Society for Scientific Values

Ethics in Scientific Research Development and Management

News And Views

Volume 5 April, 2007 No. 1

Lead us from unreal to real

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Editorial

Not By Money Alone

For the last more than thirty years SSV has been saying that the quality of Science in the country is declining especially because of the wide spread lack of objectivity and integrity in research, publication and management. But many top managers of science have not only been denying it by naming this and that superficial achievements, but also saying that SSV is denigrating Indian science. At last when condition has become very poor, the chairman of Science Advisory Council and Scientific Advisor to the Prime Minister, Dr C.N.R.Rao said in July,2006 that 'Indian science is in crisis' and at this rate it will be finished in the next five years. He suggested big increase in science funding to stop the decline, The Prime Minister Manmohan Singh has promised to double the funding for science in the next five years (Nature,445, 134-135, 2007).It is very good. But, will increased funding alone stop the decline? Any one who is closely associated with research knows that the answer is a definite no. In the past, many large funded projects have ended without bearing fruit.

In a letter published in Nature, 445,1 February, 2007 written in connection with the promise of the PM, Dr. U.C.Lavania of CIMAP, Lucknow has very rightly said that to arrest the decline of Indian science the scientific institutions must be made free from academic feudalism. **Science will not flourish unless the scientists in general and managers of science in particular learn to bow before the fact from wherever it may come and howsoever inconvenient it may be.** Dr.Lavania has identified several problems and suggested five- point plan to revive and reform Indian science. These are; (1) Universities' core infrastructure should be overhauled, with a primary focus on high-quality education.(2) Synergy and collaboration must be promoted between national institutes and centres of excellence in the universities, free from bureaucratic obstacles.(3)Project leaders must have total freedom from unnecessary red tape, in order to attract contract research and competitive grants.(4) Only accomplished, mature scientists of sound itegrity must be put in the leadership role of science managers.(5) **A statutory model code of scientific values and ethics must be created**

Because of wide spread loss of values none of these points are easy to implement. However, a definite improvement will start if the Prime Minister will take steps to create a statuary model of code of scientific values and ethics to be followed by scientists and science managers and assign the work overseeing its implementation to the 'Society for Scientific Values'. The Society has already formulated such a code. **It is now for the Scientific Advisory Council and its Chairman to take the initiative.**

— P.N. Tiwari

Authorship issue and Research Values

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Certain suggestions and values have been elaborated[1] in deciding authorship for maintaining healthy research culture. As pointed out in a recent article[2] the allocation of credit has become a value-based issue in listing of authorship since science has become more of a collaborative enterprise than it was in the past. While the pursuit of scientific knowledge implies a certain set of characteristically scientific values, the relevance of other values in the practice of science are not thereby eclipsed. Science intersects with values in three different ways. The epistemic values, which guide scientific, research itself. Secondly, the scientific enterprise is always embedded in some particular culture and values enter science through its individual practitioners. Thirdly, values emerge from science, both as a product and process and distributed in the culture of a society. The pursuit of science as an activity is itself an implicit endorsement of the values of developing knowledge of the world. The social values or research ethics are not always followed in science[3] but they remain utmost important. The disparity between the ideal and the actual merely poses challenges for creating a way to achieve these valued ends –say, through a system of checks and balances. In many of our research institutions there is a real lack of moral insight as a consequence we are biting deeply into the range of life's value. One wonders how values like filial duty, moral and ethical will work out in this reality of nepotism, hedonistic acquisitiveness and moral turpitude. Thus values provide a nexus between moral and intellectual development and also between knowledge and character. Science does not create these values; rather it introduces novel situations, which require individual to apply old values in significantly new dimensions.

In recent years, the allocation of credit has become a burning issue in listing of authors. Science has become a much more collaborative enterprise than in the past[2]. Several considerations must be taken care in determining the proper criteria for authorship between a student, research assistant and a senior scientist. Although some guidelines are available for medical sciences since last two decades but for agricultural and basic sciences these are not available. In many of research institutions and universities supervisors/guides largely govern and dictate self-made rules for determining the authorship and also its sequence. At many times the student is at mercy and helpless to obey these self imposed rules and at many occasions, they are scientifically blackmailed for authorship. It is the narrow mindedness of research guides/supervisors who satisfy themselves by highlighting petty considerations and spoil students career. The so-called "honorary /courtesy" authors dilute the credit due to those who actually do the work[2]. Although many institutions have adopted a policy for scrutiny of papers before they are sent to publications, yet the papers are sent for publication by those who have left the institutions, by ignoring the contributions made by individuals who conceptualized/planned the portion(s) of the problem. Such individuals who cut corners for petty small gains place their institutions and colleagues at high risk. Now here the guide /supervisor/ head of the institution should play a vital role to see that nothing undue is given to few while others who have really conceptualized/planned and worked hard are often neglected. For scientific community where ethical and moral values are deteriorating fast[4] it is practically difficult to prepare and implement any code of conduct and most of the matters can be settled professionally[1], yet at times the personalized egoism and nepotism are shown thus breaking the career of scholars. Although many guidelines and criteria are available for naming authorship and their sequence[5,6,7], yet these are often been ignored and it is high time that some well recognized leading forum[8] should take lead in setting rules and regulations acceptable to all institutions so that dictorial attitude of science lords/ pseudo scientists/science managers may shatter.

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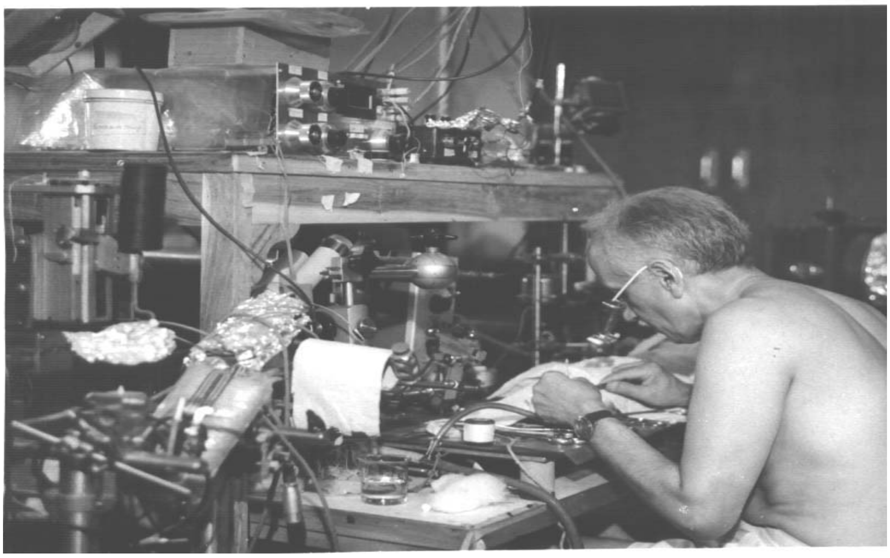
The Times and Life of the SSV Founder President Autar S Paintal (1925–2004)

(Excerpts from the article by Ashima Anand, published in J. Biosci.31(5), 2006,513-524)

1. Introduction

"In a sheaf of photographs on a table is one of an elderly man, clad only in his trousers, the lean muscles standing out in his swimmer's back as he bends over a complicated apparatus garnished with oscilloscopes, recorders, and a massive camera that once formed part of a World War II surveillance aero plane. In many ways it is a classical record of classical science, the scientist in his laboratory at the dead of the night, immune to the passing of time and the fact that he is seminude. The progress of the experiment is independent of the minor constraint of the motion of the earth on its axis, and the shirt is off for what better way than this to beat the Delhi heat" (Fig.1).



