Scientific research as a career: Challenges and obstacles

M. K. Chandrashekaran

Of all the communities available to us, there is not one I would want to devote myself to, except for the society of true searchers, which has very few living members at any time.

A. Einstein in a letter to Max Born in 1924.

Good scientists generally do better if given a boost in the form of some kind of peer recognition. But please remember all manners of selection processes are fallible and there are always the richly deserving ones who, alas, get overlooked. But genuine excellence will express itself sooner or later and cannot be suppressed for too long. But during the period of waiting for recognition we would do well to recall that a tremendous amount of great science had been already done before Alfred Nobel wrote his will.

Scientific research as a career has had its own mysterious lure in many countries and cultures. The rewards are often not visible and all too rarely tangible. According to Pierre Auger's estimate (now a cliche) 90 per cent of all the scientists that have ever lived are alive today. But the word 'scientist' itself was not coined in English until 1841. There were 'men of science' who frowned at being called scientists. As late as 1895 the *London Daily News* was still protesting about the word 'scientist' as 'this American innovation'. T. H. Huxley complained only a hundred years ago "To anyone who respects the English language, I think 'scientist' must be as pleasing as 'electrocution'". For all its lack of pedigree the profession has always attracted men of eminence.

Even though one would not, *a priori*, expect it, an extensive scientific psychological study has established that there is a certain distinctive combination of traits which characterizes the scientific personality. The study referred to US research scientists and there are no reasons to believe that US scientists are any different from other scientists. So I mention below what passed for the portrait of a scientist in 1950 when Anne Roe publicised her findings based on a three-year investigation of anthropologists, botanists, geneticists, palaeontologists, psychologists, chemists, biochemists, physicists and astronomers:

> The scientist has a general need for independence, autonomy and personal mastery of his environment. His attitude toward his work is happy and intent, emotionally he is at the same time stable and sensitive. He has a strong ego, and is not usually impulsive or talkative. He is open-minded about religion and somewhat careless about church-going and skeptical about the details of any one doctrinal orthodoxy. The more violent crimes are practically unknown among scientists. Overall the scientists' personality is remarkably similar to that of creative artists or writers.

And this description of scientists by Sir C. P. Snow:

> Nevertheless it is true that, of the kinds of people I have lived among, the scientists were much the happiest. The reasons for this are not simple. Partly, the nature of scientific activity, its complete success on its own terms, is itself a source of happiness; partly, people who are drawn to scientific activity tend to be happier in temperament than other cleverer men.

Scientists have had their moments in the sun. Their glory is fortunately contemporary and, unlike that of many an artist's, not posthumous. Scientists are also of various kinds as P. B. Medawar has observed. They are variously collectors, classifiers, tidiers-up, detectives, explorers, artists, artisans poets, philosophers, even mystics. Science seems to yield to each his own. Interestingly most scientists could easily have been something else instead and yet been successful. Curiously scientists do not figure as heroes in fiction or novels. Sinclair Lewis' Martin Arrowsmith marks an exception. After the Second World War there has been a crop of comics where, unfortunately, the scientist is wicked and a villain, always with a beard and always talking in heavy Central European accents.

Most scientists are scientists by accident but many are scientists by design. Often they are impelled by role models. Rarely, as in the case of Sir C. V. Raman, they are 'made for the role' the 'naturals'. A. Einstein and N. Bohr and nearer home S. Ramanujan are the other 'naturals'. Scientists like C. V. Raman are *swayambhu*—they make themselves and attain a kind of scientific *siddhi* by themselves. Their first dozen papers are typically 'single author papers'.

Now the mention of single-author papers brings me to the hurdles part of our vocation. I have heard it said often that teachers can also be hurdles. The case of the exploitative teacher who slips in his name in papers he never wrote, is not new. A well-known case of exploitation of students by a superviser is that of L. Agassiz, the eminent nineteenth century naturalist who taught in Harvard. He was accused of dishonestly appropriating the work of his students. Other celebrated cases include Sigmund Freud and Humphrey Davy. Contract research and the Ph D system are said to be behind exploitativeness of students by their guides. It must also not be forgotten that scientific endeavour is now becoming increasingly a team effort. The single-author paper has become a rarity now in science. Organizations which confer awards/honours on scientists now even stipulate that the scientist declare his part or the percentage effort he has put into a multiauthor paper.

But there is another side to the teacher in science. This is the side of the teacher as *guru* and *acharya*. It may not be common knowledge that it was in Germany during the nineteenth century that the doctorate was first established as an award for achievement in science. It is also in Germany that the so-called research schools built around eminent scientists first emerged. It was common for German students until recently to study in several universities before they settled down to a profession. They went from university to university often to be able to learn under an individual Professor of repute. The Professor himself handled the lectures called Vorlesungen—literally meaning reading out and in the sciences, also the practicals with the help of an 'assistant'. I have myself met junior semester biology students in Germany in the early sixties who were leaving Tübingen

University, the next semester for Munich University to be able to hear Karl von Frisch talk about 'dance orientation and language of bees'. Many students of biology had come to Tübingen to attend the lectures of E. Bünning on biological clocks, the protozoologist Max Hartmann or the developmental biologist A. Kühn and the lectures of the abstemious and simple Walter Zimmermann on palaeobotany and the ptelome theory. The fortunate few stayed on for their doctorate work with the Grand Old Men.

