both flexible and surprisingly easy to change (Hall, L., *PLoS One*, 2012, **7**, e45457). To a simple minded scientist like myself, the experimental design used by researchers in cognitive psychology seems devious and designed to expose fickle minds. Hall *et al*. have carried out a survey using a simple two page list of statements on issues that appear to require moral judgements. In a readable summary that appears online in *Nature News* (19 September 2012) Zoë Corbyn points out that the issues range 'from prostitution to the Israeli–Palestinian conflict'. In eliciting answers and discussing subsequent justifications for their responses, the participants were exposed to an intriguing sleight of hand by the researchers. Corbyn describes the trick: 'Each contained two sets of statements, one lightly glued on top of the other. Each survey was given on a clipboard, on the back of which researchers had added a patch of glue. When participants turned the first page over to complete the second, the top set of statements would stick to the glue, exposing the hidden set, but leaving the responses unchanged.' Hall *et al.* cleverly changed one word in two of the statements in the hidden set to provide a response precisely opposite to the original. Half or more of the participants were willing to accept and even argue in favour of opinions opposite to the one they had originally indicated, serving to confirm a phenomenon originally labelled as 'choice blindness', which appears to influence taste, smell and aesthetic choice. While the study reported by Hall *et al.* will stir debate on the reliability and efficacy of questionnaire based surveys, the complexity of decision making and opinion forming processes in the human brain are likely to confound researchers in cognitive psychology for the foreseeable future. As more 'hard' scientists drift into the area of cognition, with tools that directly measure physiological activity in the brain, the subject is likely to enter a phase of great flux, fuelling many future debates.

For administrators and politicians charged with the responsibilities of governance, it may be heartening to note that more than half the signatories to a petition expressing a view or demand, may indeed be able to support an exactly opposite view or demand. Flooded, as we are, with many contentious moral and social issues we may benefit by reflecting on impulsive decision making as a positive force and choice blindness as a means of understanding fickle public opinion.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Scientists, Papers and Journals: Coping with Rejection

A long time ago Charles Dodgson (more famously known as Lewis Carroll) described Man as an 'animal that writes letters'. If he were with us today he might describe scientists as 'an animal species that writes papers'. Writing a manuscript, describing new results, not always earth shaking, and sending it off for publication in a journal is an almost routine activity amongst scientists, especially those involved in academic research. Researchers fall into many categories, but discerning observers might generally agree that three broad classes are identifiable. There are compulsive authors, who can hardly wait to describe a result or finding, however incremental. Writing comes easily to these scientists, who are generally quick to brush off criticism from referees and editors. It is their energy and optimism that keeps journals alive. There is a second group of authors, who are careful in their choice of work to be publicly disclosed and who prefer that every paper be based on a substantial body of results. This group invariably consists of scholarly, but cautious individuals. Indeed, the onward march of science depends greatly on their contributions. Finally, there are the self-proclaimed perfectionists, who intend to produce that one jewel of a scientific publication, which will forever alter the contours of their disciplines. This class of researchers rarely manage to put pen to paper (or more accurately in today's context, fingers to the keyboard). They are generally recognisable in the community as critics of all their fellow researchers. This group often resembles literary and music critics who neither write nor sing.

Even thirty years ago, the scientific literature seemed to consist of a largely useful body of knowledge on which further advances would be based. The number of journals and papers in every discipline and sub-discipline appeared manageable, with conscientious scientists being able to keep up with much of the published work in their fields. But, times have changed and changed dramatically. Scientists are now routinely judged by the number of publications they produce, the impact factor of the journals in which they are published and their influence on the field, as measured by the number of citations they accumulate. Of these parameters, the citation count may be the most reliable indicator of scientific impact, especially when examined several years after the appearance of a paper. Faculty members in academic institutions need to show evidence of productivity as judged from a list of publications, in order to secure career advancement. Students and postdoctoral researchers need to display a significant number of publications to move forward in their careers. Institutions are ranked worldwide on the basis of their published output and even countries are constantly compared. Unsurprisingly, science journals have mushroomed and the scientific literature is a vast ocean, with each new paper being an infinitesimally small drop.

The rise of scientometrics has completely transformed the way the average academic scientist views the practice of science. Assessments of individual performance are increasingly based on apparently quantitative indicators, with the Hirsch *h*-index rapidly gaining ground as a favoured metric. Young academics worry about *h*-indices at a time when they should hardly be worried about a parameter that is almost guaranteed to improve with age. Indeed, aging scientists who decline in every other way are encouraged by a metric that must normally show a positive trend or at best remain at a steady level. Many prolific authors have already begun to devise ways of gaming a system that places an undue reliance on quantitative metrics for assessing scientific quality. The burgeoning scientific literature is growing at a rate which is unsustainable, driven by the desire to maximise publication numbers and citation counts.

Can a parameter like the *h*-index be used to predict the future course of a scientist's career? For the uninitiated, a scientist with an *h*-index of 10 will have at least 10 papers listed in his curriculum vitae, which have each been cited at least 10 times in the literature. A recent commentary in *Nature* advances an intriguing view that a formula can be constructed, which permits the prediction of the *h*-index of a scientist as a function of time (Acuna, D. E., Allesina, S. and Kording, K. P., *Nature*, 2012, **489**, 201). The authors have assembled a dataset of '3085 neuroscientists, 51 *Drosophila* researchers and 151 evolutionary scientists' for whom they 'constructed a history of publications, citation and funding'. In putting together this dataset, the authors imposed a few restrictive conditions in filtering an initial sample of 34,800 neuroscientists, 2000 *Drosophila* researchers and 1300 evolutionary biologists. The final set required that those included had an '*h*-index greater than 4 (to exclude inactive scientists)'. The publications analysed were published after 1995, a restriction required 'because electronic records are sparse before 1995'. The authors included must have 'published their first manuscript in the past 5–12 years'

and 'were identifiable in *Scopus*'. The *Nature* note will undoubtedly attract a wide readership as there are three formulae presented that should allow readers, with even minimal computational abilities, to predict their *h*-indices for the following year and for five and ten years in the future. In keeping with the modern frenzy for quantitation, *Nature* has provided an online facility for readers to try it out for themselves, with the caveat that the projections 'are probably reasonably precise for life scientists, but likely to be less meaningful for the other sciences'. The authors suggest a potentially dangerous use for their formula, arguing that it 'is particularly useful for funding agencies, peer reviewers and hiring committees who have to deal with a large number of applications'. In pushing the quantitative method they note that 'statistical techniques have the advantage of returning results instantaneously and in an unbiased way'. Even as I suppressed a feeling of unease, that the authors may have overstated their case and produced a formula that would be welcomed by science administrators, I was encouraged by their concluding sentences. While acknowledging that their 'findings and predictions may not alleviate scientists' angst over their careers, the results offer some comfort by showing that the future is not so random'. They note, somewhat comfortingly, that 'the occasional rejection of a paper may feel unjust and indiscriminate, but in the long run, such factors seem to average out, rendering *h*-index trajectories relatively predictable'.

Rejection is an occupational hazard for authors. While experienced authors display admirable resilience in searching for journals that will publish their work, the urge to seek out 'high impact' journals ensures that rejection is a common experience for most scientists. Having just received a rejection letter, for what I imagined was an extensively reviewed and revised manuscript, my spirits lifted slightly when I came across a news item that drew attention to a study that concluded 'that rejection improves eventual impact of manuscripts'. The story begins on a cheerful note: 'Just had your paper rejected? Don't worry – that might boost its ultimate citation tally' (Ball, P., *Nature News and Comment*, doi:10.1038/nature.2012.11583). The paper being highlighted, which appeared online a few days ago, analyses the 'flows of research manuscripts among scientific journals' in an attempt to 'reveal hidden submission patterns (Calcagno, V. *et al.*, *Sciencexpress*, 11 October 2012; http://dx.doi.org/10.1126/science.1227833). The authors conducted a difficult exercise in attempting to track the, sometimes tortuous, history of manuscripts before they appeared in print. Their focus, predictably, was in the area of biological sciences where the literature has been expanding at a phenomenal rate. Their strategy was to contact 'the corresponding author of virtually all research articles published between 2006 and 2008 in 16 subject categories of Biological Sciences (923 journals)' and to ask them to answer a direct and simple question: Was the article 'first submitted to the publishing journal and, if not, the name of the journal previously attempted'. This survey yielded the 'submission history of 80,478 articles (37% of all enquiries)' from which the authors go on to 'reconstruct the network of manuscript flows among scientific journals'. They note that 'this network can be used to learn more about publication strategies and perceived journal importance than is available in citation networks'. Ferreting out the often unknown and sometimes forgotten history of published papers may seem a curious exercise, at first glance, to casual readers. Calcagno *et al.* however reach conclusions that must spark interest in scientists intensely focused on the race to publish and to publish in high impact journals. As anticipated, high impact journals, *Nature* and *Science*, predictably, amongst them, were key nodes in the network. Surprisingly, the authors discovered that the high profile journals 'published a higher proportion of papers that had been previously submitted elsewhere than did more specialized and lower-impact publications', leading Philip Ball to conclude that 'rejection improves eventual impact of manuscripts'. Reading this was like balm to my wounded ego, still smarting from recent rejection. Clearly, my colleagues and I would now be well advised to raise our sights and aim higher. The Calcagno paper reveals interesting facts about scientists and their journals. They note that, 'overall 75% of all published articles were first-intents, with a range of 67% to 87% across subject categories. None of the journals … sampled was found to be purely recycling manuscripts rejected from other journals. Thus most articles were initially targeted to the journal that would eventually publish them.' Experienced authors, tempered by early experiences of rejection, undoubtedly exercise better judgement in their choice of journals to which they submit their manuscripts. Limiting risks and adopting a pragmatic approach in submitting manuscripts is presumably a widely adopted strategy.

The authors pose some interesting questions. 'Does submission history reflect the intrinsic "quality" or, in a more quantifiable way, the impact or utility of articles following publication?' Armed with their formidable dataset of prepublication histories, Calcagno *et al.* turn to the *Web of Science* to examine citation counts for papers for about 3–6 years, following their appearance in print. Their findings may encourage authors struggling with the process of resubmission after rejection. Curiously, 'resubmissions were significantly more cited than first-intents published the same year in the same journal'. Every reader experienced in the game of publishing papers will undoubtedly have his or her own explanation for these findings. The authors go on to gently suggest that 'these results should help authors endure the frustration associated with long resubmission processes and encourage them to take the challenge'.

Science watchers armed with the technologies of the information age will undoubtedly continue to provide new insights into the behaviour of scientists and their journals.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Stress Research: Bridging Sociology and Biology

Stress is a common phenomenon in modern life. In academic institutions students encounter stressful situations with a fixed frequency, when they are compelled to write examinations. In the final years of school the pressures build, as competitive entrance examinations, requiring intense preparation, loom ahead. Research students who have generally put the hurdles of examinations behind them face an unfamiliar pressure; the need to obtain research results of an adequate quality and quantity to successfully complete a Ph D thesis. Research is often an uncertain enterprise, requiring several years of effort to put together a dissertation; a long haul involving close interaction with a supervisor. The relationships between students and mentors in research institutions can at times be strained; a not uncommon situation in human interactions. Supervisors are often under pressure to produce results and publications to obtain grants to further their own research careers. This pressure is often transferred to students, who are the true workforce in research laboratories. It is therefore common in academic institutions to encounter a fair number of unhappy individuals, who appear to be finding it difficult to cope with pressures and stress. Having done more than my fair share of amateur counselling for distressed individuals, I was pleasantly surprised to be invited to the opening of a web-based 'chat site', which would facilitate quick, easy and anonymous counselling by experienced counsellors. Being able to seek help without attracting undue attention can be a source of comfort to distressed individuals. Stress and strain are terms often used and readily understood by physicists and engineers. Stress is a term that is hard to define and even harder to measure in studies of human behaviour. Some individuals find it difficult to handle even mildly uncomfortable situations, while others display remarkable resilience, coping even with situations which appear catastrophic to most people. Modern life, especially in crowded urban centres provides an environment that promotes stress. Research on the factors that contribute to stress and strategies for creating environmental conditions for reducing stress has largely been the preserve of sociologists. Is human behaviour determined by our genetic make-up or is it entirely shaped by our environment? This is an old question, which has often been asked triggering a debate that appears to pit sociologists against biologists – 'nature or nurture'.

A special section in the October 11 issue of *Nature* presents a wonderfully readable overview of the growing recognition that severe and prolonged stress does indeed leave a physiological imprint, with biochemical changes triggering a cascade of events that influence gene expression. Intriguingly, some current research appears suggestive of aberrant physiology being transmitted to the next generation. The prefatory editorial reminds readers of the origins of the sociology–biology divide, quoting Francis Galton: 'Nature is all that a man brings himself into the world; nurture is every influence which affects him after his birth.' The advance of modern biology over a century has ensured that 'biological determinism' is not an idea that is any longer tenable. Neuroscientists are comfortable with the idea that the environment can profoundly influence brain activity. In contrast, sociology has often sought to distance itself from scientific research that probes the causes of human behaviour. At times, this stubborn distancing from science has served a valuable purpose. As the *Nature* editorial notes, by 'energising the debate' sociologists 'did a service to both academia and society by keeping scientific arrogance and influence in check' (*Nature*, 2012, **490**, 143). Stress can be caused by a myriad factors. However, individuals can differ widely in their responses to stress; an observation that suggests that it might indeed be rewarding to understand the 'roots of resilience' (Hughes, V. A., *Nature*, 2012, **490**, 165).

Most readers will relate immediately to a feature that highlights the role of urban decay in promoting stress and all the attendant consequences. Alison Abbot cites a study in a London suburb, that revealed a doubling of schizophrenia cases 'from around 11 per 100,000 inhabitants per year in 1965 to 23 per 100,000 in 1997 – a period in which there was no such rise in the general population'. With populations in cities growing alarmingly, and India's cities are amongst the fastest growing, public health issues relating to mental illness may assume disturbing levels. Can the effects of stress on the brain be measured? It has long been known that the immediate physiological response to stress results in a surge of the levels of the hormones adrenaline and cortisol. These

hormones are released in response to signals from the brain, a complex process involving both chemical and electrical events. Once introduced into battle, adrenaline and cortisol direct blood flow and enhance sugar levels, providing the burst of energy that animals and humans need both for fighting and fleeing. The reserves of energy that we summon arise from an instinctive response of the brain to stress. Can these responses be monitored and are there methodologies that permit an assessment of the long term consequences of these sudden and dramatic chemical events triggered by stress? Brain imaging techniques now allow an unprecedented view of the workings of different regions of the brain. The *Nature* article draws attention to a study published last year that 'clearly showed that people who grew up in cities process negative emotions such as stress differently from those who move to the city as adults' (Abbot, A., *Nature*, 2012, **490**, 162). The study by F. Lederbogen *et al.* (*Nature*, 2011, **474**, 498) was cleverly designed to probe the effects of 'social stress' on two groups of volunteers, with distinctly different backgrounds. Interestingly, the regions of the brain activated by stress appeared to be different in volunteers with a history of city dwelling, as compared to the group who had only recently moved to urban surroundings. These studies appear to provide a connection between environmental factors, the province of sociologists, and neurobiology and neurochemistry, which undoubtedly lie in the domain of hard science. A mounting body of evidence suggests that there is a distinct genetic predisposition to psychiatric illness, with environmental stresses hastening the descent into disease. Ongoing research attempts to pull together functional magnetic resonance imaging studies and large scale analysis of genes correlated with the occurrence of schizophrenia. Stress appears to activate regions of the brain that process negative emotions to a much greater extent in those bred in cities as compared to their compatriots from non-urban backgrounds. The *Nature* report provides interesting previews of planned research, which attempts to delineate the most stressful regions of a city; a project that draws together a curious combination of physicists, geoscientists and neuroscientists.

The biochemical markers of stress are now becoming more and more evident. Elizabeth Blackburn, the 2009 Nobel laureate in Physiology or Medicine, provides a clear warning that chronic stress and disease may be linked by the observed shortening of 'telomeres', in diverse examples of stress over prolonged periods. Telomeres are long stretches of up to a few thousand DNA bases, which shorten each time a living cell divides. While enzymatic mechanisms exist to extend telomeres, aging results in shortening. The finding that pronounced shortening is also correlated with stress suggests that stress induced premature onset of age related disease is a distinct possibility (Blackburn, E. H. and Epel, E. S., *Nature*, 2012, **490**, 169). Recent work underscores the connection between shorter telomeres and stress in groups ranging from 'kindergarten-aged children to adults as old as 80; from small clinical samples of less than 100 people to large population-based samples of thousands'. Blackburn and Epel note that there are 'three pairwise links involving three factors: stress with telomere shortness; stress with disease risks; and telomere shortness with risks for these diseases. It is hard to avoid the inference that at least one of the ways stress causes chronic diseases is by shortening telomeres'. Do these research findings hold promise for 'practical ways to improve human health'? One approach would of course be to propose a search for drugs to boost telomerase activity (the process by which cells lengthen eroding telomeres), 'without inducing unwanted side-effects'. This is a strategy that will require a formidable investment of effort, with no guarantee of quick success. Are there any alternatives? Blackburn and Epel point the way towards an approach that might find favour with social scientists. They suggest: 'More feasible approaches to alleviating telomere shortening could involve mitigating the conditions that lead to chronic stress and helping people change certain behaviours.' The warning signs of shrinking telomeres appear to be evident in children subjected to abusive treatment and in infants born to stressed mothers. Blackburn and Epel conclude on a sombre note: 'Telomeres send one or more signal – from the tips of our chromosomes – that unmanageable social and psychological stress, especially during early life, is as insidious as smoking or too much fast food.'

Are there other connections between our genes and our environment? Recent research suggests that epigenetic processes may hold the key. In attempting to explain how 'trauma affects people differently', E. J. Nestler highlights work that suggests a role for 'experience-dependent molecular alterations to DNA or proteins that alter how genes behave without changing the information they contain' (*Nature*, 2012, **490**, 171). In hunting down biochemical markers of stress response, there is a need to distinguish changes that are adaptive in nature from those that are deleterious. Nestler surveys controversial work suggesting that animals can 'inherit epigenetic vulnerability to stress'. Animal models are becoming available to determine whether maternal stress can be transmitted to offspring. Nestler is cautious in pointing out that 'epigenetics is in vogue'. There is indeed an understandable haste in identifying 'correlations between behaviour and molecular alterations in cells without establishing a causal link'.