Autar Paintal, determinedly at work in his laboratory: having recently been through the most trying years of his life, which were replete with expressions of academic disharmony. Finding an enormous gulf between their achievements and his, some of the faculty – not much younger than him – was trying to shut down his laboratory. His election in 1981 to the fellowship of the Royal Society of London (1981) seemed to have intensified their effort in this direction. Had a passionate involvement with scientific activity not been the fulcrum of his life, and had not his well wishers and admirers at home and abroad stood by him, he would have certainly succumbed to the pressure and closed his laboratory, so tenacious was the exercise to do so.

Paintal was born on September 24th, 1925 in Mogok, a town in northern Burma (Myanmar). His father, Man Singh, was one of the few members of the family who survived the great plague epidemic of 1903. He went to live with an uncle in Burma where he studied at Rangoon Medical College and obtained a licentiate degree to practice medicine. Thereafter he worked for the British Medical Service. But, not unusually for those times, Dr Man Singh found himself in the grip of the movement for independence. His political views were inflected towards socialist ideology and he was fiercely combative with his British employers especially when it came to upholding the rights of the underdog. In his own words, he was “Always in disagreement with his seniors”.

2. Early days

As a youngster Autar Paintal spent a lot of time hanging around the hospital and surgery where his father worked – occasions, which the latter utilized to tutor him in many matters. It is possible that his upright and unbending attitude in issues of right and wrong took root during these exchanges.

He had to change schools every few years, as after every confrontation with the British, his father was posted out, at times to remote mosquito-infested towns as “punishment postings”. He at first attended St Paul’s School in Rangoon and then when the family moved to Mandalay, he went to St Peter’s. Here football was his great passion (on our visit to this school in 2003, a year before he passed away, he was pleasantly surprised to find that the goal posts still stood where they used to). He never lost an opportunity to play truant and go fishing with his friends in the moat that surrounded Mandalay Palace. He recalled that invariably he was caught and caned, as was the practice in those days; he took to wearing two pairs of shorts at the same time to lessen the pain. This may have been an early instance of his devising something simple to overcome a problem, a quality that persisted right until his last days. The extra pockets also allowed him to stow away any unfancied part of his lunch. From Mandalay the family moved to Kalaw, a picturesque hill station in the southern Shan States where he went to Kingswood school. He was ten years old and left in the school’s residence when the family returned to Rangoon. In 1939 when war was imminent and a Japanese occupation likely, he was sent to an aunt in Lahore ahead of the rest of the family to finish his matriculation. He was fourteen years old, studied in the Khalsa High School and worked hard at his lessons without help from anyone. His cousins went to a fancy school and generated a lot of peer-pressure, which he countered by excelling at studies and amusing them with his fund of limericks. Along with them, he also learnt to row the large country boats that were used for ferrying villagers across the river Ravi. Rowing became a favourite pastime and was to remain a way to relax, a form of exercise and a getaway. Much later, in January 1986, when he found his name in the President's list of honours, he bade us pack a picnic and spend the entire day on the river Yamuna, rowing and spotting birds on the water's edge. Sundown made us go back when he had to answer the phone calls from which he could not escape any more.

In Lahore, Paintal attended Forman's Christian College and wrote the Intermediate Examination of Punjab University (1943) after which he joined the family in Lucknow, where his father had chosen to settle down.

3. Medicine and early research

He read medicine at King George's Medical College in Lucknow in 1943, where he was supported by the Burmese government as he was an evacuee from that country. He had to sign a bond to serve Burma after completion of his medical studies but was unable to do so because the conditions prevailing at the time prevented him from travelling there. The years at King George's Medical College (1943–1948) were marked by distinctions, honours and awards, finished off by winning the coveted Hewitt Gold Medal for obtaining the highest marks in the final MBBS examination. For the ultimate quiet that he sought for concentrating on his lessons, he often found himself studying under the bright lights in the grounds of the Lucknow Residency. On other occasions he extended this by holding together a few of his friends in a discussion group in the coffee house in Hazartganj or the "high street" in Lucknow. Leisure time was spent rowing on the river Gomti with friends, and as amateurs the group became skilled enough to win awards in the annually held Regattas of Lucknow University.

One does not know what drove him to the research bench with such intensity after having obtained a degree in medicine, which he did in 1948 with many honours. His enviable performance as a medical student meant that he was expected to take up clinical medicine and make a success of it in more than one way. Clearly, what did take him away from pursuing clinical medicine as a career was the 'duplicity' as he often put it, that he began to encounter in the profession. After the first false report that he was made to sign by his senior, he all but made up his mind not to pursue medicine as his career. His attitude towards such tendencies hardened with time. Later, it was to result in his spearheading the movement for ethical values in the conduct of science; by 1986 he had helped found the Society for Scientific Values – the first of its kind in the world.

Paintal started to work for an MD degree in psychophysiology whilst a lecturer in the Physiology department of his medical college. His thesis was entitled "Electrical resistance of the skin in normals and psychotics". He chose the problem himself and worked on his own. Building apparatus and handling it with great dexterity, he collected extraordinary data and devised an index which came to be referred to as the 'Paintal Index' Since the index was independent of basal skin resistance, it assumed considerable value till more advanced methods were available to psychiatrists to diagnose psychosis (Paintal 1951).

4. PhD work in USA

In 1950, having acquired a post-graduate degree in physiology and brimming with ideas, he applied to the Rockefeller Foundation for a Fellowship to work on a problem which was largely of his own choosing. He arrived in Edinburgh in November of that year to work for a PhD, just three months ahead of Professor David Whitteridge, who was to introduce him to visceral sensory physiology. At that time the department of physiology at the medical school had a reasonable library and workshop but nothing in the way of a well-equipped electrophysiological research laboratory. With help and advice from Jock Austin, an electronics engineer who was his technical assistant, the Professor built up an excellent infrastructure for research and teaching in electrophysiology, and supplemented these efforts with lectures in electronics to students and junior colleagues. Like the other new students, Paintal too was encouraged to spend the first six months building equipment from parts obtained from World War II disposal or surplus equipment. He had become quite skilful at rigging up electrical circuits during his MD studies at Lucknow.