During the student's apprenticeship with the master, the student assists his master in many ways: with the preparation of his classroom demonstration materials, charts, atlases, specimens, animal vivisection and so on. The manner of service is reminiscent of our *gurukula parampara*. I commend this method at the risk of being branded an old-fashioned chap. It does take a great personality, though, to inspire the kind of respect in a student that he is willing to live in a spirit of service to his teacher. In return the teacher inspired and taught by precept.

The element of challenge is present at all times in the life of a scientist. The first of them, of course, is the self-doubting which any sensitive person is given to. That is the very first chapter in P. B. Medawars' *Advice to a Young Scientist*. 'How can I tell if I am cut out to be a scientific researcher?' I personally think that every scientist finds an answer to this question sooner or later. But since the very definition of what is expected of a scientific researcher is often vague, many scientists may never have found the answer. One can still hear many middle-aged scientists say that they continued to be scientists since they had not learnt any other honest trade or craft! But that is only light banter. The constant challenge to a scientist is the very high standards he sets himself. To state it in an analogy of mixed genders he wishes to live like Caesar's wife, beyond reproach. In setting himself standards and goals, a scientist transcends national boundaries and even temporal frames. It is often only expected of the scientist that he should be as good as anyone anywhere. Easily said. In fact this is a self-imposed rigour in science. Scientists revel in peer recognition. They would travel to the poles in winter and to the deserts in summer to bask in peer acclaim in international conferences.

Scientific genius, like commonsense, crops without regard for country, language or colour. Which is why I did not mention Indian Science as a separate entity and did not throw in the scientific equivalent of 'be Indian and buy Indian' which is, publish only in Indian journals. It can only be at best scientific jingoism.

Scientists are not known to be clannish, or exclusive except in as much as they would like to be among themselves. Few practitioners of any other profession would be so wholesome in the praise of their colleagues as scientists are. As a Life Science Library book put it: 'In a quest for Fermis and Einsteins, they advertise the delights of their calling widely. They keep no trade secrets and welcome all apprentices. Astonishingly, often in history, one scientist will acclaim the ideas and help in the training of a younger man destined to unseat and overshadow him. The tradition is a venerable one. When Isaac Newton was a 27-year-old graduate student at Cambridge, his chief mentor, Isaac Barrow, then only 39 himself voluntarily resigned in Newton's favour from the Lucasian Chair of mathematics'.

Having thus glorified my profession I must also take time off to lament the decline of the quality of scientific effort in India especially in the universities. The decline has been attributed to the migration of the better scientists from universities to the vastly better equipped National Laboratories established after India became independent. This migration even though foretold by men of vision like Sir C. V. Raman was alas inevitable. There was no other way the National Laboratories could have been manned. Nevertheless this is no reason why university science must continue to languish. German universities interestingly, did not get side-tracked with the creation of the several Max-Planck-Institutes around scientists of great promise. On the contrary, more German university Professors won the Nobel Prize than the Directors of Max-Planck-Institutes. Ironically as an eloquent testimomy to the ephemeral nature of excellence, the inefficient and demoralising bureaucratic set up of the German Democratic Republic succeeded in reducing the famous German Universities such as Jena, Leipzig and Danzig to faint memories of themselves. All this in less than five decades. Centuries of academic excellence and European idealism were reduced to rubbles in just over forty years of mismanagement and neglect. It is depressing to even think of university life in India in this gloomy light. J. V. Narlikar has described the problems of doing science in India in a university set-up in a thought-provoking article. He movingly states how when he returned from Cambridge in 1962 he had wanted to take up a university job and how he was forced instead to accept "the second best" option. The return of Narlikar and Govind Swarup from one of the most prestigious institutes of fundamental research to the university stream warms the cockles of the hearts of scientists like me, who have voluntarily chained ourselves to the blackboard, chalk piece and the science bench in universities. There are few experiences in life which can be as heady as teaching a handful of highly motivated and bright students. Depending on how good the students, the experience can be more satisfying than publishing a short communication in *Nature*. As for science—good science, some persevering university 'teachers' still make do and build first rate laboratories in the sense of H. J. Bhabha ('A first rate laboratory is one in which mediocre scientists can produce outstanding work'). The question of course is, 'for how many years should we persevere?'.

A lot has been written in recent years about 'fraud' in science. Science will also have its occasional black sheep just as any other profession would. When deliberate fraudulence is practised by a scientist it does strike the rest of the world as a blasphemy. In reality, however, very few scientific frauds turn out to be deliberate or wanton. They are in fact so few, that books get written about them. The best-known scientific hoax of this century practised by Paul Kammerer of Austria and brought to book by H. E. Bateson is described in an absorbing manner by Arthur Koestler in his book 'The case of the midwife toad'. Fraudulence is against the very grain and credo of science.