Stress and the mental disorders that often follow pose a difficult challenge. As the connections between biology and the environment are better understood it may be possible to search for solutions. This will need sociologists and biologists to work in tandem. Stress research may indeed bridge the two cultures.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

X-rays, Crystals and Diffraction: Illuminating the Structure of Matter

Students of science, in the final years of school, and those who continue to study physics in college, confront the famous equations of physics, at times with trepidation. Newton's $F = ma$ makes an early appearance; mechanics seems to relate to everyday experience, although a little reflection might lead to the conclusion that most individuals are comfortable with an Aristotelian world view. Einstein is inextricably linked, in the popular mind, with $E = mc^2$; the mass–energy equivalence conjuring up the dramatic image of mushroom clouds after a nuclear explosion. There are many other immortal equations, easy to remember but harder to relate to, like Schrödinger's $H\psi = E\psi$, which introduces us to the uncomfortable idea that everything may not be what it seems to be; its symbols representing a hidden complexity that eludes common understanding. There are other equations like Boltzmann's $S = k \log W$, an equation of 'compact and penetrating beauty' in Jacob Bronowski's words, which are striking for their simplicity of form and remarkable for the deep insights into nature that have been derived from them. It is easy to gauge which equations have imprinted themselves in the minds of students, who have little intention of pursuing a career in science, during casual questioning at interviews. There is one, a favourite in the many committees in which I have been a spectator, which appears embedded in the memories of students, in the years after exposure to college physics – Bragg's $n\lambda = 2d \sin\theta$. Bragg's law is simple in form, but spectacular in the insights that it provides on the interaction of X-rays with crystals; laying the foundations for building the edifice of X-ray crystallography, a technique, which more than any other, has contributed to our understanding of the structure of matter over the last century. Bragg's equation appeared almost exactly a century ago in November 1912, an anniversary highlighted with characteristic elegance by John Thomas, in an essay which notes that 'the seminal work begun in Yorkshire that summer of 1912 still resonates worldwide'. He draws attention to the title of a lecture scheduled at the Cambridge Philosophical Society, to mark the occasion, to be delivered by Venki Ramakrishnan, which says it all: 'Seeing is believing: how a century after its discovery, Bragg's law allows us to peer into molecules that read the information in our genes.' Crystallography is, in Thomas' words, 'still the single most powerful tool for scientists in physics, biology, medicine, materials and Earth sciences as well as for many breeds of engineer' (Thomas, J. M., *Nature*, 2012, **491**, 186). Ironically, Thomas forgets to mention chemistry, a Freudian slip. Indeed, most scientific advances that critically depend on X-ray crystallography have been recognized with Nobel prizes in chemistry.

Röntgen's discovery of X-rays is also a November event that dates back to 1895. The serendipitous discovery of a mysterious radiation that appeared to penetrate soft matter has probably had a greater impact on medicine and chemistry than any other finding of the 19th century. Radiology's origins may be traced without any difficulty to Röntgen's famous X-ray images of his wife's ringed hand. A year after a reluctant Röntgen, known for his reticence, spoke about his discovery, X-rays entered medical diagnosis. In the early years, patients encountered X-ray machines that appeared formidable and intimidating, a sharp contrast to modern day facilities. A commentary on 'First-generation radiography: The patient's perspective' provides a fascinating account of what it was like 'to undergo radiography in the first generation'. The authors quote extensively from Thomas Mann's novel, *The Magic Mountain*, that appeared in 1927. Mann, the 1929 Nobel prize winner for literature, was a patient at a tuberculosis sanatorium when he encountered X-rays. 'His wife had been a student of Röntgen at Munich' (Gunderman, R. B. and Tritle, B. A., *Radiology*, 2011, **259**, 321). One of Mann's characters in the book is permitted to view 'his own form through the lens of the X-ray'. He sees 'precisely what is hardly permitted man to see, and what he had never thought it would be vouchsafed him to see: he looked into his own grave… and for the first time he understood that he would die'. Gunderman and Tritle note that 'for centuries the skeleton has symbolized death'. X-rays strip away the flesh allowing us, in the authors' words, to perceive 'the same skeleton, symbol of the ineluctable mortality that lurks inside every human being'. Indeed, Röntgen's wife described the view of the skeleton of her hand as 'a premonition of death'. The early amazement and fear of X-rays quickly gave way to wide acceptance. A light hearted retrospective on Röntgen rays draws attention to poetry and limericks inspired by the discovery.

A doggerel that appears in a 1970s physics book is a sample (Rowe, R. C., *Drug Discovery Today*, 2003, **8**, 60)

'*I'm full of daze,*
Shock and amaze;
For nowadays
I hear they'll gaze
Thro' cloak and gown – and even stays,
These naughty, naughty, Röntgen Rays.'

What were X-rays? Were they particles or waves? These are questions that occupied physicists in the years following Röntgen's discovery at a time when the edifice of classical physics seemed on the verge of crumbling under the pressure of impending revolution.

By 1912, Paul Ewald's work on crystals and their optical properties was becoming known, leading Max von Laue to suggest the famous experiment, carried out by Friedrich and Knipping, which revealed that ZnS crystals acted like a diffraction grating when a beam of X-rays struck the material. X-ray diffraction was born and the Braggs, father and son, armed with the insights from Bragg's law, quickly began to determine the structures of simple inorganic crystals. Sodium chloride succumbed first, quickly followed by the potassium halides and finally diamond, all in 1913. A century later it is hard to imagine the excitement that must have been caused in the physics and chemistry communities at that time, although World War I must have undoubtedly limited the impact to a great extent. Within fifteen years after their institution, three Nobel prizes in physics were awarded for the discovery of X-rays and their abilities to lay bare not only the human skeleton in living people, but also to uncover the arrangements of atoms in crystals. Röntgen in 1901, von Laue in 1914 and William and Lawrence Bragg in 1915 were honoured for their work, with Lawrence Bragg at 25 being the youngest ever Nobel laureate. Crystallography became a discipline that made major inroads into the study of minerals and inorganic materials by the 1920s and the first steps towards the structure determination of small organic molecules were evident by the 1930s. For the practitioner, the 'phase problem' loomed large; a technical hurdle that impeded the progression from an X-ray diffraction pattern to a crystal structure. Specific techniques developed to overcome the obstacles to three-dimensional structure determination allowed some major battles to be won in the 1940s and 1950s. The conquest of penicillin, not long after the antibiotic era began in the mid-1940s, and vitamin-B12 revealed molecular structures unprecedented in chemistry. Crystalline fibers, less ordered than the perfect single crystals of chemistry, provided limited diffraction data but together with model building allowed X-rays to breach the barriers between the 'small' molecules of chemistry and the 'large' molecules of biology. The alpha-helix in fibrous proteins, the DNA double helix and the collagen triple helix (an Indian triumph) appeared in the years between 1951 and 1954. Size barriers were truly breached when the structures of the proteins myoglobin and hemoglobin appeared, revealing for the first time the formidable complexity of molecular structures in biochemistry. Crystallography had given birth to structural biology. The 1962 Nobel prizes in chemistry and physiology or medicine recognized the structures of proteins (Kendrew and Perutz) and DNA (Watson, Crick and Wilkins). The hard won successes of penicillin and vitamin-B12 were recognized a couple of years later (Dorothy Hodgkin). Even in the late 1960s and early 1970s X-ray diffraction remained the domain of specialists. The advent of the direct methods of phase determination coupled with the development of immensely efficient computer programs for structure solution and refinement, together with the availability of automated diffractometers with intense X-ray sources and sophisticated detectors, opened the floodgates of molecular structure determination, X-ray diffraction was an essential analytical tool in chemistry by the mid-1980s. The 1985 Nobel prize in chemistry to Jerome Karle and Herbert Hauptman recognized the key catalytic role of the mathematical approaches that led to this transformation. The esoteric, probabilistic methods that formed the core of the direct solution of the phase problem were not readily assimilated in the years immediately following their publication. In Hauptman's assessment the initial reactions were skeptical, if not hostile. He says: 'In hindsight I think this reaction was due, first, to the strong mathematical flavour of this early work, not well understood by most crystallographers, as well as the ingrained and almost universal belief that the phase problem was unsolvable in principle and that any claim to the contrary must therefore be flawed' (Hauptman, H. A., *Struct. Chem*., 1990, **6**, 617).

Crystallography in India traces its origins to C. V. Raman's laboratory at the Indian Association for the Cultivation of Science in Calcutta in the late 1920s leading to the publication of the structures of naphthalene and anthracene by Kedereshwar Banerjee in 1930. The highpoint was reached in Madras in the 1950s and 1960s when the structure of collagen was determined by G. N. Ramachandran, culminating in the pathbreaking work on the conformations of protein chains. 2014 has been declared as the International Year of Crystallography, marking the 100th anniversary of von Laue's Nobel prize. In reflecting on the evolution of crystallography over a century I was drawn to an unusual article entitled 'Domesticating the crystal: Sir Lawrence Bragg and the aesthetics of X-ray analysis' which argues that Bragg's science preferred 'aesthetics over mathematics'. The author goes on to conclude that 'placing X-ray crystallography in this category will give us a richer view of modern physics, too often seen as relatively plus quantum mechanics' (Black, S., *Configurations*, 2005, **13**, 2007). Early X-ray images of human bodies evoked a premonition of death. When Röntgen's rays are trained on inanimate crystals, they bring them to life. X-rays have truly illuminated the structure of matter.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science in India: Reflections on the Anniversary of a Congress

The Science Congress is the only congregation of scientists in India that attracts media attention. Curiously, few practising scientists of note consider the Congress as an important event. Pomp and ceremony take precedence over substance. Over the years the Congress has been reduced to an occasion where the inaugural session appears to be the *raison de etre* for the meeting. The traditional opening address by the Prime Minister predictably reiterates governmental commitment to support science and invariably promises to remove the many bureaucratic hurdles that sometimes loom larger than life in the minds of many scientists. The presence of the executive head of government invests the inaugural event with an importance that is often not commensurate with the quality of the scientific sessions that follow. The occasion is also used to showcase a couple of Nobel laureates, who fly in to speak to audiences with little appetite for excessively technical talks. The organisers, bolstered by considerable government backing, are always good hosts; the distinguished foreign presence ensuring that the Congress always acquires a degree of respectability rarely supported by the scientific program. The next Congress, scheduled for January 2013, due to begin in less than a month, marks the 100th such event. The Congress returns to the city of its origin, Kolkata and will undoubtedly honour the memory of its first president, Ashutosh Mukherjee (1864–1924); a man who might justly be credited with fostering the renaissance of science in the city, and the country, in the first two decades of the 20th century. Calcutta, then, was the epicentre of the intellectual ferment that accompanied the first stirring of nationalism in India. Even as the British moved the seat of administrative and executive power to Delhi in 1911, Calcutta firmly established itself as the capital of modern science in this country. The Science Congress in Ashutosh Mukherjee's time must undoubtedly have had an ambience far removed from the Congress a century later. The city too has not only changed its name but, most visibly, bears the imprint of the turbulent century that has followed. Ten years ago, in marking the 90th anniversary, the Indian Science Congress Association (ISCA) produced three volumes titled *Shaping of Indian Science* (Universities Press, Hyderabad, 2003), which document the presidential addresses over the past one hundred years.

The origins of the Science Congress are described in the 1928 presidential address by John Simonsen, an Englishman and a natural products chemist, who was appointed as Professor of Chemistry at Presidency College, Madras in 1910. He describes the circumstances of that time as 'uninspiring' and notes that 'research in the universities at the commencement of the century was practically non-existent'. Some parts of Simonsen's address may stir a feeling of *déjà vu*: 'Early in the present century it was recognised by those in authority that all was not well with university education in India and what may be called the Curzon Commission on University Education was appointed.' In recommendations, that would be repeatedly echoed in the future, the Commission suggested new courses ('honours courses'), increasing 'the teaching staffs in the various colleges' and 'the desirability of stimulating research in university colleges'. Simonsen adds that the Commission 'recognized the correctness of the view … where there is no zeal for research there is no vitality in teaching'. If Simonsen were to survey the science scene in India today, he might well endorse a corollary: 'Where there is no teaching there is little vitality in research'. Simonsen and his colleague in chemistry, P. S. MacMahon of the Canning College, Lucknow, both of whom 'felt the great lack of scientific intercourse' generated a circular in 1911 proposing the creation of an 'Indian Association for the Advancement of Science'. Although the Simonsen–MacMahon circular was drafted in 1911, the high noon of the British presence in India, it bears repetition a century later: 'We realise that the future of Science in India depends upon the adequacy of the practical training which students receive in college laboratories and furthermore that nothing is better calculated to increase its efficiency than the inculcation of research as the ultimate purpose of all scientific knowledge. It is unnecessary to point out how many and varied are the problems awaiting solution or how intimately the social and economic future of India is bound up with the successful application of scientific methods to all the activities, whether agrarian or industrial of the community.' The Provisional Committee originally invited to define the scope and constitution of the association included only two Indians, J. C. Bose and P. C. Ray, both from the Presidency College, Calcutta. Most members were from

government organizations; a notable exception was Morris Travers, the first Director of the Indian Institute of Science, then in its infancy. Simonsen, in looking back at the scientific scene in India in 1928, notes with some distress that the status of Indian universities is far from satisfactory. He notes that administrative bodies have, at times, ordered that 'the percentage of passes in an examination should be increased'. Such events, in Simonsen's words, result in 'a general diminution in the status of the degree and if this is permitted to continue the degree of an Indian university will cease to be of value in academic and industrial affairs'. Concerns about the current quality of degrees awarded by the ever-expanding university system continue to worry academicians more than eight decades after Simonsen's address. He did note, thoughtfully, that there might be a change 'when there is a clearer realisation of the difference between knowledge and wisdom' and goes on to quote Cowper:

'Knowledge and Wisdom, far from being one,
Have oft times no connection. Knowledge dwells
In heads replete with thoughts of other men;
Wisdom in minds attentive to their own,
Knowledge is proud, that he has learnt so much;
Wisdom is humble that he knows no more.'

The first volume of the ISCA Presidential Addresses begins with Ashutosh Mukherjee's inaugural speech in 1914 and ends with a 'stenographic summary' of Jawaharlal Nehru's address on 3 January 1947, eight months before independence. Nehru, then a key member of the Interim Government, appears to have had an intuitive idea of the difficulties that lay ahead: 'Governments normally are very slow and the only thing that moves them is some immediate outcry which affects their future indirectly. Therefore, I should discourage among the scientists a reliance always on what Government may or may not do.' This was the first time that Nehru was to publicly state his commitment to promoting science in India: 'We are intensely interested in scientific development in India and we shall do everything in our power to encourage scientific research.' The Nehru era, between 1947 and 1964, marked a period of considerable growth in the number of institutions devoted to research and teaching in science and engineering, despite an economic situation which was hardly conducive to high growth. In nearly fifty years after Nehru, his sentiments have almost invariably been echoed by his successors. Over the last twelve years governmental spending on science has increased substantially and there has been a sudden spurt in the creation of new institutions. Despite the upsurge in spending and the rapid growth of infrastructure for science, a sense of unease prevails. The universities across the country are in a state of disarray. They are not the centres of research which must have been thought of in the early years of the Science Congress. The regulatory bodies created to oversee higher education, brought into existence by legislation in the early years after independence, are now rudderless and cast adrift by governmental neglect. Ashutosh Mukherjee and John Simonsen would hardly have been encouraged by the scene that confronts us today.

The intensely competitive nature of modern science has begun to throw up new challenges for those who practice and manage scientific research. The ability to quantitate 'scientific performance' using the measures of scientometrics has made 'science watching' a respectable sub-field of science. India's scientific output in the twenty years after economic liberalisation has increased steadily; but it is a rate of growth that pales by comparison with that of China, often leading to pessimistic forecasts for the future. There is also a disconnect between the various agencies and arms of government that determine science policy and funding and the institutions where the research is actually carried out. Nehru's romantic view of science and his proximity to influential men of science has not always been shared by his successors. Today the charge of a Ministry of Science and Technology is viewed as a 'demotion', when compared to the Ministry of Petroleum, by large sections of public opinion; an indication that in public perception science and technology appear unconnected with national development. This is hardly a situation that would have been anticipated by the founders of the Science Congress.

The early years of the Science Congress coincided with a golden era of science in India. Much of the growth in science immediately after independence can be traced to men of scholarship moulded in those days, driven by the, now forgotten, ideals of building a new nation. This journal was conceived at the Science Congress in Bangalore in 1932 and was born a few months later in July 1932. L. L. Fermor's Presidential Address of 1933 discusses the financial needs of the journal ending on an optimistic (and prophetic) note: '*Current Science* will no doubt eventually pay its way.' The discussions to create institutions that promote and foster science also began at the Science Congresses in the late 1920s and early 1930s leading to the formation of the Academies. Both the Congress and the scientific enterprise in India have grown and transformed over the century that has passed. Anniversaries, especially a centenary, are an opportune time for reflection and serious introspection. Does the Congress contribute in any way to the 'shaping of science in India'? Does science merit an important position in a national agenda for the future? The last question might well be answered by Martin Forster, who quoted Napoleon Bonaparte, in his 1925 Presidential Address at Benares: 'True conquests, the only ones which leave no regrets are those which are gained over ignorance. The most honourable, as well as the most useful occupation for nations is to assist in extending human knowledge.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Tools as Drivers of Science

In the mid 1970s, when I began my career as an observer of science and scientists, academic laboratories in India were poorly equipped. Instrumentation was scarce and computers, where available, were monstrous entities hidden away in air-conditioned seclusion. Few laboratories and even fewer offices sported air-conditioners. The capricious power supply was not supplemented by standby generators. Power cuts, frequent and unpredictable, provided a justifiable reason to adjourn to canteens. Physicists often made their measurements with home built devices and engineering departments appeared filled with large and impressive machinery, conveying a sense of great utility. Relatively few theoreticians stalked the corridors of academic physics and engineering departments. 'Simulation' was a word that had not yet entered the lexicon in our institutions. Chemistry appeared to require little by way of sophistication; laboratories sported glassware of various shapes and sizes, with heating and stirring being the most common operations. Mixture separation relied on the robust methods of classical chromatography, which taught students the virtues of experimental skill and patience. Analysis was dependent on a couple of sturdy and over-used infrared and UV–Visible spectrometers, housed in centralised instrumentation facilities. Nuclear magnetic resonance (NMR) spectrometers were rare; available only in privileged institutions and often declared as 'national facilities'. X-ray diffractometers were yet to make a significant appearance. 'Structural biology' was a term that had not surfaced. Electron impact mass spectrometers were an esoteric and distant tool for most chemists in India. Biology appeared to demand even less by way of instrumentation. Light microscopes and refrigerated centrifuges, supplemented by a carefully preserved spectrophotometer, seemed to serve the needs of biochemistry and microbiology, while botany and zoology were even less demanding subjects. Ice flakers were largely unknown and crushed ice was simply obtained by able bodied students pounding a large block of ice, enclosed in a sack, to pieces. The ends of physical exercise and the need to vent frustration were both served in this essential operation, which most research students of chemistry and biology quickly learned to master. Liquid nitrogen was scarce; liquid helium truly rare leaving physicists content with temperatures commonly attainable. Biology was poised on the cusp of change. The term 'molecular biology' was beginning to be heard in seminar rooms. DNA was on its way to becoming an object of reverence. The emerging methods of recombinant DNA technology were mentioned with awe in corridors and 'genetic engineering' rose with amazing rapidity to become a much sought after discipline amongst new students entering research. 'Biotechnology' promised much and traditional biology courses were quickly renamed, hoping to attract students to a subject that held out hope for a bright future.