Life for him was quite simple till July 1951 when he decided that it was impossible to record the electrical activity of single nerve fibers using the old "steam box". In those days the entire experimental animal, which was a cat, was kept inside a box in which steam was generated continuously. This was done in order to prevent the dissected-out nerve from which electrical recordings had to be made, from drying up. But a dripping condensate made it impossible to see the nerve fibers through the microscope; dissecting them so finely as to provide identifiable components of the compound action potential was unthinkable. Instead, Paintal thought of doing away with the box and immersing the nerve under study in liquid paraffin, to prevent it from drying. He told the Professor his plan, saying that otherwise it was going to be impossible to dissect out single live nerve fibers from the vagus nerve and measure their individual conduction velocities. This was the object of his study and was necessary for studying the properties of nerve fibers innervating the heart and lungs, which are carried in the vagus nerve. Whitteridge warned him that others before him had tried to dissect and record from single fibers from peripheral nerves under paraffin, but had given it up as a failure. But he persisted. Jock was aghast, warning him, “You are throwing away the Professor’s box, you know! I hope you have a return ticket on the ship to Bombay.” Finally, Paintal did succeed in dissecting nerve fibers under paraffin and recording the electrical activity not only from pulmonary stretch receptor fibers (the ones that respond when lungs are inflated) but also from nerve fibers originating from baroreceptors in the heart (sensory receptors that are sensitive to stretch of the heart muscle). However, it was not until he had recorded from nerve fibers coming from the lung vasculature that the Professor himself worked on, measured their conduction velocity and ‘proved’ it with some more new tricks, that the latter finally got excited.

This technique came to stay and is utilized even today by neurophysiologists in laboratories all around the world. By fearlessly opening the chest and prodding about the heart and lungs with a glass rod, he also succeeded in short-circuiting the Professor’s rather elaborate and indirect ways of localizing cardiac and pulmonary vagal afferent nerve endings

5. After PhD work

He became adventurous after submitting a PhD thesis and decided to find out where in the lungs the pulmonary vascular receptors that Whitteridge had been studying were located**.** To his utter surprise and horror he found them in the left and right atria (i.e. in the heart and not in the lungs).

He knew the Professor would be shocked and did not know how to break the news to him. More data was required which he succeeded in obtaining. After a couple of weeks, Whitteridge accepted the finding and said that he was delighted to have been corrected. This gave Paintal instant fame and notoriety, and also inaugurated a new phase in his career. But at that time he did not realise the importance of the finding, certainly not till he had communicated it to the subsequent Physiological Society meeting, where Whitteridge got up and gave an appreciation of what he called Paintal’s “heroic experiments” (opening the chest of the cat for localizing the receptor with a single active fiber on the electrode). He found this rather embarrassing but enlightening at the same time. When the paper was published in the *Journal of Physiology,* it had only Paintal’s name on it (Paintal1953a). The Professor had shown him the rules of the game. These were, to give credit unhesitatingly where it was due, and to restrict the authorship of papers to those who actually took part in the study. The rules remained with him throughout his life but he found that his insistence on them were a source of great irritation to several of his colleagues in the institutions that he worked in, in India. For him, a legitimate author was one who, finding a pack of slides suddenly thrust into his hand, could give a sensible talk about the paper under discussion, and additionally be able to answer all questions asked.

Thus the first of his significant findings was showing that the “pulmonary vascular fibers” which Whitteridge had reportedly discovered, did not exist. The techniques that he introduced and the sensory receptors that he discovered and described have become the building stones of our present knowledge of visceral physiology. Amongst the cardiac receptors that he discovered, are the type B atrial receptors which signal the amount of atrial filling and the ventricular pressure receptors which cause a fibers fall in blood pressure when pressure inside the ventricles rises; the work came to be included in the Benchmark Series, as one of the classical papers in cardiovascular physiology. Other discoveries were the gastric stretch receptors, mucosal mechanoreceptors of the intestines, pressure-pain receptors of muscles and contributions to the understanding of chemoreception in the peripheral (arterial) chemoreceptors.

From the very beginning of his career, Paintal had to make do with meagre resources. He not only taught himself “mathematics for technical students” and “physics for biologists” but also the German language — a fair number of the classical papers in physiology that he wished to read were written in German. He made several electronic circuits in his laboratory in Delhi and was able to repair his Beckman and Grass preamplifiers and stimulators most of the time. David Whitteridge remarked once that Paintal was probably the only Director in the world who wields a soldering iron. According to him, his laboratory in Delhi took form as a result of friendships with the electronics technicians in all the laboratories that he had worked in. After Jock in Edinburgh came Harry Feintuch at the University of Utah. In 1958, as he was about to leave the US for India, he received a telegram from Professor Kurt Krämer, the Director of the Physiologisches Institute, Göttingen, inviting him to spend some time there as a Guest Professor, and to demonstrate to the Germans too, his newly discovered neuro-physiological techniques that had been fascinating laboratories all around. The equipment which he wished to use had been built in the university’s workshop in Utah and was lying in a ship in Genoa destined for Bombay. So he went to Genoa, got it out of the hold and brought it to Göttingen where with the help of the electrical, mechanical and electronic workshops he built some more bits and got the experimental set-up going.

Throughout his life, holidays and weekends saw him either repairing his own or a student’s equipment or getting it ready for the next experiment. He remained in admiration of electrical engineers, who he said “are priceless people, they are like neurologists in tracking down the site of lesions”. When computerized equipment took over physiology laboratories, he looked upon them with disinterest – not being able to calibrate them and not being able to identify the source of the problem when they stopped to function. Indeed, more so because colleagues started to talk in terms of “cleaning up their data” with the help of computers.

6. Back in India

From Edinburgh he went to work in Kanpur (1952–1954) at the Technical Development establishment Laboratories (TDE) of the Ministry of Defence, after seeing the post of a Technical Officer advertised by the Union Public Service Commission. The agreement with the Rockefeller Foundation had demanded that he return to an institution where he would also teach, but the hiring criteria at his parent institution in Lucknow showed scant interest in his scientific achievements or capabilities. After a harrowing exchange of letters between the Lucknow University, the Foundation and himself he was freed to take up this assignment. By contrast, the Superintendent of the TDE laboratories, Dr T S Subramaniam, showed great generosity and allowed him to carry out the work begun in Edinburgh, outside normal working hours and on holidays. His main assignment was to develop suitable clothing keeping in mind the equipment that the armed forces worked with and the extremes of temperatures that it was carried out at. Here he discovered the gastric stretch receptors (the ones which are responsible for the immediate satiation of hunger and thirst). With this he opened up the study of electrophysiology of sensory mechanisms of the gastrointestinal tract. But to him the finding did not appear to be glamorous enough, and yet it seemed to have given him the momentum that culminated in subsequent discoveries. By the end of two years he began to despair in the academic wilderness that he had got into. After making sure that such a thing would not happen at the Vallabhbhai Patel Chest Institute, Delhi University, he accepted the position of an Assistant Director there (1954–1956). He had been in pursuit of a vagal sensory receptor that he first encountered in the Edinburgh days. It was late one night (or early in the morning) in 1954 that a thin nerve fiber responded with a volley of impulses within 2.5 s to a right atrial injection of phenyl diguanide (pdg), an amidine derivative which produces the cardiovascular and respiratory fibreses that he had begun to study in Edinburgh. A further study and its identification marked the important discovery of a lung sensory receptor, which later was to be named the juxta-pulmonary capillary or J receptor.