Furthermore, the atmosphere in contemporary research laboratories, most of them funded by private enterprises

does not make for probity in scientists working there Scientific research is no more a mildly pleasurable pursuit. It has become self-centered and competetive. Damn fairplay. I was recently discussing some data with a European scientist friend of mine, once a model of caution. We interpreted the data on hand this way, that way and every which way. He finally said 'Put it down as demonstrating this and not that. Our interpretation is most probably wrong all over anyway'. That reflected the changing times and the slackening rigour. Perhaps it also portended advancing age and the dawning of cynicism.

To compound matters, scientists do not relish writing about themselves or even being written about, an exception seems to be the remarkable Stephen Hawking. Most scientists live in constant dread of being interviewed by media persons and of the ignominy of making it to the pages of popular weeklies. A German scientist—a biologist—who was mentioned once too often in *Der Spiegel* in the sixties had to face peer opprobrium even though his science, by all accounts, was judged as having been good.

One of the eternal dilemmas of a scientist is to know when he has indeed 'arrived'. In science you are not seeded as in Wimbledon. You do not know your number in the hierarchy, your rank in the pecking order. But with the kind of exhilaration good science can generate, even the travel is well worth one's while as the arrival, if there is indeed any. Peer appreciation is the only ultimate reward a scientist awaits. In concluding may I state that a good young scientist must view every hurdle in his way as a challenge. Hurdles are there to be surmounted. After all, kites rise against the wind and not with it.

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M. K. Chandrashekaran is in the School of Biological Sciences, Madurai Kamaraj University, Madurai 625 021, India.

Enrico Fermi and the nuclear age

In November 1954 I had occasion to attend the fall meeting of the American Physical Society held in Chicago. Thousands of physicists had assembled. There were many plenary lectures, invited lectures; there was one by Subrahmanyan Chandrasekhar. I felt that the usual exuberance and boisterousness associated with the young of America were missing. Something seemed to be wrong. When I expressed this to my neighbour he asked, 'In what way' and, I replied the atmosphere suggested an impending doom. He said 'yes it does; do you not know that Fermi is seriously ill and is dying?' (Fermi died two weeks later.)

In Chicago, I visited the Squash Court under the Stagg football stadium where Fermi and his colleagues had assembled the 'atomic pile' and performed one of the most significant experiments which affected science and technology of the twentieth century. I also met and talked to Arthur Compton who was in charge of the project.

It all started in 1934. Fermi's group, at the University of Rome, established that many elements, if bombarded with neutrons, would be transmuted into new radioactive elements. Later that year, they found that neutrons filtered through paraffin were much more effective in producing nuclear reactions than those emerging directly from a radon plus beryllium source. Fermi gave the explanation that neutrons were slowed down by elastic collisions in passing through paraffin and these spent more time in the vicinity of the nucleus and so were more likely to be captured by the nucleus. Fermi and his group also irradiated uranium with neutrons the same year but did not detect nuclear fission because they had covered the sample with an aluminium foil to stop lower energy alpha rays, which uranium spontaneously emitted even without any bombardment.

Bohr at a lecture in Princeton in 1939 leaked out the great news of the discovery of Hahn and Strassmann and the interpretation of Lise Meitner and Otto Frisch that the uranium nucleus split into two equal parts when bombarded by slow neutrons—a phenomenon which they called fission. The same year Fermi discovered in the Pupin laboratory of Columbia University that the number of neutrons released in fission was between 2 and 3, enough to initiate a chain reaction.

The rest is history. A team of scientists was assembled in Chicago. It was Fermi's advice to build the 'pile' in the Squash Court and not in the Argonne Forests. The 'pile' was constructed with 340 tonnes of the purest graphite (moderator), 37 tonnes of uranium oxide, 5 tonnes of pure uranium metal and a few control rods of cadmium (which is voracious in absorbing neutrons). Fermi with his proverbial slide-rule calculated the exact position of the cadmium rod at which chain reaction would occur. The operation began on December 2nd. The pile was operated with a single control rod. There were many safety precautions—the most peculiar was the one rod being held by a rope outside the pile where a team member stood with an axe! At 3.41 p.m. the reactor became self-sustaining (1/2 watt). It was worked for 12 minutes when the safety rod was lowered. The operations were shifted then to Los Alamos; the bomb made and dropped on Hiroshima (on 6th of August 1945), and on Nagasaki—the latter, some say, was just to prove that a plutonium bomb also works.

When Rudolph Peierls (who played a major role in all these and who later was listed as a security risk) came to India, I asked him for a brief story.

He remarked how great men could be utterly wrong. Rutherford in his 1937 British Association speech had said, 'Anyone who looked for a source of