By the early 1980s, the organisational structure of the departments of government, charged with supporting academic research, had undergone major transformation. Individual investigator driven grants and departmental support programs became available, even as new techniques began to gain ground. The drive towards organised research gained momentum and equipment began to slowly appear in many institutions. Money was scarce and the battle to obtain equipment could be long and wearying; maintenance a difficult and tiring task. Foreign suppliers had a negligible presence in India and local agents lacked both the expertise and the tools with which to rectify malfunctioning equipment. Foreign exchange, in the days before the economic liberalisation, was not always readily released for major imports. The winds of change were felt in the late 1980s as the personal computer revolution gained momentum, driven by a sudden loosening of import restrictions. The effects of an improving economic situation were evident by the late 1990s as science funding began to show an upward trend. The growth in research grants and support for establishment of major research facilities in academic institutions has been pronounced in the last decade, with the promise of increased research spending in the 12th Five Year Plan, whose formal launch is anticipated very soon. The major institutions and centres of research in India present an appearance today that is a far cry from the scene that confronted observers twenty years ago. Laboratory instrumentation has grown dramatically in numbers and in sophistication. The many new institutions that are being created find it easier to acquire equipment than to recruit faculty. New centres in old institutions (and mine is a prime example) are significantly better equipped than old traditional departments. In the burgeoning number of new institutions that have been created, equipment has often arrived before laboratory spaces are available. Long delays in installation and use are not uncommon. Researchers on

the verge of an independent scientific career, fresh from postdoctoral stints in foreign laboratories now set about attempting to replicate experimental facilities that they are familiar with. The sophistication of the faculties created does not always match the nature of the research envisaged. Centralised facilities of a very high degree of complexity of operation and maintenance are created with a limited assessment of the long term needs of manpower and supporting infrastructure to ensure efficient functioning. Despite concerns raised by the rapid expansion of research facilities in many institutions, there is little doubt that the past decade of steadily growing science funding has enhanced the quality and quantity of scientific output across the country. While some analysts worry that the rate of growth of scientific research in India does not match that in countries like China, which gallop along at a fearful pace, unbridled expansion undoubtedly creates new problems for the scientific community to address.

Is there a downside to the march of science driven dominantly by new instruments of measurement and analysis. Is science, as Freeman Dyson asked years ago, 'being driven by new technology rather than by new concepts'? Dyson returns to this question in a commentary that asks: 'Is science mostly driven by ideas or by tools?' (Dyson, F. J., *Science*, 2012, **338**, 1426). He begins, predictably, with Thomas Kuhn's analysis of the development of science, *The Structure of Scientific Revolutions*, which appeared in 1962 and was greatly influenced by the upheaval in physics in the early years of the 20th century. Kuhn introduced into the vocabulary of science the word 'paradigm', which is more misused than understood. The romantic view of science owes much to Kuhn. Revolutions happen, 'a discontinuous shift from one paradigm to another' when, in Dyson's words, 'new ideas explode with a barrage of new insights and new questions that push old ideas into oblivion'. Relativity and quantum mechanics were 'new paradigms created out of abstract ideas'. This physics centric view of science may ignore revolutions that happened over long periods of time, imperceptibly changing the foundations of disciplines. The cataloguers and classifiers who were the drivers of change in the 19th century, Mendeleev in chemistry and Darwin in biology were responsible for gradual shifts in ideas that dominated their disciplines. Atomic theory subversively entered chemistry, establishing itself as opponents gradually departed from the fray. In attempting to address the question posed in the title of his commentary, Dyson draws attention to a 1997 book, *Image and Logic*, by Peter Galison which emphasises the key role of tools in driving science. Dyson notes that 'Galisonian science' always existed, but was dismissed 'with the epithet normal' by Kuhn. He points out that 'the tools of steam engine technology came first, before the ideas of thermodynamics; the tools of telegraphy and telephony came first, before the ideas of information theory'. Dyson provides a unique insight into the state of science in the middle of the 20th century, when 'the world of physics was sharply split into Kuhnian and Galisonian programs'. He is well positioned to pass judgement: 'The old heroes of the pre-war revolutions were pursuing private dreams of Kuhnian revolutions still to come. Einstein dreamed of a unified field theory. Heisenberg and Schrödinger and Dirac each had a dream based on equations rather than on experiments. Each of them believed that progress in physics could only come through revolutionary new ideas. Each of them dreamed of repeating the triumphs of the 1920s. Meanwhile, the younger generation was using the new tools generated by military technology to push science ahead in Galisonian style.' He cites examples: Martin Ryle and radioastronomy, Willis Lamb and the hydrogen atom's fine structure, Maurice Wilkins and Rosalind Franklin's X-ray photographs of DNA fibres, which led to the double helix and Melvin Calvin's use of radioactive tracers and paper chromatography to probe photosynthesis. He concludes: 'Four new tools created four new sciences. Ten years after World War II ended, Galisonian science was roaring ahead while Kuhnian dreams had faded. And so it continued for the rest of the 20th century.' Dyson's focus is physics and his assessment at the end of the first decade of the new century is revealing: 'We are standing now as we stood in the 1950s, between a Kuhnian dream of sudden illumination and a Galisonian reality of laborious exploring…. The balance today is more even than it was in the 1950s.'

If we escape from a physics centric view of science one might conclude that in disciplines like biology or chemistry a Galisonian world view prevails. Biological sciences today are driven, more than ever before, by tools and technologies. As data of diverse kinds pour in, the tasks of interpretation seem daunting. In a companion commentary, Sydney Brenner provides a characteristically provocative view of biology, arguing that 'the whole of biology must be rooted in DNA, and our task is still to discover how these DNA sequences arose in evolution and how they are interpreted to build the diversity of the living world. Physics was once called natural philosophy; perhaps we should call biology "natural engineering" ' (*Science*, 2012, **338**, 1427). Biological researchers today may be divided into two camps; one using a wide range of tools to address specific problems while the other uses a limited range of extraordinarily powerful tools to generate a bewildering mass of data. The tensions between the advocates of what is euphemistically called 'hypothesis driven research' and the practitioners of technologically driven 'data intensive research' are evident. But both camps are adherents of Galisonian science; that is the reality of biology. As Indian laboratories in academic institutions accumulate the increasingly expensive tools and technologies of science, it may be important to remember that tools are only as good as the workmen who handle them.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science in School and College: Teach Less, Learn More

The fact that the number of well prepared high school students opting for undergraduate courses in science is relatively small, has been widely discussed in policy making circles and the media over the past several years. There is a strong perception that the best prepared students entering college overwhelmingly prefer engineering or medical courses, leaving the sciences a distant straggler at admissions time. The drift away from science and mathematics is visible even earlier in the school years, as students move away to commerce and other subjects, often finding science forbidding and difficult. At college level, an archaic description of some courses, engineering, medicine and even pharmacy amongst them, as 'professional' colours public perception. An inescapable corollary of this nomenclature is that all other courses, most notably those in the sciences and humanities, are 'unprofessional' or less likely to lead to employment in lucrative and rewarding professions. The word 'education' has slowly been replaced by the term 'human resource development', implying a shift of philosophy in the arena of school and college education. The rapid growth of private colleges and deemed universities awarding 'professional' degrees, with academic standards lightly overseen by bodies like the All India Council of Technical Education (AICTE) and the Medical Council of India (MCI), has resulted in many controversial situations over the past few years. The award of B Tech degrees in Biotechnology with syllabi (and teaching) that can hardly prepare students for a 'profession' has clearly established that sound 'business models' dominate the sphere of education. The three year undergraduate degree courses in the sciences immediately place graduating students at a disadvantage over the four year 'professional courses', since a further two year Master's program is imposed before students enter research programs leading to a Ph D. 'Integrated Ph D' programs are confined to a few institutions, where additional courses can be organized. There is also a differential scholarship in Ph D programs which distinguishes students from 'professional' and 'unprofessional courses'. Engineering today is hardly distinguishable from the sciences and there is a great need to reexamine the structure of undergraduate courses. While perceptions about college degrees in different subjects may account for declining interest in science courses, there are other issues regarding the teaching of science in schools that merit attention.

In attempting to promote interest in science education the Government has attempted many experiments. In all major policy interventions, in recent years, schemes to provide financial incentives have been the most visible. Money is a universal carrot and scholarships of various kinds are now available to entice students leaving school to enter undergraduate science courses. The Department of Science and Technology's Kishore Vaigyanik Protsahan Yojana (KVPY) program was a forerunner of the INSPIRE fellowship scheme; both of which have been growing in strength. New institutions, the Indian Institute of Science Education and Research (IISERs) that offer undergraduate science education in a research environment have been started. Old and venerable institutions like my own, the Indian Institute of Science, have started four year, flexible undergraduate programs which may motivate students towards careers in science. These experiments are only a small step and the verdict on the outcome of these initiatives will undoubtedly become known only years later. Schemes to provide research grants to colleges and poorly funded universities have been in vogue for some time, although it may still be too early to detect any discernable influence of these approaches to promote interest in science in colleges. While interventions at college and university level are discussed and some experiments to catalyse change are in progress, the scenario at school level is much less widely discussed. Some questions that might be worth considering are: (1) Do we teach too much or too little science in school? (2) Do we teach the right subjects or are curricula fossilised over time? (3) Does the teaching of science, constrained by a rigid examination system, inhibit students from studying these subjects at college level? (4) Do teachers face constraints that are not factored in by bodies that impose syllabi?

In thinking about these questions I was drawn to a critique of the manner in which children study science: 'There is a disconnect at the heart of the … education system that is having a devastating effect on how and what children learn. Research shows that the most meaningful learning takes place when students are challenged to address an issue in depth which can only be done for a relatively small number of topics in any school year. But the traditional process of setting standards tends to promote a superficial "comprehensive coverage" of a field whether it be biology or history, leaving little room for

in-depth learning. The curricula and textbooks that result are skin-deep and severely flawed.' Are these the words of an Indian commentator deeply concerned about school science education in India, where high school textbooks for the central school boards have now acquired formidable dimension? Indeed, some textbooks are so large and heavy that they seem more designed to improve weight lifting abilities rather than to kindle interest in apparently dry science subjects. Such books, often the product of a 'committee approach' to writing, leave teachers struggling to 'complete portions' before examinations. Curiously, the harsh assessment of curricula and textbooks refers to the school education system in the United States. The author, Bruce Alberts, is an eminent biologist, former President of the US National Academy of Sciences and currently editor of the journal *Science*. He is caustic when he says, 'the factoid-filled textbooks that most young US students are assigned for biology class make science seem like gibberish – an unending list of dry meaningless names and relationships to be memorized' (Alberts, B., *Science*, 2012, **338**, 1263). Reading this, I was relieved that nearly half a century ago the high school curriculum that I encountered contained little by way of science and seemed more focussed on history, literature, languages and geography, with mathematics standing alone. Large doses of facts and the importance of memorising science (rather like poetry) appeared in the college years, by which time choices had already been made and most escape routes closed.

The idea of a leaner curriculum advanced by Alberts may well be considered in India, where school textbooks have bloated to a level clearly designed to drive students away from science. Those who persist largely view school science as the passport to the 'professional' courses. Teachers may also welcome a reduction in the coverage of subjects like chemistry and biology which appear to be an unending collection of facts, presented in largely indigestible fashion. Teachers have little or no opportunities to participate in efforts to improve methods of science communication and to develop strategies for conveying, at least in part, the excitement that accompanies scientific discovery. Alberts notes that by the time students complete prescribed biology textbooks and the class based on them, 'it is clear they will know nothing of the kind of biology that inspires passion in the souls of scientists working in labs…'. If this is true in the United States, it is probably even more so in India, where curricula and examination systems leave little room for experimentation.

Teacher training, especially in the sciences and mathematics is an area of critical importance. While it is relatively easy to announce and formulate 'national missions' in the area of teacher education, implementation on the ground poses formidable logistical problems. Most importantly, there is a need to create a large cadre of 'trainers' who will undertake the task of teacher training. There is a great need to facilitate mechanisms by which school science teachers directly carry out modern experiments, eventually increasing the exposure of students to the role of experiments in science. There are wide variations in the quality of teachers in urban and rural schools and government and private schools. There are also considerable differences in the course content between state and central boards. Attempts to impose uniformity in standards have met with considerable reservation, if not outright hostility. While interventions at school level face many, apparently insurmountable obstacles, reforms in science courses at college and university level may be marginally easier to approach. A greater degree of autonomy in designing course content may be feasible at undergraduate level. Alberts argues that there is a 'need to replace the current "comprehensive" overviews of subjects with a series of in-depth explorations'. He suggests that this will require abandoning 'the one-size-fits-all textbooks… in favour of a large set of much shorter curriculum units, each designed to facilitate the active exploration of one important topic in depth for a month or so'. While Alberts' prescriptions are directed towards school education in the US, they may indeed be relevant for undergraduate courses in India, where early specialization and 'comprehensive coverage models' do not necessarily promote enthusiasm for science on a broad front.

The role of research project based learning in undergraduate science courses needs to be explored in India. This is nearly impossible within the framework of a three year program, but would be feasible in a four year course, bringing bachelor's degrees in science on par with the 'professional courses'. This is a model that has been frequently suggested but is only recently beginning to be implemented in isolated institutions. Experiments ongoing in the United States suggest that 'Inquiry Based Instruction' has proved very promising, promoting an interest in science well beyond the traditional classroom based instruction. Teams of undergraduates engaged in collective research projects learn 'skill sets necessary for scientists including communication, teamwork and the development of scientific ideas' (LaRue, C. C. and Padilla, P. A., *Science*, 2012, **338**, 487). The outcomes of experiments in research based learning are now being documented, with many examples where biology comes to life (Bascom-Slack, C. A. *et al.*, *Science*, 2012, **338**, 485). In a follow-up editorial, Alberts calls for a close collaboration between research scientists and teachers in developing 'new curriculum materials and teaching methods' that 'can help change the current trajectory of science education' (*Science*, 2012, **338**, 1396). If science courses are to be made attractive to students, scientists cannot sit on the sidelines and lament. They must actively engage, wherever possible, in experiments to transform science teaching in schools and colleges. Only the most highly talented can proclaim as Mark Twain did: 'Don't let schooling interfere with your education.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Remembering Ramachandran: Marking an Anniversary

January is a favoured month for scientific meetings in India. A recent conference in Bangalore was unusual in that it marked the 50th anniversary, a truly golden jubilee, of a scientific paper published from Madras University. The path breaking report introduced the Ramachandran Map into the literature of biochemistry and structural biology. The conference was intended to be a celebration of a past triumph; for some an occasion to discuss progress in the field, for others a time to recall and introspect. It is not often that a paper published from India has had as lasting an impact on a discipline as this contribution, circumspectly titled 'Stereochemistry of Polypeptide Chain Configurations', which first appeared in 1963 (Ramachandran, G. N., Sasisekharan, V. and Ramakrishnan, C., *J. Mol. Biol*., 1963, **7**, 95). This paper provided a remarkably simple and elegant approach to understanding the three-dimensional arrangement of atoms in the backbone of polypeptide chains, which lie at the heart of protein structures. The paper by Ramachandran *et al.* used the readily appreciated idea of atoms as hard spheres, defined by a finite radius, to suggest that non-bonded atoms in spatial configurations of molecules cannot approach closer than the sum of their radii (the van der Waal's radii long discussed in the literature of physics and chemistry). The contortions of a long polymeric, polypeptide chain were readily described using two variable parameters at each discrete monomeric residue. These parameters were the dihedral angles or torsion angles, defined by the positional coordinates of four sequentially linked atoms along the polymer backbone, denoted with the Greek symbols phi and psi. Structure space in three dimensions could then be analysed comfortably on a two-dimensional phi–psi plot. Using a hard sphere model Ramachandran *et al.* were then able to delineate clearly the regions of conformational (phi–psi) space, which were 'allowed' for amino acid residues in proteins. The 'contact criteria' used to demarcate allowed and disallowed regions were strikingly simple, brooking no violations. The phi–psi plot appeared when there were, for all practical purposes, no experimental observations from protein crystal structures. The myoglobin and hemoglobin structures were appearing at moderate resolution in the late 1950s, work for which John Kendrew and Max Perutz received the 1962 Nobel Prize for chemistry. When observations of residue conformations became available in the mid 1960s, it was apparent that amino acid residues did indeed largely obey the dictates of the Ramachandran map. In the decades that have followed, hundreds of thousands of individual amino acid phi–psi values have accumulated. In a striking vindication of the original Ramachandran construction, 'experimental maps' conform almost entirely to that originally outlined in the 1963 paper. Ramachandran died in 2001 at a time when his name had already entered the textbooks of biochemistry. He had effectively stopped working in the field about twenty years earlier. In a development which would undoubtedly have pleased him, the importance of Ramachandran's map remains undiminished with time, continuing to provide new insights as the technologies of structural analysis, primarily X-ray diffraction and NMR spectroscopy, advance to expand the database of protein structures. Molecular dynamics simulations, carried out on increasingly powerful computers, allow molecules to explore torsional angle space, Ramachandran space, in a manner that could scarcely have been anticipated in the 1960s. As members of Ramachandran's department in Bangalore (and I am one) basked in the warm glow of reflected glory at the recent symposium, I could not but help reflect on the man and his times.