This was the first great discovery in the field of medicine in independent India; there had not been one of comparable impact since Ronald Ross’s finding of the role of the mosquito in the life cycle of the malarial parasite. Paintal had wished to preserve for posterity the bit of lung from which he had just recorded, but unfortunately it had to be thrown away: at the time, the rules of the Institute did not allow him to issue out formalin from the stores. Later, as Director of the Institute (1964–1990), he tried to streamline its functioning by keeping the interest of the research investigator foremost.

7. The person

In most matters of life Paintal was painfully naive and quite incompetent at guessing what was expected of him. On this being pointed out, he would remark that his training as a scientist had rendered him incapable of such “psychic attributes”. He was at the same time full of fun and dare and had a sense of humour, which navigated him through life. At committees and meetings his eyes would blaze with anger at the first hint of dissembling, but would sparkle with mischief as he made light of other serious moments with jokes and limericks.

He was an unaffected man who would make tea for all visitors to his lab and wash up later. The Vice-Chancellor’s office in Delhi University was always amused to find him telephoning them directly and answering their calls without an assistant. Summer or winter, it was not unusual for him to walk across to the University for a meeting, the relevant files tucked under his arm. He was happy to have the Director’s office double up as his laboratory, which was especially convenient if an experiment was on; the experiments could easily go well past midnight. He was not even a Professor of Physiology at the Chest Institute — just the Director doing his own research.

Paintal pursued scientific activity for science’s sake, which required him to sacrifice social success and personal security. He found that leading such a life was full of excitement and freedom. He was part of a large family and his playing fields extended to Europe and North America. He admitted that life for a scientist in India was almost an impossibility; “One had to spend 12 to 14 hours daily in the laboratory and that left little time for observing the innumerable ceremonies, festivals, fastings and relatives; the latter had to be received, entertained and seen off often enough.

His pleasures were simple. Disappointments with failed experiments were overcome by going off for a walk or bird watching if the weather was cold, and for a swim. If it was warm. When writing to his friends about his good and bad experiments and where they were leading, there would always be a paragraph devoted to such activities. To Richard Riley, an old colleague, while describing our newer experiments that were still holding up the model of carotid body chemoreception that he had proposed earlier, he wrote “We made it to the Wazirabad bridge thrice (!) this year. This was entirely because of ‘foreign aid’. We had Richard Iggo, Ainsley ’s son (Ainsley Iggo was *a collaborator from Edinburgh*) with us and he is an expert punter. So with us rowing and him punting we made it with great ease. It was also amazing that two years later we should see the white breasted kingfisher precisely in the same location, on a branch sticking out from the eucalyptus grove.

8. Conduct and goals of scientific activity

He put in as much effort as he could to highlight, nationally and internationally, India's contribution in his field. To him, organizing a meeting or a symposium seemed appropriate only when there were advances to be talked about and taken advantage of; the three symposia that he organized in his lifetime were just such occasions. He published slowly and with care and recommended that the best journals be sought if findings were not to be lost from view. He was, however, appalled to find that the number of citations that a publication received was being used as a criterion of achievement in science. "This has had an adverse effect by reducing discoveries and advances. Many scientists do not have goals relating to making of discoveries or making inventions or trying to apply science for social needs. One gets the impression that scientists are not unhappy on account of not having made an important discovery. They are happy because their papers are being cited in peer-reviewed journals. Citations can be considered as being equivalent to the ovation given to performing artists such as musicians. Can the intensity of such ovations (i.e. number of people clapping) be considered as the brilliance of the music composition itself?"

His obsession with maintaining standards and ethics in science was well known and he never minced words while talking about it, either privately or publicly. After having served for a spell as the convener of the Biological Sciences Panel, he wrote to the Chairman of the University Grants Commission, "I have got the firm impression that the members of the Panel and other referees to whom some proposals have been sent, do not go through the proposals carefully. Most of the proposals themselves are poorly written up. They do not begin from a particular point in existing knowledge and as a result they do not seek definite answers. In my opinion they are not useful to our country and it is quite easy for me to see that only an appearance of applied research has been given. Although I have painted for you a logical picture, let me assure you that the Biological Sciences Panel is in a far superior to the situation to the one that prevails in the field of medical sciences e.g. Physiology. The main purpose of these research projects is to expand one's research empire, have large laboratories full of equipment, carry out mundane work and produce large numbers of papers that the publishers are only too happy to publish for the sake of their own existence."…. "I have been searching very hard for the past 8 years for the means by which we should improve the scientific standards of work done by our scientists. I have even tried to set an example, but I do not suppose it (the example) has been given enough for others to follow. Doing my own experiments, analysing my own results, repairing my own instruments and cleaning up my own apparatus, is not an example that has been followed. In the field of biology it has been demonstrated for over 100 years that the best research was done by the biologist himself with the help of a collaborator. It is therefore not surprising, that the quality of biological research is not high because nearly all investigators get their work done entirely by research fellows ranging from 4 to 20 or even more".

He advocated international scientific collaboration (especially as the Director-General of the Indian Council of Medical Research), but not AID. Not surprisingly, he always talked fearlessly about intellectual independence. After one such occasion in 1988, where told the graduates of the University of North Bengal 'think for themselves and not be dependent on others', the Ambassador of the United States asked him to desist from making such statements adding, by way of threat, "It will be bad for you".