G. N. Ramachandran came to the Indian Institute of Science in 1942 to study for a Master's degree in Electrical Engineering. C. V. Raman was the dominant presence at the Institute. When Ramachandran persistently tried to move to the physics department, legend has it that Raman facilitated the transfer by telling the Head of Electrical Engineering: 'I am admitting Ramachandran into my department as he is a bit too bright to be in yours' (Sarma, R., *Ramachandran: A Biography of the Famous Indian Biophysicist*, Adenine Press, New York, 1998). In the 1940s under the influence of Raman, Ramachandran began researches in optics and the emerging technique of X-ray diffraction. The year of India's independence, 1947, saw him submitting a Ph D thesis. Ramachandran was already marked by his contemporaries as a scientist of extraordinary promise. His passion for science is evident in letters written from England in 1947, to a friend from IISc and later a colleague at Madras University, S. Swaminathan who was to become an eminent organic chemist (reproduced in the biography by Raghupathy Sarma). In one, he asks Swaminathan: 'Are you working on

stereochemistry or some other branch? Did you try the cyclohexanone problem? What happened to the paludrine patent?' His early letters reveal a deep and instinctive appreciation of the importance of stereochemistry. Few students of physics in India (and indeed anywhere else) would have worried about stereochemistry and the cyclohexane problem in the mid 1940s, while pursuing research in optics and crystallography. Ramachandran, probably influenced by his mentor Raman, appears uncomfortable in England and less than enthusiastic about his scientific environment in Cambridge. In one of his early letters he says: 'I sometimes wonder whether Professor Raman was not right in advising me not to come here'. He ends a December 1947 letter with the salutation *Jai Hind*, undoubtedly influenced by the times.

Ramachandran's stellar career in India began at IISc in 1949 when he established one of the earliest groups in the country in the area of X-ray crystallography. He did not stay long; moving to Madras in the autumn of 1952 to become head of a newly established physics department, at the remarkably young age of thirty. The University had hoped to entice Raman, then in his sixties, who in turn suggested Ramachandran. A benign and supportive Vice Chancellor, A. Lakshmanaswamy Mudaliar backed the young star to the hilt facilitating the flowering of a department in a university, which remains unmatched in its accomplishment even decades later. Together with Gopinath Kartha, Ramachandran produced a model for the structure of the most abundant mammalian protein collagen, based on meagre fiber diffraction data and superb structural intuition. The collagen triple helix which appeared in 1953, and is accepted as essentially correct today, was in some ways an even more difficult structure to deduce at that time than the DNA double helix, which appeared a year earlier. A controversy with an English group led by Francis Crick soon followed which undoubtedly took its toll, but had one silver lining. Crick argued that the Ramachandran structure of collagen had unacceptable steric contacts between non-bonded atoms, setting in motion the work at Madras on optimal steric contact distances, which eventually laid the groundwork for the development of the Ramachandran Map. Looking back decades later, one cannot escape the feeling that in this apparently unequal contest, David does successfully prevail over Goliath.

In the 1950s and 1960s Ramachandran brought the world of science to Madras. His department became the leading centre in India and one of the most productive in the world in the areas of crystallography and biomolecular conformational analysis. Two major international meetings in 1963 and 1967 were attended by virtually most of the major figures in the field. There have rarely been conferences in India which have had such a major impact.

It is hard to create a good department or institution in India; it is however very easy to destroy the best rather quickly. Madras in the late 1960s, in a changing political climate, turned increasingly unfavourable for unfettered scientific research, compelling Ramachandran to return to the place where he had begun, the IISc. He began a new department which he named 'molecular biophysics', presumably to avoid confusion with radiation biophysics which had a strong presence in India at that time. The term 'structural biology' was yet to be invented. The department's subsequent rise to prominence, even after Ramachandran effectively left the field in 1979, was undoubtedly based on the strong focus of research in an area in which he had already established a major school. For the last two decades of his life, until his passing in 2001, Ramachandran was not actively engaged in the area in which he had made so many seminal contributions. He was largely absent from public view. Sadly, modern science often requires a constant reiteration of activity in a field. Isolation may have been primarily responsible for the fact that Ramachandran was not more widely recognised in India and abroad.

Ramachandran was a brilliant man. There were areas where he entered only briefly, but nevertheless produced fundamentally important insights. Three-dimensional image reconstruction, which today lies at the heart of tomography and the ubiquitous CAT scans and magnetic resonance imaging, was a field where his lone paper had a significant impact. He was among the first to recognise the importance of anomalous dispersion in X-ray diffraction, publishing a key paper in this journal in the early 1950s. There were other areas in which his forays were brief but important, but none quite matched the lasting impact of his work on the conformations of biomolecules. Ramachandran was a difficult man, as highly creative individuals often are. Temperamentally unstable, he was childlike and naïve, at times, in his enthusiasm for science; unreasonable, demanding and impatient at others. He brought to his research a clarity of thought which allowed him to see the core of a problem, unobscured by a wealth of irrelevant detail. In India Ramachandran's work, unrecognized by a Nobel prize, has never received the level of public acclaim accorded to many others. He remained a 'scientist's scientist' till the very end. Our Nobel fetish has ensured that Ramachandran's work is not more widely used as an inspirational model for students entering science. Curiously, scrolling down the Nobel foundation's roll call of honour I realised that many pieces of work have not stood the test of time. In retrospect, there is a sense of satisfaction that Ramachandran and his map will remain in the biochemistry textbooks for a long time to come. For those of us, whose paths crossed that of Ramachandran, the golden jubilee of his celebrated paper was an occasion to reflect on the man and his work. It was a rare privilege to have known him.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science and Social Science: The Spreading Tentacles of Biology

Biology is a discipline that bridges the 'soft' social sciences and the 'hard' sciences on a very broad front. Some practitioners of biology resemble physicists and chemists in their methods and approaches. Others favour methodologies, which resemble those of social scientists. Among the social sciences, economics is a subject where both rigorous mathematics and the somewhat fuzzier ideas of evolutionary biology have made an impact. If Malthusian thought influenced Darwin, he in turn influenced the economists of his time. More recently, geneticists and economists appear to be embroiled in a controversy, where the results of genetic analysis may have been overinterpreted, while examining the factors that drive national economies. A news report entitled 'Economics and genetics meet in uneasy union' draws attention to a debate that has been sparked by the circulation of a study which appears to suggest a link between the genetic diversity of a country's citizenry and the success of a national economy. Such an attempted correlation is bound to raise hackles, inviting the inevitable charge of 'genetic determinism and racism' (Callaway, E., *Nature*, 2012, **490**, 154). The *Nature* report reminds readers of Stephen Jay Gould's famous admonition to those who are prone to correlate the uncorrelated: 'The invalid assumption that correlation implies cause is probably among the two or three most serious and common errors of human reasoning.' The key conclusions of the 107 page paper (economists appear to write significantly longer papers than scientists) are succinctly summarized in the *Nature* report: 'High genetic diversity in a country's population is linked with greater innovation … because diverse populations have a greater range of cognitive abilities and styles. By contrast, low genetic diversity tends to produce societies with greater interpersonal trust, because there are fewer differences between populations.' The report appears to extrapolate these findings to further suggest that, 'countries with intermediate levels of diversity, such as the United States, balance these factors and have the most productive economies'.

Many casual readers, and I am one, would instinctively recoil from a conclusion that genetic factors dictate economic fitness; a reminder of darker times when genetics seemed to be a weapon more often misused than used. The critics of the paper in the *American Economic Review* were quick to mount an offensive, charging that 'the suggestion that an ideal level of genetic variation could foster economic growth and could even be engineered has the potential to be misused with frightening consequences'. The critics, geneticists and biologists amongst them, appear to be overreacting. While flawed statistical methodologies may be involved, many more factors presumably contribute to the interpretation of the data. The authors defend themselves by responding that critics have only a 'superficial understanding of the empirical techniques involved', adding that the study is unrelated to the perennial 'nature or nurture' debate. In an editorial comment *Nature* notes that for economists and geneticists to collaborate fruitfully 'both sides must take seriously the standards, methodology and history of the other'. Economists attempting to carve out a new niche, sometimes called 'genoeconomics', may underestimate the complexity of genetic data. They may also forget that 'human geneticists bear the historical scars of eugenics, and more recent accusations of insensitivity to indigenous populations'. Can geneticists learn from the economists? Undoubtedly, the social scientists have developed their own quantitative tools and are, as *Nature* notes, 'experts in measuring human behaviour both individual and collective' (*Nature*, 2012, **490**, 144).

If biology can influence economics, can politics be far behind? Indeed, two weeks after highlighting the fracas between economists and biologists, *Nature* carried a news feature on 'The Anatomy of Politics'. The article draws attention to the fact that 'an increasing number of studies suggest that biology can exert a significant influence on political beliefs and behaviours'. An interesting suggestion follows: 'And shrewd politicians might be able to take advantage of those biological levers through clever advertisements aimed at voters' primal emotions' (Buchen, L., *Nature*, 2012, **490**, 466). What are these 'biological levers' that politicians must learn to operate? Buchen highlights a number of reports in the literature of biology and social sciences that appear to link political positions and social attitudes to genes and hormones. Once again, studies in this area are calculated to remind observers of the dark days when genetic determinism seemed to be a malignant force in human affairs. While US studies focus on views that are 'right' or 'left', conservative or liberal, similar studies may be harder in larger democracies like India, where religion, language and caste become additional terms in a complex equation.

The first suggestions that genes may be linked to the positions people take on social issues appeared in the mid 1980s and were largely ignored. More recently, in 2005, studies on data from twins appear to have stirred interest in a field where biology intrudes into politics. If our political and social beliefs are written into our genomes, the hunt for candidate genes must inevitably follow. The *Nature* report cites studies on 'genes involved with the olfactory system and the neurotransmitters glutamate, dopamine and serotonin' which 'have all been linked to behaviours such as voter turnout and ideology'. Curiously, these reports have appeared in journals which must be as far removed from biology as can be; *Journal of Politics* and *Journal of Political Psychology*. Are such studies reproducible? Apparently not, as a comment in the *Nature* report indicates: 'The reviewers have not been familiar with the replication problems that exist'. Biologists who have struggled to identify the genetic markers for disease must undoubtedly view the entry of political scientists into genomics with a considerable degree of skepticism.

Genes only provide a blueprint for complex physiological mechanisms. Biochemistry translates the genetic message into action. Can the body's chemistry control reactions to specific situations and influence attitudes? After all, a surge of adrenalin often comes to our aid in difficult circumstances. Recent research suggests that glucocorticoids, steroid hormones released by the adrenal gland, may act as mediators of social stress and influence behaviour. The finding of 'surprising neuro-anatomical specificity' promises 'therapeutic approaches that could intervene and restore normal behaviours' (McEwen, B. S., *Science*, 2013, **339**, 279). While studies in mice may take a long time to translate into effective therapeutic intervention in humans, the unmasking of the mechanisms that determine behavioural responses to stress is clearly a first step along a long road. The connection between hormones and the environment has long been recognized. Indeed, the word 'chemistry' is widely used to describe social interactions. For sociologists interested in human behaviour an old hormone, oxytocin, has made a dramatic reappearance. Oxytocin, a peptide hormone synthesized by the hypothalamus and secreted by the pituitary gland has been widely used in clinical practice to induce uterine contraction. The molecule's synthesis in the laboratory was a landmark, recognized by the 1955 Nobel Prize to Vincent du Vigneaud. Half a century later, oxytocin is being touted as a potential behaviour modifier and possibly a potential therapeutic for treatment of psychiatric disorders. A news report in *Science* begins by alluding to a string of 'breathless' media reports which 'have credited this hormone with promoting the kind of teamwork that wins World Cup soccer championships and suggested that supplements of the peptide could have prevented the dalliances and subsequent downfall of a certain high ranking US intelligence official' (Miller, G., *Science*, 2013, **339**, 267). The *Science* report draws attention to 'a genuine and infectious excitement among many scientists about the hormone's role in social behaviour', an area far removed from its primary function 'in childbirth and lactation'. The interest in oxytocin appears to stem from studies that suggest a role for oxytocin in promoting 'trust and cooperation' and in making 'people more attuned to social cues'. While many psychiatric disorders have become targets for oxytocin, autism appears to be a condition where administration of the hormone may be worth investigating. Unfortunately, several ongoing studies may be throwing up results that argue for a more cautious approach in clinical trials. The effects of oxytocin in humans may vary with a 'person's genetic makeup and psychological status'. Large scale studies are underway where the potential benefits of oxytocin in autistic children are being explored; the results of which will be eagerly awaited. Animal studies using prairie voles as subjects are throwing up results that the short term beneficial effects of oxytocin in promoting bonding with a mate may be offset by a reversal of behaviour in the long term.

Social scientists may be drawn to a report that appeared a couple of years ago that declared, 'oxytocin promotes human ethnocentrism' (De Dreu, C. K. W. *et al.*, *Proc. Natl. Acad. Sci. USA*, 2011, **108**, 1262). Using clever experimental design, De Dreu *et al.* assessed the effects of oxytocin in modulating social attitudes in studies involving a large number of participants. Their results point to the role of oxytocin in enhancing ethnocentrism – 'the tendency to view one's group as centrally important and superior to other groups'. This is a quality which 'creates intergroup bias that fuels prejudice, xenophobia and intergroup violence'. Clearly, De Dreu *et al*. draw a diametrically opposite conclusion to those who have called oxytocin a 'love drug' or 'cuddle chemical'. While oxytocin promotes cooperation within groups ('in group favouritism') it is clearly capable of enhancing intergroup prejudices. These authors 'suggest that oxytocin has a role in the emergence of intergroup conflict and violence'. While the role of hormones in influencing the 'social brain' is being increasingly recognized, cognition and physiology may also be linked. A recent report which must attract political scientists is engagingly titled: 'The political left rolls with the good and the political right confronts the bad: connecting physiology and cognition to preferences' (Dodd, M. R., *Philos. Trans. R. Soc*., 2012, **367**, 640). Such studies attempt to provide a biological basis for 'at least a part of our political differences', suggesting that physiology and cognition conspire to 'lead people to experience the world in fundamentally different ways'. In a country where political and social conflicts dominate everyday discourse, we might be comforted by the thought that as the tentacles of biology spread into the social sciences, greater understanding of human behaviour may help in promoting tolerance.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Open Access: Tearing Down Barriers

Academic scientists are judged by the papers they publish. The pressure to publish in the peer-reviewed literature of science has grown tremendously over the last thirty years or so; a period that has coincided with a rapid increase in global investments on scientific research. Individuals, institutions and even countries attempt to maximise published output, with the result that there has been an almost unmanageable increase in the number of scientific journals that are published today. The technologies of the information age have contributed to this explosion of scientific literature as many new journals appear only in electronic form, while many old journals move towards dispensing with the traditional, printed form. Journals have always needed money for production. A long time ago, when scientific societies and academies were the sole publishers, many ventures functioned like 'not-for-profit' organisations, performing a community service. Editors and reviewers were unpaid volunteers, growing in number as science expanded. Science publishing as a commercial enterprise began to take root in the 1960s and grew very rapidly in the following decades. Traditionally, libraries paid for journals and institutions largely bore the costs entailed. The community of academic readers was generally unaware of the expenditure incurred. The growth of large publishing monopolies, the proliferation of journals and their rising cost have led to a continuous escalation in library budgets at major institutions across the world. A Harvard faculty notice is succinctly titled: 'Major periodical subscriptions cannot be sustained.' The April 2012 circular notes that 'many large journal publishers have made the scholarly communication environment fiscally unsustainable and academically restrictive'. Curiously, while the best of universities in the West have been alarmed at the runaway rise in library costs, Indian institutions have displayed little interest in asking whether their libraries get fair value for the money spent. The sudden spurt in the number of new science and engineering institutions in India, IITs and IISERs amongst them, has provided the large publishing houses with an expanded market in India. The pricing policies for scientific journals can be even more capricious than domestic airline fares. The transition from the printed journal to the electronically accessible product through the medium of the internet has facilitated 'bundling' on a scale that could scarcely be imagined a decade ago. Publishers can now offer access to over a thousand journals with a single omnibus subscription. There are very, very few institutions, if any, in India where such 'bundling' strategies are advantageous. The sheer number of journals in the 'bundle' is used by publishers to sell the notion of a relatively low cost per title. Librarians in our institutions are still coming to terms with this changing scenario. The new institutions, often consisting of a handful of faculty and students, are saddled with a base subscription, when they enter consortia of libraries, ensuring that future escalations are based on a high starting level. While university libraries abroad have reacted in an organized manner to counter the alarming costs of providing access to information, there is little evidence of concerted action in India.

Should access to the results of scientific research be restricted by issues of cost and copyright? Ideally, access to scientific papers should be free. After all much of academic research is supported by public funds and it appears reasonable that the results of research should be freely available. This argument is indeed the *raison d'etre* of the 'Open Access' movement, which has grown in strength and diversity over the last few years. Open access (OA) advocates have been tireless and articulate, championing a cause that does not always appear to resonate with practising scientists. Most researchers are driven by the desire to publish their work in journals which have the highest possible impact and are rarely concerned about the accessibility of their articles to readers, who are unable to pay for the journals. Scholars who have been interested in issues of access to the scientific literature have been quick to point out that the publishing industry, which imposes restrictive copyright practices and promotes monopoly pricing, relies almost entirely on authors, referees and editors who contribute their time and effort voluntarily. The practice by which authors, supported by public funds, transfer copyright to a commercial publisher appears heavily weighted in favour of publishers. In recent times, the continuing financial success of academic publishers, in times of recession, has attracted attention. *The Economist* noted sometime ago: 'Academic journals generally get their articles for nothing and may pay little to editors and peer reviewers. They sell to the very universities that provide the cheap labour…. For more than a decade the dominant model has

been the "big deal". Publishers sell access to large bundles of electronic journals for a price based on what colleges used to pay for paper ones. Prices of big deals rise at about double the rate of inflation' (*The Economist*, 26 May 2011). The excessive profitability of the large publishing businesses, even in difficult times, must inevitably attract adverse attention.

The 'Open Access' movement now draws its adherents from different constituencies. The early champions appear to have emphasized the authors' right over their work, arguing against the copyright transfer clauses of journals. This group advocated, in compelling fashion, the virtues of 'open archives', which use the technologies of the internet to make the literature accessible. Well maintained, freely accessible repositories coupled with the power of search engines appear to provide a readily, implementable solution. A second group, which may have emerged later, is driven by rising journal costs. This group is responsible for launching 'Open Access Journals' as a financially viable alternative in science publishing; encouraging a transition from the traditional 'reader (library) pays' model to an 'author (funding agency/institution) pays' model.

Most researchers I have encountered (and my experience is largely limited to India) have little interest in the 'open access' debates. As writers they would, of course, like to publish in the highest impact journals, regardless of issues of access. As readers they would like ready access to any literature that they seek, invariably demanding that institutions ensure access even to journals that are exorbitantly priced. The open access advocates have had little impact on the scientific community in India. Seminars discussing open access issues are invariably affairs in which the evangelists preach to the converted. The vocabulary of the open access movement is often unfamiliar. I was therefore pleasantly surprised to stumble across a book, *Open Access* by Peter Suber, which is an excellent and easily readable primer on the movement to make the results of scholarly work freely available. The author's preface is engaging, urging readers to plunge on: 'I want busy people to read this book. OA benefits literally everyone, for the same reason that research benefits literally everyone.' Suber is clear 'that the largest obstacle to OA is misunderstanding. The largest cause of misunderstanding is lack of familiarity, and the largest cause of unfamiliarity is preoccupation. Everyone is busy. There has been organized opposition from some publishers, but that has been a minor impediment by comparison'. His remedy for misunderstanding 'is a clear statement of the basics for busy people'. I believe the book will serve this purpose admirably. The author notes that the book itself will be freely available, a year after publication (*Open Access*, Peter Suber, MIT Press, Cambridge, Ma, 2012). This is a book that must be read by those busy scientists who publish a lot, read a lot and have had little time to grasp the nuances of the open access movement. It must also be read (and read carefully) by strident advocates, who have little time to allay the fears of those unfamiliar with the issue. I have often heard the cry in India that 'government must mandate' that scientists publish their work in open access journals. This is a dangerous call, which may create an atmosphere of government control, which is likely to stifle scholarly work. The book must undoubtedly be read by those who oppose open access. A little understanding may dispel many fears.

Stevan Harnad, a pioneer of the OA movement called the approach which uses repositories (often institutional) and voluntary self archiving as '*green* OA'. Open access 'delivered by journals regardless of the business model' is termed as '*gold* OA'. Green OA can be delivered if there is institutional will and community participation. Gold OA would, of course, be the most desirable. Many commercial publishers appear to be veering around to providing access, sometimes using an author fee and at other times using a time delay. As publishers and OA advocates continue their skirmishes, governments have begun to use the power of legislation to force delayed access after publication of publicly funded research. Both the United States and Britain already have legislation in place, which will undoubtedly be fine tuned in future. There is a need for India to revisit the area of copyright as it pertains to scientific research publications and to promote open access initiatives in a manner that does not impede the ability of scientists to publish in journals of their choice.

The open access wars have provided an opportunity for the rise of a new breed of publishers, who sense a commercial opportunity in the 'author pays' model of scientific journals. A recent commentary charges that 'predatory publishers are corrupting open access'. I was struck by an unflattering reference to India: 'Perhaps nowhere are these abuses more acute than in India, where new predatory publishers or journals emerge each week. They are appearing because of the market need – hundreds of thousands of scientists in India and its neighbouring countries need to get published to earn tenure and promotion' (Beall, J., *Nature*, 2012, **489**, 179). The author argues that in the world of predatory publishing, peer review is no longer an important step in maintaining both quality and integrity. He is critical of open access advocates who 'overlook the importance of validation in online publishing'. Open access conflicts have also led to the emergence of 'extremists' who practice what Suber terms as 'vigilante OA' or 'piratical OA'. The hauntingly sad story of Aaron Swartz who was young, brilliant and passionately committed to open access illustrates the importance of recognizing that the 'age of the internet' has profoundly altered the way in which access to information is perceived. Swartz committed suicide after being prosecuted for 'retrieving 4.8 million documents from JSTOR, a fee based repository' (*The Economist*, 13 January 2013). Future generations may indeed dispense with restrictive practices altogether. Access barriers, like the Berlin Wall, will inevitably be torn down.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Bonds: Chemical, Hydrogen and Others

The chemical bond is central to chemistry. The line that connects the symbols for the elements signifies a profound and intimate union of atoms to create molecules. Chemical imagery conjures up the 'electron pair bond', an idea, due to G. N. Lewis, whose appearance in 1916 dramatically transformed 20th century chemistry. Bonds could now be made, broken and moved about by agile electron pairs, as molecules transformed and reacted. At times, pairs bound in covalent union could be separated; a divorce that created free radicals – singles searching for partners. For students of chemistry a course on 'chemical bonding' seems almost as essential as 'classical and quantum mechanics' for physicists and genetics for biologists. Curiously, a couple of years ago the journal *Nature* ushered in the 'Year of Chemistry' with a provocative commentary, catchily headlined 'Beyond the bond'. Readers were attracted with the declaration that 'new techniques show the bond to be a convenient fiction, albeit one that holds the field of chemistry together'. The author, Philip Ball, quoted Charles Coulson whose 1952 text *Valence* was required reading for those seeking a theoretical understanding of chemistry's most enduring image: 'A chemical bond is not a real thing: it does not exist: no one has ever seen it, no one ever can. It is a figment of our own imagination' (*Nature*, 2011, **469**, 26). Why do chemists persist with an idea that theoreticians caution is a poor representation of reality? Ball answers: 'The bond is literally the glue that makes the entire discipline cohere; to consider it an objective reality is necessary for any kind of discourse on chemistry'. In raising questions about chemistry's theoretical foundations, Ball adds: 'The discipline is in fact riddled with such convenient (and contested) fictions, such as electronegativity, oxidation state, tautomerism and acidity'. Having studied and taught many of these 'convenient fictions', I was consoled by the thought that drifting away into the fuzzier world of biology had provided me with some relief, from the predatory assault of theoreticians and sophisticated experimentalists, who seemed intent on destroying a familiar and comfortably fuzzy view of chemistry. My spirits further lifted when I came across a 'definition of the hydrogen bond' by an international team of 14 chemists, who had been commissioned by the International Union of Pure and Applied Chemistry (IUPAC). For the general reader, I might add that IUPAC is to chemistry what FIFA is to football and the ICC to cricket. Even as Ball was raising doubts about the covalent chemical bond, here was chemistry's highest body laying down definitions of a much weaker bond in a magisterial report, suggesting that chemistry's fictions were here to stay (Arunan, E. *et al.*, *Pure Appl. Chem.*, 2011, **83**, 1619–1636; *ibid*, 2011, **83**, 1637–1641).

The 'hydrogen bond' is an old concept which is crucial to our understanding of the properties of substances as diverse as water and DNA. The chemistry of heredity is determined by the hydrogen bonds that hold DNA strands together in the double helix; Watson–Crick base pairs are now common knowledge even in high school. The historical origins of the hydrogen bond have always been the subject of debate. Most students of chemistry associate the idea of the hydrogen bond with Linus Pauling, arguably the most influential chemist of the 20th century. Pauling's 1939 classic, *The Nature of the Chemical Bond* taught generations of students everything that seemed worth knowing about chemical bonds, strong and weak. His success in using hydrogen bonds as a dominant force in determining fibrous protein structures revealed the helical structures of polypeptides, a forerunner of the DNA structure. But, Pauling was not the man behind the idea of the hydrogen bond. A perceptive colleague drew my attention to a recent 'thesis' by Patrick Goymer, which appeared to mark an anniversary – '100 years of the hydrogen bond'. The author notes that Pauling drew attention to an 'unremarkably sounding study' that appeared in 1912 (Goymer, P., *Nature Chemistry*, 2012, **4**, 863). My curiosity was aroused, prompting a quick search that led me to the paper by T. S. Moore and T. F. Winmill modestly titled 'The state of amines in aqueous solution' (*J. Chem. Soc. Trans*., 1912, **101**, 1635). This long and detailed study of the ionic conductivity of amines in water is filled with forbidding tables and has no specific mention of a 'hydrogen bond'. But there is a structure and a mention of a 'union', both of which suggest that these uncelebrated authors were indeed on the verge of uncovering a weak interaction between molecular species. Could the 'hydrogen bond' have met with a warm reception even before the Lewis electron pair covalent bond? Probably not. Inevitably, the origins of the hydrogen bond are traced today to a 1920 paper by Latimer and Rodebush (*J. Am. Chem. Soc.*, 1920, **42**,

1419), which appeared from the chemistry department at Berkeley. A footnote in this paper tantalisingly provides an earlier reference to unpublished work by Maurice Huggins, then a first year graduate student. A wonderfully readable account of the reception of the concept of hydrogen bonding by the chemical community between 1920 and 1937 by Denis Quane, notes that this 'unpublished work' was actually an undergraduate 'term paper' written in May 1919 (Quane, D., *Bull. Hist. Chem*., 1990, **7**, 3). In later years Huggins would indeed attempt to provide his version of the historical record (Huggins, M. L., *Angew. Chem. Int. Ed. Engl*., 1971, **10**, 147). Quane notes that the first use of the term 'hydrogen bond' may have appeared in G. N. Lewis' 1923 book, *Valence and the Structure of Atoms and Molecules*. The emerging New World ideas on bonding in chemistry were not always welcomed in the Old World. Quane notes that Henry Armstrong, a conservative British chemist, was scathing in a letter to *Nature* in 1926, describing the hydrogen bond as a formula in which 'hydrogen figures as a bigamist'. He dismisses G. N. Lewis as a 'Californian thermodynamiter, who has chosen to disregard the fundamental canons of chemistry – for no obvious reason other than that of indulging in premature speculation upon electrons as the cause of valency'. Between 1920 and 1937 bonds in molecules were debated intensely, with Pauling emerging by the end of the 1930s as the figure who unified and clarified many disparate ideas of molecular structure.

The centenary of the largely unknown Moore–Winmill paper has passed unnoticed. Can chemists and chemistry find a more appealing cause to celebrate? After all, this is a discipline sorely in need of favourable public attention. 2016 which marks 100 years of the most enduring 'fiction' in chemistry, the electron pair bond, may be a strong candidate. Bonds of every kind – covalent, ionic, hydrogen and even the recently suggested 'halogen' bond – could be highlighted in a celebration of the way in which atoms unite in molecules and molecules unite in chemistry and biology. In reading about the hydrogen bond I was reminded of a commentary that addressed 'the precise chemical nature of this unique interaction'. This essay has a striking title: 'The name is bond – H bond' (Martin, T. W. and Derewenda, Z. S., *Nature Structural Biology*, 1999, **6**, 403). The line is, of course, a variation from a(nother) work of fiction: 'The name's Bond. James Bond.' Curiously, even as we wonder whether chemists have allowed a 'hydrogen bond centenary' to pass unnoticed, the world of entertainment celebrated the 'Bond' golden jubilee with considerable fan fare. A new James Bond film, an interlude at the Oscars ceremony and saturation screening of Bond films on television marked the 50th anniversary of the appearance of the first movie, *Dr. No* in 1962. Another anniversary can be celebrated this year; the 60th year since the first of Ian Fleming's books, *Casino Royale*, appeared in 1953. Considering these dates reminded me that a long time ago, in my youth, chemistry's 'bonds' seemed hard to understand and, at times, forbiddingly dull; a characteristic that seemed to be shared with works of non-fiction that students were compelled to read as part of general education. Fleming's fictional 'Bond' was infinitely more entertaining. In writing this column I cannot help a feeling of guilt; is even a mention of James Bond appropriate on the generally stodgy pages of a scientific journal? Was Fleming's work even appropriate for scholarly study by social scientists and those concerned with popular literature. I was heartened to note that Indiana University, Bloomington, far removed from the glamorous locales where Bond practiced his craft, hosted a symposium titled, 'Ian Fleming and James Bond: The Cultural Politics of 007' (Hitchens, C., *The Atlantic*, April 2006). Is there any connection between the most 'fictional Bond' and science, apart from the unending stream of 'innovative technology for spies' that appear in the films? A quick search on the internet produced Richard Zare, a Stanford physical chemist of considerable distinction and infectious enthusiasm, explaining Bond's preference for dry martinis, 'shaken not stirred'. The physical chemistry of mixing ice cubes and alcohol left me, a teetotaller, mildly bemused. A more direct connection may be found in the origin of 007's name. Fleming, a bird watcher, borrowed the name of a decorated, ornithologist, James Bond (1900–1989), an authority on Caribbean birds and author of a 1936 book on the *Birds of the West Indies*. His obituary in the *New York Times* (17 February 1989) quotes Fleming: 'It struck me that this brief, unromantic Anglo-Saxon and yet masculine name was just what I needed and so a second James Bond was born.'

Hunting for the origins of names and terms can be an entertaining exercise. Even the apparently prosaic 'fictions' of covalent bonds and hydrogen bonds provide human drama as we trace their roots. Maurice Huggins, fifty years after his undergraduate term paper, lectured at the Royal Swedish Academy in Stockholm in 1969. G. N. Lewis died mysteriously in his laboratory in 1946, his body found next to a leaking hydrogen cyanide line. He had returned from lunch with a long time rival Irving Langmuir, who received a Nobel Prize in 1934. Lewis was nominated over 30 times but never rewarded. A 2006 study that revisits the circumstances of Lewis' death, concludes that he died of a 'broken heart'. Is there a moral to the story? Michael Kasha, a student of Lewis in the 1940s provides one: 'We try to teach our students to be generous, to be honest, to give other people credit and don't expect any prizes – because you're not likely to get them' (Delvecchio, R., *San Francisco Chronicle*, 5 April 2006). Pauling's wife, Ava Helen, said it well at an International Congress in Yugoslavia in 1957: 'The only bond more important than the hydrogen bond is the human bond.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Gold: Macroeconomics and Nanoscience

Early March is always dominated by the Finance Minister's presentation of the Annual Budget in Parliament, made invariably on the last day of February; a ritual that appears to have few parallels elsewhere in the world. Budget watchers, many of whom should know better, wait with bated breath for the policy announcements that may revive the flagging spirits of the economy. Would there be new proposals that might inject even a limited dose of Keynsian 'animal spirits', alluded to by the Prime Minister not too long ago? As a curious, but completely ignorant bystander, I have always been struck by the fact that even a generally excitable group of parliamentarians appears to subside into thoughtful silence, as Finance Ministers over the years have quoted dry statistics and flowery poetry in long speeches, while experts wade through the voluminous budget proposals seeking evidence for fresh initiatives. This year was no exception. The poets may have been less evident and the deficits more prominent, but there was one sentence that caught my eye: 'The household sector must be incentivised to save in financial instruments rather than buy gold.' The Finance Minister seemed weighed down by the growing imbalance between the import and export of the total of goods, services and transfers that contributes to the country's rising debt. He was candid: 'My greatest worry is the current account deficit (CAD). The CAD continues to be high because of our excessive dependence on oil imports, the high volume of coal imports, our passion for gold and the slowdown in exports.' Why had the Finance Minister turned his guns on gold?

Trained as a chemist and having drifted away to the fringes of biology decades ago, I have never had even a passing interest in the precious metals. For me, elemental gold and its relatives have always been tucked safely out of sight, in the recesses of Mendeleev's Periodic Table. My interest in gold was sparked, momentarily, a few months ago by an announcement that a university would reward its stellar students with gold medals, each containing 5 grams of 'pure gold'. The unstated implication was that the medals of yesteryear were contaminated or probably largely composed of baser metals. Shortly thereafter, I was reminded that I too was the recipient of a gold medal, undoubtedly conferred by an error of judgement, which had remained uncollected for several years. The organization was kind enough to suggest that it might be sensible to collect the medal as it was fashioned from an unspecified quantity of gold, presumably of a desirable purity. I remained disinterested until the Finance Minister and the March issue of the journal *Nature* aroused my interest in gold. Indeed, in his post-budget interactions with the press and the public, the Minister seemed almost wistful: '…if we do not import gold for one year, half of our current account deficit will disappear.' The Minister's aversion for gold appears, to an uninformed observer like me, to stem from the fact that gold imports in India lead to an excessive outflow of foreign exchange. The imported gold disappears into private hands in widely dispersed hoardings, which contribute negligibly to economic growth. Vanity and a well honed hoarding instinct contribute to India's insatiable and unsustainable desire for gold. The journal *Nature* in its special supplement on 'Gold' provides a very broad overview of the international scenario on production and consumption, even while drawing attention to state-of-the-art applications in science and technology of a metal that has always occupied centre stage in human affairs.

The prefatory editorial note, introducing the special section in *Nature* (2013, **495**, S1), reminds us of the 'celebrated place' that gold (element 79 in the periodic table) occupies in 'the history of science and technology'. Michael Faraday experimented with colloidal gold in experiments that today would have qualified as nanoscience (and even possibly, nanotechnology). Ernest Rutherford ushered in the age of atomic physics and chemistry, by bombarding a slice of gold foil with alpha particles and interpreting his results in terms of a nuclear model, that forever altered our view of atoms. More recently, the electronics revolution has been driven by the elements. While silicon occupies the pride of place, gold has played a critical but unobtrusive role. The supplement's editor, Herb Brody, reminds us of gold's place in language: 'Its contribution to metaphor alone is legion – who would want a therapy that is, let us say, the silver standard.' Gold's electronic structure, rarely discussed in chemistry courses, is responsible for its characteristic colour, the golden hue which has captivated humanity for millennia. Most remarkably, gold can play tricks with light; a property that lies at the heart of the ongoing photonics revolution. Neil Savage draws attention to a chalice, the Lycurgus Cup displayed in the British museum, which has a jade colour in ambient light, but glows bright red when light shines through it. The artisans who crafted the goblet embedded 'tiny beads of gold

and silver, about 70 nanometres in diameter' into the glass, creating an artefact, which clearly must find a place in the pre-history of nanotechnology. The properties of gold, as the metal is shrunk to nano-dimensions, are truly spectacular. Light striking surfaces with nanometer dimensions orchestrates a symphony of gold's electrons, resulting in the phenomenon of surface plasmon resonance which drives much of modern research in photonics (Savage, N., *Nature*, 2013, **495**, S8). Gold nanoparticles appear to be a 'goldmine' for research in the areas of solar energy conversion and nanoelectronics and in building metamaterials 'that seem to defy common sense'. Gold, more precisely 'nanogold', has been tantalisingly promising in the area of catalysis. The ability to produce truly nanoscale gold particles often containing a well defined number of atoms (clusters) fuels much of the excitement in the applications of gold in nanotechnology. Mark Peplow traces the history of gold particle mediated catalysis to the late 1980s, when Japanese chemists discovered the catalytic conversion of noxious carbon monoxide, produced in automobile exhausts, to the more benign (although I suspect some may disagree) carbon dioxide (*Nature*, 2013, **495**, S10). Since then, gold catalysts periodically surface in the literature, although there are few major successes. For the mechanistically inclined, the idea that gold atoms at the 'corners' of nanoparticles are catalytically competent has triggered the hunt for smaller and smaller, viable gold clusters.