9. Society for Scientific Values

In about the mid-1970's several instances of scientific misconduct in India began to be written about in science magazines the world over, he became concerned about the declining standards of scientific ethics in the country and started to speak about this and about the lack of goals and absence of accountability in Indian research activity. In 1986, after having conducted many rounds of informal discussions, he and several others who were similarly distressed by the situation sent out a circular to a large number of scientists in the country, explaining the necessity of founding a society to be called the 'Society for Scientific Values'. An excerpt from the circular reads as follows. "After independence, India has made considerable investment for the development of science and technology. There are many scientific and technical institutions, some of which have been very well equipped. However, the scientific contributions have not been commensurate with the investment. In fact, hardly any discoveries, innovations and technologies have originated in the country in recent decades. There are several reasons for this, e.g. inadequate salaries and other needs such as housing, transport, schooling, and medical facilities and so on. But these are not the main reasons, as these facilities were not better before independence when some outstanding contributions of great importance were made in science in India. It is the lack of healthy scientific environment which has been throttling the creative potential of Indian scientists and technologists". According to him a healthy scientific environment was one that was free from prejudices, bureaucratic formalisms, dishonesty, propaganda of unsubstantiated research claims, suppression of dissent, showmanship, sycophancy, political manipulation, maneuvering, and so on. He further advocated that it was of "utmost importance to promote, by personal and collective efforts, the ethics and norms of science not only for the progress of science and technology in the country but also for building its national character". This society was the first of its kind in the world and though not immediately, its creation made the International Council of Scientific Unions aware of the necessity to direct all constituent scientific academies of the world to create similar sections in their organizations which would set down guidelines and be seen pursuing them (for information about the aims and activities of the Society for Scientific Values, In his professional lifetime he had seen the advent of newer techniques and approaches, with molecular biology heading the list. Did he feel that the days of integrative systems physiology were numbered, especially when he saw the younger generation of physiologists and biologists in India referring to his science as "old fashioned". I think not. For most of the time he found that they did not have questions to answer and appeared only to be following the fashions of the day.

— P.N.Tiwari

Science Education in Universities

P. N. Srivastava
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I have been associated with science teaching in universities for more than fifty years. I am well aware with the situation prevailing in both the central and state universities and have, therefore, been very much concerned about it. In terms of publications of scientific research in the world, India ranked 8th during 1980s, slipped to 13th during 1990s, and nose-dived to 21st the present decade. I am not too sure if it has been only because of the deterioration of universities. My Presidential address in the Indian Science Congress session, 1994, dealt mainly with this issue. There were a number of editorials written in national dailies emphasizing that my warning had come not a day too soon. Eminent scientist and educationist Nobel Laureate Sir John Kendrew, who had attended the Congress, had also written to me that "Your congress was a great success and I particularly appreciated your forthright remarks about science education and the contemporary problems of Indian science. I only hope they have met with some response from your government authorities ". Nothing of the kind happened.

My science congress address was in response to the recommendations of the Scientific Advisory Council to the PM in 1990. They had accepted that science education was bad in the country. Fifteen sub-committees were constituted to discuss the issue and recommended how different areas be developed in various national laboratories and that some of them be given the status of deemed universities. It is amazing that not one sub-committee was constituted to discuss science education in universities and no recommendations were made how to improve them.

It is a matter of great satisfaction that at last the Scientific Advisory Council meeting in last July came strongly in support of science education and universities. The address of the Chairman, C. N. R. Rao, "Science in Crisis: A Commentary on the Present State of Science in the Country" deserves full support with all seriousness Soon after that the Indian National Science Academy, New Delhi and the Indian Academy of Sciences, Bangalore, focussed their attention on increasing and improving the 'supply side' of the scientific and technological community. A major change is needed to attract a large number of young Indians to science-based careers. They have recommended to the Planning Commission to come forward with a support of Rs. 7,334 in the XI Plan. The support, as a first step, is basically meant for special assistance to ten premier universities, up gradation of state universities, undergraduate science teaching in leading universities and IITs and enhanced assistance to 200 undergraduate colleges etc. I only wished they had also added in this list 1000 schools apart from Central schools and Navodayalas as well. I hope the Planning Commission would consider the recommendations favourably. Their recommendations are a necessary complement to the public investment. It will help to sensitize the society at large to the possibilities that such an investment will help realize.

The same Scientific Advisory Council, two years back had strongly recommended the institution of two Indian Institutes of Science Education and Research (IISER) at Pune and Kolkata. They had been allocated a sum of Rs. 500 crore each to start with. Later on the third one was added at Mohali. The Ministry of Human Resource Development has recently cleared the fourth one in Orissa. The fifth and sixth ones are in the pipeline for Bhopal and Thiruvnanathapuram. I am sure further requests would come from other states as well. I am not against this at all but why is it that at least the better universities were not considered suitable to provide good science education? Why do we show this distrust for the universities? I wonder how many countries have such diverse categories of institutions for science education namely universities, deemed universities, national laboratories. IISERs etc. I am afraid that in case of any financial crunch it would first fall on the universities. It is fortunate that it is only the present government that has accepted the recommendations of the Kothari Commission, 1966 and the National Policy of Education, 1986 that a minimum of 6 per cent of the GDP be spent on education although all the previous governments had promised it. Further, it is heartening that the Planning Commission is going to raise the funding for education in a big way in the XI Plan

The parliament has reserved 27% of seats for OBCs in higher education across the board and has ordered the increase of that percentage of seats in universities and colleges for which funds would be provided by the government. It is no doubt necessary in the over all interest of the nation. However, it has recently been reported that Moily Committee, at present, has accepted only about 20% of the amount requested for by JNU and Delhi universities for infrastructure, faculty and other requirements. This would be perhaps true for all other universities as well. Each IIT will have to induct about 70 faculty members each year in every institute. Already in IITs, IIMs and premier universities a fair percentage of posts have not been filled up since proper candidates were not available. Infrastructure and buildings can be provided and built but good faculty cannot be produced on demand. If unsuitable faculty is appointed today, they will be there for about forty years. How are the institutions going to manage this increase? Teaching does not mean only providing tables and chairs. Our parliament should discuss this issue as well. If proper support were not forthcoming, it would only result in further deterioration in standard of teaching in universities and colleges that is even now in a poor state.

Spotting fraudulent claims in science

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Most scientists are honest, while some will commit scientific fraud by deliberately deceiving colleagues and/or the public with a false claim. They may report experiments that have never taken place, describe patients that do not exist or distort data and illustrations to appear more convincing. Norwegian physician Jon Sudbø invented some 900 patients in a study published in The Lancet in 2005, claiming that common painkillers help protect against oral cancer. [1] German physicist Jan Hendrik Schön falsified data in multiple papers, including 15 publications in the top-ranking journals Nature and Science. Most recently, South Korean scientist Hwang Woo Suk fabricated data published in Science, claiming to have grown stem cells from human embryos. [2,3]. In all three cases, retractions occurred after the fraud had come to light.

It is important that the media report on scientific fraud in order to hold the scientific community accountable for maintaining standards in research — which is often funded with public money. The scientific community should not only act swiftly to punish fraud, but it should also raise questions about the failure of co-authors to know and understand more about the work being published and to prevent the fraud occurring. By publicising fraud, the media can also help to protect the public against fraudsters who, for example, cause patients to delay getting appropriate treatment in preference for unproven medications — as happened recently with AIDS patients in South Africa. [4] Furthermore, the media's reputation is at stake if a fraudulent claim has had prior publicity.

Why is fraud so difficult to detect?

Scientists, as a rule, follow an accepted code of conduct. They begin with experiments designed to answer a scientific question or create a new product. They present their results to colleagues and then publish them in a scientific journal. A good quality journal requires independent experts to certify that a paper's results are valid — a process known as peer review.

The process of peer review means journalists can usually assume that published work is of a high standard and worth reporting. And this is usually true. But peer review is not designed to detect fraud, and peer reviewers and journalists alike can be fooled by fraud that is well disguised.

After all, reviewers do not witness the experiments, so they must trust that the study is honest, and may not notice if data are fabricated or altered. The fraud often only comes to light when other scientists are unable to replicate results. Non-expert journalists have little chance of uncovering such deception.

Sometimes, however, fraudsters have so obviously flouted the normal standards of scientific conduct that well-informed journalists are as capable as scientists in raising the alarm. For example, they may omit scientific evidence altogether and rely on anecdotal observation — even in a published report. In clinical studies, they may fail to register details of their experiment to regulatory authorities or refuse to make test results available for independent analysis.

How can you get better at detecting fraud?

The following tips are intended to make journalists better equipped for judging the quality of scientific claims and detecting fraud:

Get to know a field of research

Attend scientific conferences or visit research institutes and meet scientists in your area of interest to find out their goals, methods and progress and also the type of criticisms they may have of each other's work.

Visit university libraries, or use internet databases such as PubMed to find publications on a particular topic or by a certain author. This will provide more insight into individual studies. Although primary research papers may be too full of jargon and technical detail to make much sense to a non-specialist, review articles, which explore ideas and hypotheses, may be easier to follow and present a more general view of a fields progress.

Check the quality of peer review

Ask the scientist whether their claim is published in a peer-reviewed journal. Even if the answer is yes, do not assume this to be a mark of quality — different journals have different criteria and practices, and the quality of their peer review varies accordingly. It is therefore important, if possible, to find out the quality of the journal in question. To do so, consult scientists directly, or check with university librarians that the journal is held in high regard. High quality journals tend to be more widely read and more frequently cited in academic papers. Journalists may also wish to try the internet search engine Google Scholar, a free resource that rates results according to the number of times a paper is cited by others, and hence indicates relative importance in the scientific community.

If you are uncertain about the journal's quality, try to find out the limitations of the study. Was it too preliminary, or too small a sample size to be accepted in a higher quality journal? An honest scientist will readily admit to the weakness of a study, and the need for further research — a less scrupulous one may instead exaggerate the importance and significance of the results, and deny that any data are lacking.

If you discover that a study has been refused publication, find out why. It may be honest work, but poorly designed, or insufficient in some way. Alternatively, it may simply have been submitted to an inappropriate journal — good science, but too narrow in scope for a broad-interest journal such as Nature or Science, for example. Then again, the authors may have refused to redesign or expand their study, for fear that their assertions will be proved wrong.

Question the numbers

Are the numbers involved in a study appropriate and sufficient for the kind of investigation involved? Clinical trials, for example, proceed through three recognised phases from initial safety trials of just a handful of individuals to larger trials of effectiveness involving hundreds and then thousands of people. This will reveal whether or not a result has arisen by chance (its statistical significance), enabling conclusions to be drawn with greater certainty. Even if the statistics appear to back the claim, they are still worth checking with an independent expert, as mistakes can and do occur, including in the top journals.

Be critical if the claim is made in a public statement

A journalist hearing an unpublished claim during an interview, press conference or seminar, should dig deeper to investigate how the study has been conducted. Ask the following questions (which can also be applied to a published study):

- How credible is the scientist among his/her scientific peers? Asking other scientists directly can be a quick indication. Otherwise, checking through an internet database such as PubMed may indicate how often the person's work is cited by others.
- Is the scientist based at a recognised scientific institution?
- How is the study funded? A publicly funded study, for example, has had its protocol scrutinised by experts in order to compete against others for funding.
- Is the author likely to profit from the sale of products relating to the work? Although many journals require authors to declare any competing financial interests, some scientists fail to do so.

Find experts for advice and comment

Finding an independent expert to comment is the most reliable way to judge the validity of a study. When interviewing a scientist, ask them for the contact details of other scientists doing similar work. Alternatively, identify a relevant expert by checking the editorial board of a journal – as long as it is a reputable one. Use the PubMed database to see who has published on the topic. Or go through the list of speakers at a relevant conference, which you may find advertised in a journal, or on the website of a scientific society. Local universities, research centres, funding agencies or government departments may also provide a list of academics willing to talk to the media.

Check for ethical and regulatory approval

If the study is a clinical trial, and claims to provide evidence for a treatment, vaccine or cure for a disease, check that details concerning the drug or vaccine composition, and any toxic side effects, are publicly available. Make sure that the investigators are officially registered medical practitioners and that the trial or product has both ethical and regulatory approval — either for experimentation or for sale. There are now public databases, such as the US National Institutes of Health service, ClinicalTrials.gov, where clinical trials may be registered and which all top quality journals now insist should be referred to in published papers.

Be sure of the facts

Journalists must be certain of their evidence, as an accusation of fraud could leave someone's career in ruins. They should check their facts with more than one source, and also anticipate that they may have difficulty in persuading some researchers to speak out against a colleague. An accused scientist may threaten to sue a journalist or their paper for libel, in which case it may be wise to seek the advice of a lawyer before publication.

In conclusion, it's worth remembering that most science is honest, and fraud is difficult to detect. In following the steps above, however, a journalist can certainly enhance their skills and reputation for reporting accurate and good quality scientific studies, and may be catch a fraudster in the act.

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Links

News and information on research fraud

1. Under the Microscope Times Online (2006)
2. The Trouble with Replication Nature 442, 344-7 (2006)
3. Your Cheatin' Heart Nature Medicine 12, 490 (2006)
4. Catching the Crooks Nature Medicine 12, 490 (2006)
5. Nature online debate on peer review
6. Wikipedia on peer review and fraud
7. How to Spot HIV/AIDS Fraud AIDS Infonet Fact Sheet 206 (2006) [32KB] AIDS Truth
8. Council of Science Editors' White Paper on Promoting Integrity in Scientific Journal Publications

Organisations working against research fraud

1. UK Panel for Health and Biomedical Research Integrity
2. US Government Office of Research Integrity
3. UK Committee on Publication Ethics
4. World Association of Medical Editors

Jailed For Fraud In Research

(Excerpts from the article written by Jeneen Interlandi Published in the New York Times Magazine on October 22, 2006 /section 6

Before his fall from grace, Eric Poehlman, a tenured faculty member at the University of Vermont, USA oversaw a lab where nearly a dozen students and postdoctoral researchers carried out his projects. His research earned him recognition among his peers and invitations to speak at conferences around the world. And he made nearly $140,000, one of the top salaries at the University of Vermont. All of that began to change six years ago, when DeNino took his concerns about anomalies in Poehlman's data to university officials. The subsequent investigation — a collaboration among the University of Vermont, the Office of Research Integrity (which is within the Department of Health and Human Services) and the United States Department of Justice — uncovered fraudulent research that stretched back through almost half of Poehlman's career. The revelations led to the retraction or correction of 10 scientific papers, and Poehlman was banned forever from receiving public research money. He was only the second scientist in the United States to face criminal prosecution for falsifying research data.