Western medicine has always been wary of heavy metals, advocating a cautious approach towards therapeutic mixtures used in some forms of traditional medicine which contain apparently significant amounts of toxic elements. Gold nanoparticles have entered modern biomedical research through the exploding field of diagnostics; gold's ability to change colour has been widely exploited for detecting molecules and organisms. An area that currently appears to be attracting increasing interest is the ability of gold nanoparticles to act as transporters for drugs. Disguising the gold particle surface with nucleic acids is a stratagem that is being explored for transporting them 'into the cytoplasm without eliciting an immune response'. Cancer therapy, where distinctions between malignant and normal cells are essential, is an area where gold nanoparticles are being touted as the vehicle for the next advance on a very difficult front. Toxicity issues rear their head with predictable frequency, but growing evidence suggests that gold nanoparticles may be safer than the critics think (Weintraub, K., *Nature*, 2013, **495**, S14). In many technological applications of gold, cost issues invariably arise. The very factors which make gold the central element of the world economy may mitigate against its widespread use in the devices of the future.

The trouble with the metals is that they must be mined and the ore processed by procedures which are far from being environmentally benign. The extraction of gold using procedures that employ cyanide can leave the mining countryside devastated. Worldwide, the annual production of gold is miniscule compared to some of the other elements. 2011 figures, cited in the *Nature* Supplement, reveal that 2700 tonnes of gold were produced as compared to 8 million tonnes of silicon, 44.1 million tonnes of aluminium and 16.1 million tonnes of copper. Clearly, gold production is controlled and reserves carefully protected. The top ten producing countries are China, Australia, USA, Russia, South Africa, Peru, Indonesia, Canada, Uzbekistan and Ghana. The unrestrained passion for gold in India makes this country one of the largest importers of gold, much of it finding its way into private hoards. India comes in at the 11th position as far as 'official gold holdings' are concerned, a position that may not entirely please those who manage the country's finances. In times of economic crisis and in the face of global economic competition, 'physical gold holdings constitute a crucial geopolitical variable' (Chossudovksy, M., 2013; http://www.globalresearch.ca). It may be of some interest to ask how physical gold holdings held by central banks are audited. India's vast unaudited and hidden hoards of private gold may contribute negligibly to national wealth. With the increasing use of gold in electronics and the dramatic growth in the use of the many devices of the information age, it has become relevant to address issues of recycling. Encouragingly, the *Nature* overview suggests that for the period 2007–11 mining contributed 61.4% of gold produced while 37% came from recycling procedures. Only a small amount, 1.6%, was attributed to releases from governmental reserves. A fact that might surprise some, as it did me, was that 'the "urban mine" of electronic waste – old computers, mobile phones and the like is far richer than natural deposits; a typical open-pit mine will yield between 1 and 5 grams of gold per tonne, but mobile phone handsets can contain up to 350 grams per tonne gold and computer circuit boards up to 250 grams'. Recycling procedures appear to be becoming sophisticated and efficient and most importantly, environmentally friendly (Owens, B., *Nature*, 2013, **495**, S4). Inevitably, there is the hope that biology will trump chemistry. Microorganisms provide promising leads for leaching gold and for producing nanoparticles via a biological route (Gwynne, P., *Nature*, 2013, **495**, S12).

Gold prices have been rising exponentially over the last few years; undoubtedly an outcome of global economic turbulence. While importing oil may be an essential necessity, paying exorbitantly for gold to satisfy private vanity appears to be a path that is unlikely to help an ailing economy. Can vanity be moderated? Shakespeare may have provided wise counsel in the *Merchant of Venice*. Portia's suitor, the Prince of Morocco, chose a gold casket in which he hoped to find her portrait. Instead he found the picture of Death, with a message:

'*All that glisters is not gold,*
Often have you heard that told,
Many a man his life hath sold,
But my outside to behold,
Gilded tombs do worms enfold.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Should We Produce More Ph Ds?

A doctorate, a Ph D degree, confers on its recipients an aura of academic accomplishment that is prized. Honorary doctorates are sought after by men and women in public life; at times, leading to unseemly controversies at the annual convocations of our universities. Ph D degrees are generally acquired after several years of research, whose outcome is documented in a thesis or dissertation; often a document of considerable size. Students, who choose to plunge on towards a Ph D after graduation from college, face a long haul, which requires commitment, concentration and resilience. Research can, at times, be slow, unpredictable and frustrating, requiring students to deal with long periods when no fruitful results appear forthcoming. Ph D research also ties students to supervisors ('guides' is a term favoured in India), whose temperaments and commitment to the task at hand can be widely variable. In large research groups or in surroundings where an academic ambience is fostered, as in many research universities, Ph D students often learn more from their peer groups than from their faculty advisors. The student–supervisor relationship can be delicate; easily shattered when personal differences arise. In the best of cases, students bond to supervisors in a manner that can be heartwarming; there is indeed one debt that can never truly be repaid – the intellectual debt that students forever owe their teachers. In retrospect, most middle aged and aging academics look back on their Ph D days, with a degree of wistfulness; a period when youthful enthusiasm infected everyday research. For experimentalists 'working at the bench', directly observing the unexpected is a pleasure that can rarely be experienced, with advancing age and extraneous responsibility. For the theoreticians there is undeniable satisfaction when a calculation yields a result that provides a new insight into a nagging and recalcitrant problem. Ph D students often lead a life that is almost completely tied to their laboratories. Time can pass almost unnoticed as the years tick by.

In Indian institutions, Ph D students are the foot soldiers of research. Post-doctoral fellows and technicians are relatively rare; a marked contrast to Western laboratories. Graduate students (and post-doctoral fellows) in US laboratories have often been viewed as cheap labour, advancing research agendas of professors who run very large groups, with little time for individual mentoring. The period of a Ph D degree can stretch to several years. European and British Ph D degrees are somewhat less time consuming than American degrees. In our own institutions national scholarships can run up to five years, but the time taken for a Ph D degree is even longer in many institutions. As the head of an institution which has on its rolls the largest number of science and engineering Ph D students on a single campus in India, my attention has been repeatedly, and sometimes forcefully, drawn to the excessive lengths of time that appear to be needed to complete a doctoral degree. A mean time of about six years seems to be a somewhat uncomfortable norm. Even while being asked to reduce the time taken for the award of the Ph D degree, the institution has been repeatedly exhorted to increase the number of Ph Ds produced annually. In an environment where there is a growing reliance on quantitative metrics for assessing science, the number of Ph Ds produced appears to be gaining ground as a measure of national scientific activity. An article entitled 'The Ph D Factory' noted that 'the world is producing more Ph Ds than ever before. Is it time to stop?' (*Nature*, 2011, **472**, 276). The growing cry for 'more Ph Ds' is based on the unstated, and possibly unfounded, presumption that employment opportunities readily beckon the newly minted doctorates. In most discussions of institutional performance, I have usually played the role of the defendant in the dock, responsible not only for the inordinate length of time required to obtain a Ph D degree, but also for the less than satisfactory number of Ph Ds produced. In an arena where strong personal opinions and prejudices far outnumber hard facts, I have always found it difficult to counter the 'shorter Ph Ds, more Ph Ds' argument, with statistics to bolster my defence. Suggesting that the problem of Ph D timescales may be harder to solve than commonly imagined, appears to be an admission of both incompetence and ineffectiveness. Are there facts available which might help to provide some insights into the problems of doctoral education? Mark Twain famously remarked: 'Let us first collect the facts; then proceed to distort them'. I was therefore hugely encouraged when I chanced upon a marvellously compiled 'Survey of Earned Doctorates' produced by the National Science Foundation (NSF) of the United States. The prefix 'earned' is intriguing, suggestive of the fact that there might be a fair number of 'unearned doctorates'.

The NSF report on Science and Engineering Indicators 2012 is a treasure trove for analysts of science and technology. The chapter on doctoral education is replete with statistics, carefully collated and presented. The US research university system has been the gold standard in higher education for over half a century; the envy of other nations attempting to race along the path of development. There may be hidden in this mass of data, valuable lessons that might be learnt by other countries aiming to build an ecosystem of productive research universities. The growth of academia in the United States is clearly reflected in the rise in the number of doctorates from 8611 in 1957 to 49,010 in 2011. Inspection of the numbers over this period reveals two spurts of growth. The first occurred in the decade between 1960 and 1970 fuelled by the post-Sputnik thrust in American science and technology. The second, somewhat less dramatic surge is observed between 1988 and 1994, presumably a consequence of the electronic and biological revolutions. Is the United States producing too many Ph Ds? A recent report at the February meeting of the American Academy of Arts and Sciences (AAAS) appears to suggest that the job market for Ph Ds is poor, with the post-doctoral position becoming an increasingly uncertain road, which does not always lead to a suitable job in academia or industry. Six broad areas in which doctorates are awarded are identified (figures for 2011 in parentheses): Life Sciences (11,467), Physical Sciences (8678), Social Sciences (8120), Engineering (8004), Humanities (5214) and Education (4691). The top 50 institutions ranked by the number of Ph Ds produced are listed. The University of California at Berkeley (878), University of Michigan (767), University of Florida (762) and University of Illinois – Urbana-Champaign (750) lead the pack. Stanford (703), MIT (609) and Harvard (573) come in at positions 8, 14 and 16, respectively. Some feeling for the scale of the academic enterprise in large universities can be obtained from the figures for a single institution, the University of California's Berkeley campus. The 2011 figures for the Ph Ds produced are: Life Sciences 166, Physical Sciences 177, Social Sciences 161, Engineering 173, Humanities 128. The emphasis on research in the area of education is striking, with as many as 151 Ph Ds graduating from the Teachers College of Columbia University in 2011.

Foreign students constitute a considerable presence on US campuses, entering the country on temporary visas but often staying on after graduation. Of the 49,010 Ph Ds awarded in 2011, 14,245 recipients were foreign nationals on temporary visas. Of these, China accounted for 3978, India contributed 2161, South Korea's score was 1442, while Taiwan completed the top four with 693. South Asia watchers (and hopefully there may be some with a South Asian University coming up in Delhi) might be intrigued by the fact that Pakistan, Sri Lanka and Bangladesh contributed 91, 88 and 77 Ph Ds to the US figures. The diversity and number of foreign students is an enormous source of strength to US science, with as many as 40 countries contributing at least 50 degrees to the list of 2011 doctorates. Foreign students overwhelmingly receive their degrees in science and engineering. Amongst Indian students, 2033 received Ph Ds in science/engineering in 2011, whereas only 128 graduated in other disciplines. For China the corresponding figures are 3640 and 338. The NSF survey goes on to poll the foreign students regarding their future intentions. For the period 2005–11, 86.4% of Chinese and Indian students indicated a desire to stay on in the US; the highest amongst all the countries from which students were drawn. The figures for Japan 50.4%, UK 62.8%, Brazil 45.3% and Taiwan 57.2% are revealing; China the emerging (or emerged) superpower and India (a country actively wooing expatriates) do not seem attractive destinations for recently graduated US Ph Ds. Only two countries outranked India and China in this dismal statistic; Iran at 89% and Nepal at 86.7%, albeit with a very much smaller sample size. The gender ratios (M/F) for Ph D recipients in 2011, across the fields are: Life Sciences 45.2/54.7, Physical Sciences 71.4/28.6, Social Sciences 40.5/59.5, Engineering 77.2/22.2, Education 30.6/69.3 and Humanities 48.5/51.5. In patiently ploughing through systematically organized data tables in the NSF report, I finally found the numbers I was looking for – the average time taken for acquiring Ph D degrees and the age of the recipients. Across the fields, the median time to a doctorate is 7.7 years from the start of graduate school, while the median age at the time of the Ph D award is 32 years. The shortest median times are in the physical sciences, engineering and life sciences (6.7–6.9 years), while the humanities and education degrees appear to take substantially longer (9.3–11.7 years). Clearly very short Ph Ds, four years or less, may be the outliers.

Indian policy makers are now engaged in developing strategies to dramatically enhance the number of Ph Ds produced in India (20,000 annually is a number often heard as a target) and in boosting the quality and quantity of scientific output, largely measured by the number and presumed impact of research publications. Maximising quality and quantity of the Ph Ds and papers produced is hardly likely to be an easy task. It may be necessary to survey the employment opportunities for Ph Ds in our academic institutions and industry. Despite loud protestations about the shortage of faculty, recruitment is severely constrained by a multitude of factors. State government institutions are beset with difficulties in recruitment, while central institutions rarely find many of the Ph Ds graduating from our vast university system suitable for appointment. Industry is rarely seen as a significant employer of Ph Ds. Maybe the cry for more Ph Ds must be muted, while facts are gathered and interpreted.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Science and Law: The Gleevec Case

Laws passed by the Legislature in a democracy are always subject to judicial interpretation. In India, with very few exceptions, laws are framed by the executive, debated, refined, amended and enacted by Parliament and interpreted and enforced by the judiciary. Litigation is a popular pastime in democracies and lawyers abound. Lawyers, by definition, can provide the most compelling arguments to support completely opposing points of view. Courtroom dramas can provide riveting theater, both in real life and on the screen. Spencer Tracy's performance as Clarence Darrow in *Inherit the Wind*, the 1960 movie version of the 1925 Scopes 'monkey' trial, drew public attention to Darwin's ideas on evolution and highlighted the infirmities of the arguments for creationism. A vast majority of people (I am amongst them) view lawyers from a great distance, often bemused by the ability of practitioners of the legal profession to deal easily with contradictions and ambiguities, without the slightest sign of discomfort. It is the ability to be able to speak on all sides of a debate that sets lawyers apart. Unsurprisingly, lawyers swell the ranks of politicians in democracies. Laws can be hard to frame. When confounded by the intricacies of law, ordinary people are sometimes driven to exclaim, as Mr Bumble famously did in *Oliver Twist*: 'The law is an ass'. Eminent lawyers can transform into learned judges, often compelled to use their erudition to weigh both sides of a complex argument in order to deliver a final judgement. The pronouncements of lower courts can always be challenged, with many long running cases eventually reaching the court of last resort – the Supreme Court. In the last few weeks, the highest court in the land has been frequently in the news, delivering final verdicts on appeals against death penalties, imposing punishments in cases that have run for the better part of two decades and in admonishing the Italian ambassador, in a case that undoubtedly illustrates the complexity of situations, in which the writ of national and international laws are debated. In a recent brief, but pointed, commentary an eminent jurist, a former judge of the Supreme Court, V. R. Krishna Iyer noted: 'The Supreme Court is final not because it is infallible but it is infallible because it is final' (*The Hindu*, 13 April 2013). The processes of law, especially when criminal acts are to be judged, always attract the attention of the popular press. Civil cases are considerably less newsworthy. A recent exception, the battle waged by Novartis to obtain a patent for its anti-cancer drug *Gleevec* (*Glivec*) in India, prompted me to draw attention to the law and science in this column. The judgement delivered on 1 April 2013, by a two-judge bench, dismissed Novartis' claims, opening the door to manufacture of generics, which dramatically lower costs of patient care.

The reactions to the Gleevec judgement have been predictable. Patient groups, generic manufacturers and the vast majority of observers concerned about the unaffordability of many drugs, protected by patent regimes, have welcomed the judgement. Novartis has, of course, raised the spectre that the ruling might stifle innovation in India; a view that has been forcefully countered by many knowledgeable commentators. Multinational pharmaceutical companies have long projected astronomical costs for the drug discovery and development process, arguing that patent regimes and monopoly pricing allow companies to recoup the immense costs of the long drawn out research, development and regulatory clearance process. How are blockbuster drugs discovered nowadays, when the pipeline of potential new molecular entities is running dry? In reflecting on this question, the story behind Gleevec is worth revisiting. In 2009, the Lasker Award for Clinical Medical Research went to three researchers: Brian Druker of the Oregon Health and Science University, Nicholas Lydon who did his award winning work at Novartis and Charles Sawyers of the Memorial Sloan-Kettering Cancer Center. In introducing the work of the prize winners Joseph Goldstein, the chair of the Lasker Awards Jury, made this assessment: 'Gleevec has unquestionably transformed the treatment of CML (chronic myeloid leukemia) in much the same way that insulin transformed the treatment of people with Type I diabetes' (*Nature Medicine*, 2009, **15**, iii). In the complex and difficult area of cancer research, successes in developing specifically targeted chemotherapeutics are extremely rare. CML, which is a significant contributor to adult leukemias, is one of the first cancers to have a clear genetic link established. The identification of a gene linked to disease led to the identification of a gene product, a protein tyrosine kinase, whose enzymatic activity contributes critically to the process of cell proliferation. Brian Druker had by 1990 both the background in oncology and the expertise on tyrosine kinases to move towards testing the

idea that specific inhibitors may pave the way to viable therapeutics. Druker, an academic researcher, turned to Lydon at Novartis for potential inhibitors, developing a screen that yielded CGP57148B (or STI571), which was eventually named Gleevec or Imatinib Mesylate. Druker moved from the laboratory to the clinic in a relatively short period, especially as there were CML patients with 'no treatment options remaining' (Druker, B. J., *Nature Medicine*, 2009, **15**, xv). Lydon had by the mid-1980s, working at Ciba-Geigy (later Novartis), reached a conclusion, that many workers in the field endorsed, which pointed to the protein kinases as promising pharmacological targets. The Druker–Lydon partnership opened the doors to 'screening the Ciba-Geigy compound collection', a key step on the road to drug discovery. 'The challenges and excitement of medicinal chemistry', which Lydon refers to in his Lasker Award commentary are not often appreciated in academia in India, but are an integral part of the research that eventually leads to pharmaceuticals. As in most targeted approaches, ensuring selectivity was critical. In Lydon's words: 'On the basis of biochemical profiling data… Juerg Zimmerman made the key observation that substitutions of a methyl group at the 6-position of the anilino phenyl ring abrogate PKC inhibition but enhance ABL and PDGFR inhibition' (Lydon, N., *Nature Medicine*, 2009, **15**, xix). For readers mystified by a multitude of abbreviations, I might add a word of comfort, that Lydon merely refers to the targets that must be hit, while others must be unharmed. Collateral damage can lead to unacceptable toxicity and side effects if drugs attack biochemical pathways indiscriminately. It is the 'Zimmermann patent' which is referred to in the Supreme Court judgement. Gleevec or imatinib's discovery in the mid-1990s was based on the fact that CMLs origins are traced to a gene product – a tyrosine kinase – whose inhibition should yield a satisfactory therapeutic conclusion. A cautionary note is introduced by the third 2009 Lasker Award winner, Charles Sawyers, who describes the growing understanding of drug resistance and emphasizes the shifting view of cancer drug research: 'Although the success of imatinib was not a revolution in the Copernican sense, it spawned a transformation in cancer research that has fueled an urgency to characterize cancer genomes comprehensively and discover the driver mutation in all cancers' (Sawyers, C. L., *Nature Medicine*, 2009, **15**, xxiv). A retrospective view of the 'crusade against cancer', 40 years after its launch, notes that 'even the most successful targeted therapies lose potency with time'; for Gleevec a median time to resistance of 5 years (17% of patients) is a quoted statistic. The pitfalls of predicting the progress of cancer research are highlighted by James Watson's optimistic view in 1998 that antiangiogenesis drugs would 'cure cancer in two years'. Nearly a dozen years later, in 2009, Watson is reported to have suggested a move away from cancer genetics to 'understanding the chemical reactions within cancer cells' – old fashioned cellular metabolism (Kaiser, J., *Science*, 2011, **331**, 1542). Classical biochemists, long swept away by the surging tides of molecular biology and genetics, may appreciate the tinge of irony.