In the fall of 2000, Walter DeNino was comparing measurements in some data Poehlman had given him when he found something odd. DeNino, who was then 24, had started working in Poehlman's lab during his senior year at UVM. The young man admired the senior researcher and enjoyed the camaraderie of his peers. Poehlman pushed his team hard, but he was also charming and energetic, and he attracted people who were active athletes like himself. Lab members regularly went to the campus gym during lunch breaks and often competed in marathons together. Poehlman ran daily with students and colleagues alike

Professionally ambitious, DeNino graduated with a double major in nutritional sciences and dietetics at UVM and won several awards for the research he completed under Poehlman's tutelage. When Poehlman invited him back to the lab as a paid technician — allowing him to continue to train for the Olympics part time – DeNino saw it as the ideal way to strengthen his candidacy for medical school. Although technicians are usually at the bottom of the lab hierarchy, carrying out the more mundane aspects of their principal investigator's work, a generous scientist will give them credit on publications. DeNino figured that getting his name on one of Poehlman's papers could make the difference on a med-school application.

The fall that DeNino returned to the lab, Poehlman was looking into how fat levels in the blood change with age. DeNino's task was to compare the levels of lipids, or fats, in two sets of blood samples taken several years apart from a large group of patients. As the patients aged, Poehlman expected, the data would show an increase in low-density lipoprotein (LDL), which deposits cholesterol in arteries, and a decrease in high-density lipoprotein (HDL), which carries it to the liver, where it can be broken down. Poehlman's hypothesis was not controversial; the idea that lipid levels worsen with age was supported by decades of circumstantial evidence. Poehlman expected to contribute to this body of work by demonstrating the change unequivocally in a clinical study of actual patients over time. But when DeNino ran his first analysis, the data did not support the premise.

When Poehlman saw the unexpected results, he took the electronic file home with him. The following week, Poehlman returned the database to DeNino, explained that he had corrected some mistaken entries and asked DeNino to re-run the statistical analysis. Now the trend was clear: HDL appeared to decrease markedly over time, while LDL increased, exactly as they had hypothesized.

Although DeNino trusted his boss implicitly, the change was too great to be explained by a handful of improperly entered numbers, which was all Poehlman claimed to have fixed. DeNino pulled up the original figures and compared them with the ones Poehlman had just given him. In the initial spreadsheet, many patients showed an increase in HDL from the first visit to the second. In the revised sheet, all patients showed a decrease. Astonished, DeNino read through the data again. Sure enough, the only numbers that hadn't been changed were the ones that supported his hypothesis.

Confused by the discrepancy between the data sets, DeNino went back to Poehlman and asked to see the patient files. When Poehlman brushed him off, a disquieting feeling came over DeNino. Seeking advice, he e-mailed Andre Tchernof, a former postdoctoral fellow of Poehlman's who had recently left to head his own lab in Quebec City. Tchernof confided to DeNino that something similar had happened before to another lab member.

''He confronted him with the fact that it did not add up,'' Tchernof wrote in an e-mail message to DeNino. ''The response was a job-loss threat, more or less.'' Tchernof warned DeNino to proceed cautiously. Being associated with either falsified data or a frivolous allegation against a scientist as prominent as Poehlman could end DeNino's career before it even began. Poehlman also had a reputation for playing favorites in the lab. While DeNino had always been on Poehlman's good side, both he and Tchernof had seen Poehlman reduce other subordinates to tears for relatively minor infractions. If Poehlman's career was on the line, there was no telling what he would do to protect himself.

DeNino shared his concerns over the data with a handful of graduate students and postdocs and discovered that others had questions, too. Emboldened, he approached Dwight Matthews, a faculty member who shared lab space with Poehlman. Matthews and Poehlman had written a number of papers and grants together over the years, and DeNino worried that Matthews might alert Poehlman to his suspicions. But DeNino could not shake the feeling that Poehlman was hiding something, and he wanted guidance from a faculty member.

''First, understand that no matter how you proceed, everyone loses,'' Matthews told DeNino when they met to discuss Poehlman. ''Your career will be ruined because no one is going to protect you.'' Matthews was brutally frank. ''The university will come out bad,'' he continued, ''and Eric's reputation will be destroyed.'' He told DeNino that he would have to decide for himself what to do. As an afterthought, Matthews told me in a recent interview, he offered one suggestion: ''If you're going to do something, make sure you really have the evidence.''

DeNino spent the next several evenings combing through hundreds of patients' records in the lab and university hospital, trying to verify the data contained in Poehlman's spreadsheets. Each night was worse than the one before. He discovered not only reversed data points, but also figures for measurements that had never been taken and even patients who appeared not to exist at all. In the mornings he would return to the lab and continue working as Poehlman's technician, waiting for the right moment to confront the principal investigator.

The scientific process is meant to be self-correcting. Peer review of scientific journals and the ability of scientists to replicate one another's results are supposed to weed out erroneous conclusions and preserve the integrity of the scientific record over time. But the Poehlman case shows how a committed cheater can elude detection for years by laying on the trust — and the self-interest — of his or her junior colleagues.

The principal investigator in a lab has the power to jump-start careers. By writing papers with graduate students and postdocs and using connections to help obtain fellowships and appointments, senior scientists can help their lab workers secure coveted tenure-track jobs. They can also do damage by withholding this support.

A less self-assured person than DeNino might never have questioned Poehlman's revised data in the first place - - and not just because it might have meant risking future job prospects. The principal investigator is not just a boss; he is also a teacher with knowledge and experience. ''Trust is an essential component in any relationship, but especially between a student and mentor, especially in a research environment.Once that trust has been breached, the aftershocks can be far-reaching.

Not only does any research touched by tainted data have to be re-examined, but high-profile cases of misconduct can also shake public confidence. ‘’We already have a large subculture in society of people who don’t trust science to begin with,’’ says John Dahlberg, one of the Office of Research Integrity investigators who oversaw Poehlman’s case. ‘’This doesn’t help at all.’’

In late October 2000, as DeNino’s suspicions festered, Poehlman was presented with the Lilly Scientific Achievement Award at the annual conference of the North American Association for the Study of Obesity. The weeklong event took place in Long Beach, Calif., and Poehlman and his entire lab flew out to celebrate. More than 100 scientists attended Poehlman’s 40-minute lecture, in which he summarized the research he had conducted over the past decade. Much of the work he discussed focused on energy dysregulation – an imbalance between the energy a person consumes and what he expends. This imbalance grows more pronounced as a person ages, or when a woman reaches menopause, and it can lead to a loss of muscle mass and an increase in total body fat, which, in turn, may predispose older people to obesity and cardiovascular disease.

Because most studies that examine the physiology of aging look at only one point in time, researchers can’t tell whether the differences measured are because of age, menopause or individual variation. Poehlman’s longitudinal study on menopause collected the same measurements from each person twice over a six-year period. This enabled him to show, for the first time, that some metabolic changes were from menopause, not aging. Published in 1995 in the Annals of Internal Medicine, the study confirmed a long-held assumption and helped establish Poehlman’s reputation.

As he summarized this and other work for his colleagues, Poehlman exuded grace and confidence, mixing scientific slides with pictures of his lab team and waxing philosophical about the themes that had shaped his career. He mentioned the outstanding work of his assistants and concluded with the following advice: ‘’Work with people who are smarter than you.’’

Andre Tchernof left Poehlman’s lab two months before the Long Beach meeting but attended the Lilly Award lecture with his former labmates. The facet of Poehlman’s research that most interested Tchernof involved whether hormone-replacement therapy — providing menopausal patients with supplemental estrogen – could help them lose weight. When he was still in Poehlman’s lab, Tchernof had analyzed H.R.T. data from what was apparently the same group of patients Poehlman was discussing in his talk and had found no significant difference in abdominal-fat loss between women who took estrogen and women who didn’t. But the slides Poehlman presented at the meeting showed a big difference: women who had received estrogen lost twice as much abdominal fat as those not taking supplements.

''I have no idea where those numbers came from,'' Tchernof whispered to DeNino, who was sitting next to him in the front row. ''That's not what we found, at all.''

By December, DeNino's relationship with Poehlman had deteriorated badly. When Poehlman learned that DeNino was raising questions about data integrity with other people in the lab, they all but stopped talking to each other. Late in the month, following a series of letters, e-mail and confrontations between Poehlman and DeNino, none of which laid the technician's concerns to rest, DeNino went to Thomas Mercurio, the university's general counsel, to lodge a formal, written accusation of scientific misconduct against his former mentor. The process he set in motion would take almost six years to conclude.

Dr. Burton Sobel, the chairman of Poehlman's department, was determined to do everything by the book. After meeting with DeNino, whom he later described as ''forthright, composed, clearly troubled,'' he contacted Poehlman, saying he needed to discuss something unpleasant with him. Sobel was astonished by Poehlman's response. The accused scientist gave him the impression that nothing was wrong and seemed mostly annoyed by all the fuss. In his written response to the allegations, Poehlman suggested that the data had gotten out of hand, accumulating numerous errors because of handling by multiple technicians and postdocs over the years.

Two days after DeNino filed his formal accusation, Richard Galbraith, the program director of the university's General Clinical Research Center, escorted the campus police chief to Poehlman's office. It was the week between Christmas and New Year's Day, and only a handful of researchers continued to hunch over their lab benches. Galbraith had the task of impounding the evidence necessary for an investigation into DeNino's claim.

The inquiry itself fell to five faculty members picked by the Dean of the medical school.

On Feb. 9, 2001, Poehlman appeared before the panel to address the charges against him. Poehlman's entire defense seemed to hinge on a series of flimsy notions. First, he attributed his mistakes to his own self-proclaimed ineptitude with Excel files. Then, when pressed on how fictitious numbers found their way into the spreadsheet he'd given DeNino, Poehlman laid out his most elaborate explanation yet. He had imputed data — that is, he had derived predicted values for measurements using a complicated statistical model. His intention, he said, was to look at hypothetical outcomes that he would later compare to the actual results. He insisted that he never meant for DeNino to analyze the imputed values and had given him the spreadsheet by mistake. Although data can be imputed legitimately in some disciplines, it is generally frowned upon in clinical research, and this explanation came across as hollow and suspicious, especially since Poehlman appeared to have no idea how imputation was done.

When the university decided to proceed from an inquiry to a formal investigation, Poehlman fought back, seeking an injunction from Judge William K. Sessions against UVM and stalling the proceedings for several months. During this time, Poehlman tried to undermine DeNino's credibility as a whistle-blower by suggesting that the technician was homophobic — Poehlman is gay — among other things. Eventually, DeNino hired his own attorney, Philip Michael of the New York firm Troutman Sanders.

Poehlman's desperate defense, however, eventually unraveled. After the injunction was dropped, a two-year investigation by UVM led to further review of Poehlman's work by the integrity office. By March 2005, his case had expanded to include a rare criminal prosecution by the United States Department of Justice. By then Poehlman had left UVM for the University of Montreal. But with the threat of prison looming, Poehlman changed his mind and offered his full cooperation.

He waived his right to an indictment and pleaded guilty to falsifying information on a federal grant application. He also agreed to pay $180,000 to settle a civil complaint filed by the University of Vermont plus $16,000 in attorneys' fees for DeNino. The plea came with an especially devastating admission: he acknowledged that his most noted research, the longitudinal study on menopause, was almost entirely fabricated.

Federal sentencing guidelines called for five years in prison based on the amount of grant money Poehlman had obtained using fraudulent data. But no scientist had ever spent time in prison for fabricating data.

The sentencing judge was William Sessions, the same judge to whom Poehlman denied all allegations of misconduct at the injunction hearings four years earlier. He told Poehlman to stand and receive his sentence: one year and one day in federal prison, followed by two years of probation.''When scientists use their skill and their intelligence and their sophistication and their position of trust to do something which puts people at risk, that is extraordinarily serious,'' the judge said. ''In one way, this is a final lesson that you are offering.''

— P. N.Tiwari

Membership of the Society for Scientific Values

Scientists who wish to join the efforts of the Society to promote ethics (support right and oppose wrong) in scientific research, development and management and, who meet the following requirements are welcome to become the member of the society.

1. He/she should have allowed his name to appear as an author in only those publications in which he/she was actively involved, in data collection, theoretical formulation, design and construction of apparatus, field trips, mathematical derivation and calculations, statistical analysis and interpretation of results, as distinct from administrative support and providing funds or facilities.
2. He/she should have never plagiarized or made false claims or indulged in or supported and encouraged any kind of unethical activity in science.
3. He/she should agree to withdraw from the Society if he/she ceases to adhere to the requirements 1 and 2 above.

A scientist who wishes to become member should send his brief biodata to the President or Secretary of the Society. A member of the Society may also send biodata of such scientist for the membership. Non-scientists who have promoted ethics in their profession can also become member of the Society.

Volume 5 **April, 2007** **No. 1**

Main objectives of the 'Society for Scientific Values

1. To promote objectivity, integrity and ethical values in pursuit of scientific research, education and management, and
2. To discourage the unethical acts in these areas

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