The Supreme Court judgement on the Gleevec case is remarkable in its sweep of chemistry and law. The question before the court was whether a new physical form of Gleevec, 'the beta crystalline form of Imatinib Mesylate' could be granted a patent in India under the TRIPS Agreement and the multiple Patents Amendment Acts enacted in subsequent years by Parliament, to meet India's obligations under international agreements and to 'adequately protect national and public interest'. The Zimmermann Patent granted in the US in May 1996 was for Imatinib Mesylate and Novartis sought protection under a new patent regime in India for its apparently novel 'beta form'. Was this an invention, new and distinct from the original substance or was this an attempt to extend the life of an unenforceable patent by 'evergreening' – a common subterfuge in the unending drive to maintain a monopoly. The Supreme Court judgement contains pages of chemical procedures, and discussions of chemistry which make interesting reading for anyone with a rudimentary knowledge of the subject. New forms of old molecules, polymorphs and formulations disguised in clever ways are often claimed to have greater stability and better bioavailability. 'Incremental innovation' is a powerful strategy in extending monopolies and in breaching patent protection. It is the courts that must eventually pronounce judgement. The marshalling of arguments to reach the conclusion, that 'Imatinib Mesylate is fully a part of the Zimmerman patent' and does not constitute a new invention in any form, requires a fusion of chemistry and the intellectual property protection laws as they stand today. The learned judges cite the scientific literature extensively, quoting from papers by the Novartis group in *Cancer Research* (1996) and *Nature Medicine* (1996). The judgement provides a fascinating glimpse of a complex debate at the confluence of chemistry and law.

Are there lessons to be learnt? A point that clearly emerges is that the many amendments to the Patent laws enacted by Parliament have indeed been valuable in protecting public interest. The judges note that the 'Court was urged to strike a balance between the need to promote research and development in science and technology and to keep private monopoly (called an "aberration" under our Constitutional scheme) at the minimum'. Gleevec's origins can be traced to excellent basic science in cellular biology and its development to the great strengths in medicinal chemistry within a pharmaceutical research laboratory. Future innovation in India in this area depends critically on our ability to recognize that such collaborations are central to drug discovery. There is also little doubt that science and law will intersect with greater frequency in the future.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

The Double Helix: Sixty Years On

Sixty years ago, on 25 April 1953, the journal *Nature* published three papers under a common heading, '*Molecular Structure of Nucleic Acids*'. The first of these is the iconic paper by Watson and Crick, simply titled '*A Structure for Deoxyribose Nucleic Acid*', which has a lone illustration – the now immortal double stranded DNA structure, represented in diagrammatic form. The base pairs are shown as horizontal lines on a spiral, ladder like structure. The text introduces the selective base recognition scheme, now taught even in high school. The concluding sentence has been imitated by many authors, who hope to invest their findings with an importance that is invariably exaggerated: 'It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material' (Watson, J. D. and Crick, F. H. C., *Nature*, 1953, **171**, 738). The final sentence of the paper on the double helix is a masterfully, understated conclusion, avoiding the need to 'write at length about the biological implications' (*The Double Helix*, Watson, J. D., Touchstone Edition, 1996, p. 120). In an introduction to the 1996 reissue of Watson's 1968 account of the drama behind the discovery of the DNA structure, Sylvia Nasar, author of *A Beautiful Mind*, compares the scientific impact 'to the breakthroughs that led to the splitting of the atom and the invention of the computer'. What of the papers that accompanied the Watson–Crick note in *Nature* sixty years ago? The first by Maurice Wilkins and his colleagues, titled '*Molecular Structure of Deoxypentose Nucleic Acids*' presents an X-ray fiber diffraction pattern and an interpretation in terms of a helical-structure and notes that 'in general there appears to be reasonable agreement between the experimental data and the kind of model described by Watson and Crick' in the preceding communication (Wilkins, M. H. F., Stokes, A. R. and Wilson, H. R., *Nature*, 1953, **171**, 738). The second by Rosalind Franklin and her student R. G. Gosling, titled '*Molecular Configuration in Sodium Thymonucleate*', presents the fiber diffraction photograph that provided the key clues to the model building exercise of Watson and Crick. A reading of the paper by Wilkins *et al.* reveals the importance of 'the exceptional photograph obtained by… R. E. Franklin and R. G. Gosling from calf thymus deoxypentose nucleate'. The authors themselves interpret their data cautiously and note that their 'general ideas are not inconsistent with the model proposed by Watson and Crick in the preceding communication' (Franklin, R. E. and Gosling, R. G., *Nature*, 1953, **171**, 740). Franklin died of cancer in 1958, four years before the award of the Nobel Prize in Physiology or Medicine to Watson, Crick and Wilkins. The unkind portrait of Franklin in Watson's 1968 book has been the subject of much discussion in the years since. Seven authors were involved in the three papers that appeared in *Nature* in 1953, of whom only Raymond Gosling and Watson survive. Gosling was the Ph D student who 'wrapped DNA around a paperclip to keep the molecule's fibres stretched taut in front of an X-ray source so that he could analyse their structure'. In an editorial titled, 'Due credit', *Nature* notes that 'the result was the celebrated "photograph 51" – the image that told James Watson that DNA strands curl around each other like a twisted ladder, and that the specific pairings in the rungs are key to the mechanism of inheritance' (*Nature*, 2013, **496**, 270). 'All three papers appeared with no peer review' – a tribute to the powers of persuasion that could be brought into play in the cloistered environment of London's 'gentleman's clubs', an English institution of undisputed influence. Gosling, now 86, can be heard in an interview on a *Nature* podcast (termed a 'pastcast'), recounting the events of 1953.

The journal *Genome Biology* presents in its 25 April 2013 issue a wonderfully readable account titled, 'Raymond Gosling: the man who crystallized genes' (Attar, N., *Genome Biology*, 2013, **14**, 402). My curiosity was aroused by the concluding sentences of the *Nature* editorial (18 April 2013): 'Discoveries take ego, genius, conflict, inspiration and fierce ambition. But they also need the hard graft of Ph D students who beaver away late into the night and improvise with what they find in a stationery cupboard. They do not always receive the recognition that they deserve. Raymond Gosling is a good place to start to reverse the trend.' The DNA story is set in two locales, Cavendish Laboratory at Cambridge with Lawrence Bragg as the presiding deity and King's College, London, where John Randall put together a group to apply the methods of physics to the problems of biology. In recognizing the importance of interdisciplinary research, Randall was, in Gosling's words, 'ahead of the curve'. The young Gosling was fleeing from the prospect of doing research on 'viscosity' at University College

London. The prospect of working at a rival institution with Randall, a 'strange bald-headed little man with a Napoleonic complex, who was running the circus in biophysics', sounded wonderfully attractive. In Gosling's telling of the story, Randall's role 'in the pursuit of the double helix cannot be overstated'. The DNA model building work at Cambridge critically depended on fibre diffraction data. Randall skillfully played the two competing groups at King's College, Wilkins and Franklin, in pushing the experimental research forward. Rosalind Franklin's recruitment, in Gosling's version, was a result of Randall's conviction that Wilkins and Gosling would never 'learn enough crystallography to be able to solve this spotty diagram'. Gosling a Ph D student with Wilkins was reassigned to work with Franklin. His assessment of Randall, with the benefit of hindsight: 'He [Randall] subscribed to the divide and rule principle, as lots of people did. He thought it would make them more competitive and improve their work.' Wilkins appears to have been an instinctive experimenter. The DNA sample used to obtain the beautiful diffraction patterns, 'Signer DNA', was accidentally obtained. Rudolf Signer, a Swiss biochemist, delivered a lecture at the Royal Society on his method for producing DNA of a superior quality. In Gosling's words: 'Signer asked at the end of the lecture if anybody would like some of this material and he had a specimen tube full of this freeze dried material. Only two people put their hand up. I'm glad to say that Maurice [Wilkins] was awake enough to put his hand up!' Gosling reveals an interesting contribution made by Wilkins to the first successful experiment to obtain a good diffraction pattern of DNA. In attempting to displace air inside the camera with hydrogen, Gosling struggled with ways 'to prevent the gas from coming out of the collimator tube'. Wilkins, the 'rather shy Assistant Director of the MRC Unit', provided an innovative solution, by producing a packet of condoms, which quickly solved the problem. The *Genome Biology* commentary notes that this 'somewhat unorthodox, and nevertheless essential contribution' to Gosling's first X-ray images of crystalline DNA, was understandably omitted from Wilkins' Nobel lecture. Gosling was a witness to Rosalind Franklin's demolition of the first 'wrong model' – a double helix with phosphates in the middle – built by Watson and Crick. This was the only occasion, as Crick was to note later, that 'he ever saw Jim Watson at a loss for words'. Gosling was an integral part of the King's College effort to extract structural information from their X-ray photographs. They were overtaken by the inspired model building at Cambridge. Would they have finally hit upon the right model? Gosling thinks so: '…we would have got there at King's, but not in one fell swoop, that it would have come out in dribs and drabs about various aspects'.

Gosling's career path immediately after his Ph D illustrates a point that is not very well known today – the DNA papers appearing in *Nature* in 1953 attracted little attention. In a commentary that appeared on the 50th anniversary of the double helix, Bruno Strasser notes: 'We usually think that the double-helix model acquired immediate and enduring success. On the contrary it enjoyed only a "quiet debut"…. So when did it become so immensely popular in the scientific community? The 1960s? 1970s? 1980s? None of these. Not until the 1990s' (Strasser, B., *Nature*, 2003, **422**, 803). A plot of the citations to the Watson–Crick paper of 1953 shows a peak in 1963, following the Nobel prize and a decline thereafter, with rapid growth beginning in the early 1990s. Why should modern day authors cite the 1953 paper? After all, the Watson–Crick double helix is so well known that citation is hardly likely to be necessary. Strasser provides an answer: 'Authors in an unprecedented variety of fields, from neurosurgery to plant physiology cite the 1953 paper in an attempt to appropriate the current prestige of molecular biology for their own speciality by underlining (sometimes far fetched) historical affiliations with the double helix.' The developing pace of the human genome project in the 1990s also stimulated many citations of the 1953 paper. Strasser adds that 'setting the double helix as the highest standard of scientific creativity helped to make the descriptive genome sequencing project less tedious. After all, both were attempts at describing the structure of DNA and would result only in a guess at the possible "genetic implications" as Watson and Crick had put it.'

Whom did Watson and Crick cite in 1953? There are only 6 references in their original report. A notable omission is the absence of any reference to the work of Oswald Avery and his group, establishing the chemical nature of the genetic material as DNA, which appeared in 1944. A historical note appearing in this issue of *Current Science* analyses the citation patterns of the four papers on the double helix published by Watson and Crick in 1953–54 (Kantha, S. S., *Curr. Sci.*, 2013, **104**, 1237). The author draws attention to a paper by W. T. Astbury and F. O. Bell that appeared in 1938, some years before Avery's work, which may have been 'prophetic in recognizing the genetic role of DNA' (*Nature*, 1938, **141**, 747). I read Watson's 1968 account of the double helix in the same year, as a student wondering whether to soldier on towards an advanced degree in science. While I had no knowledge of the nature of DNA or its importance, research, as portrayed by Watson, seemed exciting – almost a game in which the winner takes all. Decades later, Bruno Strasser's assessment seemed to ring true: 'The double helix worked as the marker of an age of (lost) innocence, when youth, intelligence and self assurance were sufficient to make great discoveries in science.' As the origins of the double helix recede into history, periodic anniversaries serve to remind us of the human drama that accompanied the birth of molecular biology.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Research Assessment: Declaring War on the Impact Factor

Nearly forty years ago, when I began my research career in India, science proceeded at a leisurely pace. There was little by way of funding or major facilities even at the best of institutions. Enthusiasm and interest were the key ingredients in maintaining a focus on research. The environment still contained many role models, who had made significant contributions to their chosen fields, under undoubtedly difficult circumstances. The mid-1970s was a time when political and economic uncertainties precluded a great deal of government interest in promoting science. There was relatively little pressure on researchers to publish papers. The age of awards and financial incentives lay in the distant future. In those more sedate times, the results of research were written up when the findings appeared interesting enough to be communicated. The choice of journals was limited and most scientists seemed to be content with submitting manuscripts to journals where their peers might indeed read the papers. Journals were still read in libraries. Note taking was common, photocopies were rare and the 'on-line journal' had not yet been conceived. The academic environment was not overtly competitive. I never heard the word 'scooped' in the context of science, until well into middle age. Eugene Garfield's 'journal impact factor' (JIF) had not penetrated into the discourse of scientists, although the parameters for ranking journals had been introduced into the literature much earlier. The word 'citation' was rarely heard. In the library of the Indian Institute of Science (IISc) there was a lone volume of the 1975 *Science Citation Index* (a hardbound, printed version, extinct today), presumably obtained gratis, which sat forlorn and unused on rarely visited shelves. Only a few hardy and curious readers would even venture near this sample of the Citation Index, which seemed of little use. It required both effort and energy to search the literature in the 1970s. Few could have foreseen a time when administrators of science in distant Delhi would be obsessed with the many metrics of measuring science, of which the JIF was a forerunner. Indeed, the unchecked and virulent growth of the use of scientometric indices in assessing science has at last begun to attract a backlash; an 'insurgency' that has resulted in the *San Francisco Declaration on Research Assessment* (DORA), whose stated intention is to begin 'putting science into the assessment of research'. The declaration is signed by 'an ad hoc coalition of unlikely insurgents – scientists, journal editors and publishers, scholarly societies, and research funders across many scientific disciplines', who gathered at the annual meeting of the American Society for Cell Biology (am.ascb.org/dora/May 16, 2013). An editorial by Bruce Alberts in the May 17 issue of *Science* (2013, **340**, 787) notes that 'DORA aims to stop the use of the "journal impact factor" in judging an individual scientist's work in order "to correct distortions in the evaluation of scientific research" '.

The origins of the 'impact factor' may be traced to a largely forgotten paper that appeared in *Science* in 1927, which described a study carried out at Pomona College in California, that begins on an intriguing note: 'Whether we would have it or not, the purpose of a small college is changing'. The authors describe an attempt to draw up a priority list of chemistry journals to be obtained for the library. Budgetary constraints were undoubtedly a major matter of concern in the late 1920s. I cannot resist reproducing here the authors' stated purpose in carrying out this exercise over eighty five years ago, as their words may strike a chord in readers interested in the problem of uplifting the science departments of colleges in India today: 'What files of scientific periodicals are needed in a college library successfully to prepare the student for advanced work, taking into consideration also those materials necessary for the stimulation and intellectual development of the faculty? This latter need is quite as important as the first because of the increasing demand of the colleges for instructors with the doctorate degree. Such men are reluctant to accept positions in colleges where facilities for continuing the research which they have learned to love are lacking' (Gross, P. L. K. and Gross, E. M., *Science*, 1927, **LXVI**, 385). The procedure adopted was simple; draw up a list of journals most frequently cited in the *Journal of the American Chemical Society* (*JACS*), the flagship publication of the American Chemical Society. Much can be learnt about the history of chemistry (and, indeed, more generally about science) by examining the list of the top six journals (other than *JACS*) recommended for a college chemistry library in the United States, in 1927: *Berichte der Deutschen Chemischen Gesellschaft*, *The Journal of the Chemical Society (London)*, *Zeitschrift für Physikalische Chemie*, *Annalen der Chemie (Liebig's)*, *The Journal of Physical*

Chemistry and *The Journal of Biological Chemistry*. Clearly, in the 1920s the literature of chemistry was overwhelmingly dominated by European journals. For students growing up in the frenetic world of modern science, I might add that *Science*, *Nature* and *PNAS* appear far down the list. A similar exercise carried out today would reveal a dramatically different list of journals; undoubtedly a reflection of the turbulent history of the 20th century.

The journal impact factor emerged in the 1970s as a tool to rank journals. In the early years, it was largely a metric that was of limited interest. The revolution in the biomedical sciences resulted in an explosive growth of journals in the last two decades of the 20th century; a period that coincided with the dramatic rise of information technology and the emergence of the internet. The acquisition of the Institute for Scientific Information by Thomson Reuters lent a hard commercial edge to the marketing of the tools and databases of scientometrics; the *Web of Science* began to enmesh the world of science. Journal impact factors appear unfailingly, every year, making the business of publishing science journals an extremely competitive exercise. Journal editors scramble to devise schemes for enhancing impact factors and scientists are drawn to submit articles to journals that appear high on the ranking lists. If JIFs were used only to compare journals there may have been little to grumble about. Unfortunately, individuals soon began to be judged by the impact factors of the journals in which they had published. Some years ago the use of an 'average impact factor' was actively promoted in India, to judge both individuals and institutions. The introduction of the '*h* index', a citation based parameter that appeared in the literature a few years ago, as a means of ranking individual performance, may have drawn away a few adherents of the average impact factor. Very few proponents of the JIF as an assessment tool in India appear conscious of obvious limitations. Most impact factors are driven up by a few highly cited papers, while others bask in reflected glory. The field specific nature of the JIF can lead to extremely misleading conclusions, when comparing individuals and institutions using this imperfect metric. Despite these drawbacks, the use of JIF as a tool of research assessment has reached epidemic proportions worldwide, with countries like India, China and the countries of southern Europe being among the hardest hit. Students in India, particularly those working in the biological sciences and chemistry in many of our best institutions, are especially self conscious; constantly worrying about the JIF when they submit papers.

The Declaration on Research Assessment (DORA) is a call to take up arms against the insidious JIF. Its general recommendation is a call for a boycott: 'Do not use journal-based metrics, such as Journal Impact Factors, as a surrogate measure of the quality of individual research articles, to assess an individual scientist's contributions, or in hiring, promotion, or funding decisions.' Scholarship and achievement can be judged without using a metric that was never designed for the purpose. The Declaration also has a message that may well be worth heeding by researchers in India: 'Challenge research assessment practices that rely inappropriately on Journal Impact Factors and promote and teach best practice that focuses on the value and influence of specific research outputs.' In his *Science* editorial, Alberts is trenchant: 'The misuse of the journal impact factor is highly destructive, inviting a gaming of the metric that can bias journals against publishing papers in fields (such as social sciences or ecology) that are much less cited than others (such as biomedicine).'

Research assessments have also become commonplace in ranking institutions. The metrics used rely substantially on publication numbers and citations, invariably based on the *Web of Science*, although additional parameters contribute in differing ranking schemes. In recent times, both the Prime Minister and the President have publicly lamented that no Indian university or institution appeared in the 'top 200' in the world (*The Hindu*, 5 February 2013 and 16 April 2013). While there may be much to lament about in Indian higher education, are the rankings really an issue that needs immediate attention? In an Op-Ed piece in *The Hindu* (9 March 2013), Philip Altbach is categorical: 'For India, or other developing countries to obsess about rankings is a mistake. There may be lessons, but not rules…. The global rankings measure just one kind of academic excellence, and even here the tools of measurement are far from perfect.' Altbach notes, and many analysts would undoubtedly agree, that two systems, 'the Academic Ranking of World Universities, popularly known as the "Shanghai rankings", and the World University Rankings of *Times Higher Education* (*THE*) are methodologically respectable and can be taken seriously'. While the former measures only research impact, with several parameters weighted towards the highest level of achievement (number of Nobel prize recipients in an institution), the latter 'measures a wider array of variables'. Altbach adds: 'Research and its impact is at the top of the list, but reputation is also included as are several other variables such as teaching quality and internationalization. But since there is no real way to measure teaching or internationalization weak proxies are used. Reputation is perhaps the most controversial element in most of the national and global rankings.' Altbach's critique, of an apparent obsession with university rankings in India, was quickly countered by Phil Baty, the editor of *THE* rankings who warns: '…it would be a far greater mistake for Indian institutions and policy makers to under-use the global rankings than to overuse them' (*The Hindu*, 11 April 2013). It may indeed be important for institutions to appreciate the rules of the game if they are to achieve a competitive score. Policy makers would also benefit if they set out to understand the tools of research assessment before they begin to use them.

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Universities and Avalanches

Authors of correspondence items in this journal, sometimes, raise the flag of concern on issues that appear to be important. A recent letter that crossed my desk drew attention to errors in the classification of 'institutional members' of *Current Science*, a growing group of permanent subscribers who provide this journal with some measure of financial stability. An 'open access', not-for-profit journal like *Current Science*, which does not charge authors a publication fee, must necessarily develop alternative strategies for raising resources in order to maintain regular production. Institutional members are one important component in ensuring that income and expenditure are matched, year after year. The correspondent lauded this initiative, but seemed disturbed by the classification of some member institutions as 'universities'; his ire directed (probably justifiably so) at 'deemed universities', a status conferred by the University Grants Commission on institutions that are not created as 'Universities' by Acts of Parliament or State Legislatures. My editors' instincts might have allowed this to pass if it had not been for an emphatic assertion that seemed uncomfortably aimed: 'One would not call Indian Institute of Science a University!' (Murugesan, T., this issue, p. 1466). Having spent my entire professional career at this institution (also the birthplace of this journal; see *Current Science*, 2007, **92**, 5) the barb struck close to home. How does one classify the Indian Institute of Science (IISc)? Is it a national research laboratory or is it an academic institution like other universities? What, indeed, is a university – deemed or otherwise? To understand IISc's origins and evolution going back into the past is necessary. History has many lessons if one cares to learn them, although the quickening pace of modern life may allow little time for the luxuries of reflection.

The British created the universities at Madras, Calcutta and Bombay by the mid-19th century; the precise birthdate of 1857 is undoubtedly significant in India's colonial history. The universities, as conceived, consisted of a set of affiliated colleges, some of which were distributed over the large 'Presidencies', the principal units of colonial governance. The primary aim of the colleges then was to provide an education, inevitably leading to certification in the form of diplomas and degrees; training grounds for creating the generations who would help the British govern and administer a sprawling country. Research and advanced scholarship was never a predominant factor in determining the agenda of a university in the 19th century. The system of affiliated colleges with a 'university' as a connecting node was borrowed from the centuries old models of Cambridge and Oxford. Jamsetji Tata's original conception of IISc was a clear departure from this model. The blueprints for the institution drawn up in the 1890s discuss a 'University of Research' or a 'Research Institute'; an idea outlined with remarkable precision and clarity in documents that bear testimony to the foresight of dedicated individuals who envisioned the future (*Current Science*, 2008, **94**, 5; *ibid*, 2008, **95**, 1651). The formal birth of IISc in 1909, announced with a Vesting Order by the then British Government, marked the launch of an institution intended to train students in subjects that were then badly needed for India's future development. The first departments created – chemistry and electrical technology – clearly recognized the two key catalysts of the industrial revolution in India, anticipated by J. N. Tata; chemicals and electrical power. IISc was among the first two institutions to be accorded the status of a 'deemed university' by the UGC in 1958, shortly after the regulatory body came into being by an Act of Parliament in 1956. For a long time thereafter, relatively few institutions were added to this list; the floodgates were opened more recently as both private institutions and public organizations with mandates far removed from higher education were deemed to be universities. The word 'deemed' appears to have been somewhat tarnished over the years, leading to many discussions on what constitutes a 'university'. IISc has over the past five and a half decades awarded degrees at Bachelor's, Master's and Ph D levels. A largely forgotten course, but one well remembered by the alumni, is the three year B E program in engineering disciplines which admitted students after a Bachelor's degree in science; creating a generation of scientist–engineers distinctly different in their outlook from those trained in conventional courses in engineering. The program ended in the 1980s, as reforms in engineering education altered the duration of undergraduate courses. More recently, the introduction of a four year Bachelor's degree in science, using the widely discussed but rarely implemented flexible course structure, with the primary aim of providing research exposure at undergraduate level, is an experiment that

reiterates an institutional commitment to fostering a link between teaching and research at all levels.

The development of higher education in India in the recent past has been rapid; turbulent growth is indeed hard to regulate. Many institutions now legitimately call themselves universities; but are they truly 'universities' in the traditional academic sense, or is it a title defined in a narrow legalistic sense on the website of the UGC? Specialized universities which serve only to affiliate engineering or medical colleges can, at times, be predominantly administrative and examining bodies. Others, like the law universities are highly focused institutions. Indeed, even India's oldest universities have slowly transformed into a shadow of their former selves in the decades after Independence. The growing number of central and state universities find it a hard struggle to establish themselves and expand, in an arena where private competition is growing. While the problems of resources and faculty shortages are subjects of widespread discussion, new species of universities are being willed into existence. Policy makers have been seduced by terms like 'world class universities' or 'innovation universities', in the fond hope that the prefix would immediately transform the institution to match the best in the world. The public perception of academic institutions in India is also clouded by the constant lament that no Indian institution (or university) appears in the global rankings in the top 200 in the world. There is little understanding of the nature of the ranking schemes which are not necessarily weighted in the favour of institutions in many parts of the world.

What constitutes a university today? What will the universities of the future look like? Should institutions across the world look to western models like Cambridge and Harvard or will the century ahead witness another revolution in higher education? These are questions which spring to mind, especially when one considers strategies for coping with global competition, while achieving national objectives. Coincidentally, even while I thought about the letter to *Current Science*, I received a copy of a long essay produced by the Institute for Public Policy Research in London entitled 'An Avalanche is Coming'. The authors, Michael Barber an educationist and UK policy maker and his colleagues Katelyn Donnelly and Saad Rizvi, discuss 'higher education and the revolution ahead' (Pearson, March 2013). The publishers, Pearson, advertise themselves as 'the world's leading learning company'; a private, commercial provider of education in Britain. Pearson, a large publisher which owns Penguin and the *Financial Times*, has even launched a foray into awarding academic degrees, appropriately enough in the area of 'business and enterprise', with validation (a new form of affiliation) being provided by colleges which are a part of the University of London. The Barber *et al.* essay carries a foreword by Lawrence Summers, former President of Harvard University. Summers notes that the 'forces of technology and globalization' which have transformed 'sectors such as media and communications or banking and finance may now transform higher education'. He strikes an ominous note: 'The solid classical buildings of great universities may look permanent but the storms of change now threaten them'. In their preface, Barber and his colleagues argue that 'radical and urgent transformation is required in higher education', but worry that 'perhaps as a result of complacency, caution or anxiety, or a combination of all three, the pace of change is too slow and the nature of change too incremental'. The authors forsee threats 'to traditional 20th century universities if key institutions don't change radically'. They borrow the 'avalanche' metaphor from the historian Norman Davies: 'Historical change is like an avalanche. The starting point is a snow covered mountainside that is solid. All changes take place under the surface and are invisible. But something is coming. What is impossible is to say when.' Barber *et al.* note that 'the one certainty for anyone in the path of an avalanche is that standing still is not an option. Indeed, it is a classic error of strategy to calculate the risks of action but fail to calculate the (often greater) risks of doing nothing'. Sudden change is always difficult to come to terms with and compelling arguments are always advanced to maintain the status quo in academic institutions across the world. Recent debates in the arena of higher education in India are testimony to the enormous resistance to change. Barber *et al.* advance an interesting argument that 'the traditional university is being unbundled'. They suggest that several new species may emerge: 'the elite university, the mass university, the niche university, the local university and the lifelong learning mechanism'. I suspect many of these species may be identified, by taxonomists specializing in education, in the large body of institutions across India that have been recognized by the UGC or other authorities. The technological driver of the impending revolution in education may be the 'Massive Open Online Courses (MOOCs)' which are now available from the best universities in the world. The need to reevaluate the nature and scope of classroom lectures is being widely recognized. A new field, 'learning science' appears to be emerging, which will help to blend the right mix of online courses and classroom interactions (Singer, S. R. and Bonvillian, W. B., *Science*, 2013, **339**, 1359). A *Nature* news feature acknowledges that 'fundamental reform in on-campus teaching is a much tougher proposition' than beaming online courses (Waldrop, M. M., *Nature*, 2013, **495**, 160). The essay by Barber *et al.* is provocative. Bundling, unbundling and rebundling, words familiar to publishers, may acquire a new meaning in the context of universities. Their conclusion should stimulate reflection: 'Just as an avalanche shapes the mountain, so the changes ahead will fundamentally alter the landscape for universities.'

P. Balaram

CURRENT SCIENCE

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EDITORIAL

Signing Off

This journal will have a new Editor, beginning July 2013. R. Srinivasan, a distinguished geologist and currently associate editor, will assume editorial responsibility. The journal also has a new set of associate editors. E. Arunan, a chemist, and S. K. Satheesh, an atmospheric scientist, join N. V. Joshi on the team. Chandrika Ramesh, who oversees manuscript processing and M. S. Venugopal, who shoulders responsibility for production will be the executive editors. This new team, a mix of old hands and new faces, inherits a journal that has grown in size and visibility over the years, but which undoubtedly needs much greater improvement. Having spent a quarter of a century working on the journal, I am certain a new team will bring the much needed energy and drive for rapid improvement and change. The editorial staff, young and enthusiastic, are complemented by energetic short term interns and science writing fellows.

Signing off after a long association is never easy, but it is inevitable. Working at the journal has been a most rewarding experience, educating and challenging in large measure. Stressful situations have been substantially moderated by an outstanding set of colleagues, who have handled with equanimity the constraints of fortnightly production schedules, together with the pressures of dealing with impatient and sensitive authors, on the one hand, and defaulting and acerbic referees on the other. Issues of ethical transgression, plagiarism the most common, have been more frequently encountered in recent years, increasing the burden of the staff handling the regular operations of the journal. The journal's financial position has improved over the past few years providing some relief for those who manage *Current Science*.

I began to write this fortnightly column in July 1998, continuing till mid-2005, when I accepted my current position as the head of the Indian Institute of Science. There were two reasons for laying down the pen. Firstly, I was unsure if time to read, indiscriminately, would be available; an essential requirement for a writer perennially in search of a topic and bound by the strict deadlines imposed by the cycle of publication. Secondly, it seemed more than likely that any views expressed as an individual might be misconstrued as an institutional position, leading to potentially embarrassing, and inevitably unpleasant, conflict of interest situations. Guest writers helped to fill the editorial column, but the regularity of appearance of these columns became hard to maintain. I returned to the task in January 2008 and have struggled over the past five and half years to produce a fortnightly piece, that is passable and moderately interesting to a diverse readership. At first glance, writing appears an easy task, especially to those brimming with opinions to express and possessing solutions to the many problems that abound in academia. It is only when I have put pen to paper (my mind still freezes when confronted with a screen and keyboard; an affliction which will pass with my generation) have I appreciated the term 'writers block'. However mundane the topic, however pedestrian the writing, it can still be a formidable task to string together a number of coherent sentences. Maintaining the length of a column to precisely fit a pre-determined space, in a journal with a printing deadline, can be a tiresome and demanding task. But, arguably, the most formidable barrier to be breached in writing a regular column is to find a topic and the background material that may be needed to flesh out a theme. Reading, with little discrimination and an eye open for a possible issue to discuss, has become a habit and a hobby over the years; undoubtedly taking time away from many other more useful and productive tasks. I have also lived in the shadow of the danger of becoming repetitive, an occupational hazard for compulsive writers and speakers.

This editorial column, which has permitted me to express unfettered, and possibly uninformed, opinion and allowed me to reflect on a wide range of topics has rarely been used to acknowledge those who have shaped the journal in recent times. About half a century after its founding in 1932, *Current Science* had begun to enter a lean phase in the 1980s. S. Ramaseshan (1923–2003) began the task of transforming the journal in 1988, a process I was privileged to witness at close quarters (*Current Science*, 2006, **86**, 5). His wide understanding of science and persuasive abilities enabled him to enlist many authors and referees to the task of ensuring a steady flow of manuscripts. His emphasis on the need for the journal to appear punctually was central to the transformation of the journal in the early 1990s. He recognized

the relevance of opening the pages of the journal to a wide spectrum of opinion on issues confronting science in India. K. R. Rao (1936–2008) joined the team at the journal in 1999 as associate editor. He was meticulous, generous with his advice and help to authors and amongst the first to spot the growing menace of plagiarism. He brought to the office of the journal a degree of scholarship, that ranged far beyond his discipline of physics, and the rare qualities of patience and large heartedness in dealing with authors which has been difficult to replicate (*Current Science*, 2008, **95**, 1507). Riki Krishnan (1971–2010), whose infectious enthusiasm for the journal, history and people was unbounded, ensured that my own enthusiasm for editorial chores and the sometimes forbidding task of producing an editorial column did not wane. Riki's humble designation of 'editorial assistant' did not fully reflect his contributions to the development of the journal (*Current Science*, 2010, **98**, 731). Ramaseshan formally retired as editor in 1996, but remained a source of strength till the end. K. R. Rao worked with his customary, quiet efficiency until the end came abruptly in 2008. Riki Krishnan was taken by cancer, in the prime of life in 2010, cutting short a career that would have undoubtedly blossomed at this journal. Chandrika Ramesh and M. S. Venugopal have admirably held the fort at the office of the journal, steering a steady course through the turbulence created by changing technologies; a task rendered difficult by a demanding publication schedule. Even journals cannot escape the need for a calm and guiding influence in day-to-day administration, a function carried out remarkably quietly, with characteristic competence and commitment by G. Madhavan. Over the last few years, N. V. Joshi, K. S. Gandhi, S. S. Krishnamurthy and R. Srinivasan have borne the brunt of the editorial work as associate editors, aided by the contributions of several members of the Editorial Board. I owe a special debt of gratitude to all of them. No journal exists without authors, referees and readers. It is this diverse and far flung community which any editor must thank for the relevance and success of a journal. For all failings and errors, editors must accept responsibility and, undoubtedly, this journal may have erred on many occasions under my stewardship.

Current Science was founded just over eight decades ago as modern science began to take root in India. Much has changed since then and much more will in the future. This continuing transformation will, I am sure, find reflection in these pages.

P. Balaram

To me, his retirement from his journal — when in full command of his powers — reminded me of the similarly graceful and exquisitely timed exits of those other (and in this regard unrepresentative) Indians, Vijay Merchant and Sunil Gavaskar.

Ramachandra Guha
Telegraph India
21.09.13

"For those of us who avidly read these Editorials every fortnight as we matured as scientists this book would be a "collector's item". And for the unlucky generations who have missed being nurtured on those fortnightly masterpieces, this compilation would serve as an example of how to present a balanced perspective on matters of importance while maintaining scholarship, literary value and that wonderful tongue-in-cheek humour that is 'quintessentially Balaram'."

Anand K Bachhawat
IISER Mohali

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