Where is our science Nobel?

Ram K. Varma

This commentary seeks to introspect and analyse the possible infirmities in our scientific ecosystem which could account for our science Nobel Prize desert for over 80 years, despite generous science funding, and suggests some ideas to re-orient the system.

By the time this article is in print, the Nobel Prizes for the year 2013 will have been announced, as in any other year, starting with biology and medicine on 7 October, followed by physics on 8 October. The eagerly awaited identity of the recipients will have been disclosed. Unfortunately, much against our wishes, one would doubt very much whether any of the science prizes will come to India this year, or in the near future for that matter, though I would love to be wrong on this pessimistic projection.

One may be forgiven for projecting such an obstinate pessimism. But one wonders whether there is something fundamentally amiss in our scientific ecosystem that in spite of rather favourable funding position for science over the years, we have not been able to throw up a Nobel-worthy discovery for more than 80 years since our first and the only science Nobel for the Raman effect in 1930. It is not that India-born scientists have not been awarded a Nobel. There are, to be sure, three in post-independent India – S. Chandrasekhar, Hargovind Khorana, and more recently, V. Ramakrishnan – if one discounts Amartya Sen, who won it for economics. But all these have been achieved working abroad. So what is it, that is missing in our ecosystem that Indians working at home have not been able to achieve what those working abroad have. Of course, we are talking about exceptional talent here. But there is no dearth of talent at home.

This note is an attempt to introspect and diagnose some possible causes. I am fully aware of the rather complex nature of this undertaking and of my inherent limitations in this venture, which is fraught with the risk that some of the statements ensuing in the course of this analysis may be strongly contested in some quarters. I venture, nevertheless, to undertake this task, because anything that needs to be said must be expressed, but with the caveat that it needs to be understood in general terms, notable exceptions having been recognized.

The two most important things that determine the quality of science that we do are: (i) attitudes of scientists at the individual level towards the pursuit of science, and (ii) the quality of motivation that the institutions provide to the individual scientist, not only in terms of quality funding, but also in terms of encouraging and supporting heterodox ideas, when proposed. Furthermore, career advancement based on a discriminating assessment of one's achievements plays an important role as a motivating factor. There is, however, also a third factor which has been talked about only in casual conversations, but which needs to be specifically highlighted and discussed at some length sometime later. That is our inability as individual scientists to pool our complementary expertise and resources in the pursuit of some well-thought-out scientific goals. What is it that inhibits us from doing so?

Coming first to the attitudes of scientists towards research, it is reasonable to assume that the attitudes usually form and are moulded in their formative period during education and training – many a time influenced by some of the creative and enterprising teachers. Prompted by such attitudes, some scientists set their own benchmarks of achievement, and are not easily satisfied with their performance. They are perpetually in a no-comfort-zone. Such scientists are quality-conscious individuals who need to be observed to be encouraged in their pursuits. Their visibility in terms of standard measure of performance will generally be low.

However, pitted against such quality-conscious attitudes are those which are governed by quantity. With the passage of time, and owing to demands from the funding agencies for visibility of performance in terms of numbers, these quantity motivated attitudes have been getting a more favourable attention, whereas the quality-conscious attitudes get pressurized into being unreasonably accountable through quantitative production. I recall the case of at least one Nobel laureate Kenneth Wilson who was about to be fired from his job for not being productive enough in terms of numbers, and for 'doing his own thing' which nobody knew or cared to know. Fortunately, the day was saved for him by Hans Bethe's intervention on his behalf. Thus quality needs to be protected from the onslaught of quantity. Quality cannot be quantified, it can only be discerned, and it should be discerned and nurtured.

While quantity is important for the rapid progression of known themes, and it is nobody's case that the quantity too does not carry its own quality. But by 'quality' here one means something which is rather extraordinary in its embrace and has singular achievements as its end-point. Such achievements are not a consequence of any planned approach. They just arise in the course of an exploration. They are the reflection of an attitude towards a sense of personal fulfilment, which is well described by Chandrasekhar[1] in his article 'Science and scientific attitudes', when he makes a distinction between achievements whose goals are set beforehand – like climbing a mountain that exists, and it is to be scaled just because it is there – and the quest of the unknown and the chartering of territories whose existence we may not even be aware of when we started. There are research endeavours of the first kind, and there are those of the second. It ought to be obvious which ones are likely to yield more creative dividends. These are the ones that need to be consciously encouraged and vigorously promoted.

Having discussed what kind of research attitudes are most desirable, we ought to consider next, how are we to educate and train our young minds accordingly. Are we preparing – educating and training – our young minds well enough for the future scientific roles so that they can hope to be creative scientists and bring to the country scientific laurels? What kind of attitudes have we been inculcating in them? Are we training them to be timid and conformist? I am afraid we may have been doing so. We seem to celebrate conformism in our pedagogy, and discourage questioning – I mean tough and bold questioning – where no knowledge is taken for granted without proper scrutiny. We seem to encourage compliance and frown upon audacity. I am afraid we have been equating excellence with quantity at the expense of creativity, when we emphasize merely the high percentage of marks expected of them. We have failed to disengage quality from quantity. Discouragement from bold questioning may lead the young ones to become timid. Timidity stops one from taking risks later in life. One may want to play safe in the choice of problems to be pursued. One may get limited in one's venture to pursuing short-term goals in preference to long-term difficult issues. The impending struggle with the referees and the anxiety for the next promotion hang over the heads of the young scientists like the sword of Damocles. This situation discourages them from experimenting with heterodox ideas and to choose long-term challenging problems.

Experimentation with ideas is important to be able to strike a novel theme. All new ideas may not work. But unless one experiments with them, one would never know which one will work. Advancing the existing ones is not enough. Existing scientific trajectories and streams will not bring a Nobel, unless one either produces 'kinks' in their smooth flow or starts a new sub-stream or a new trajectory. A new successful trajectory, starting HERE, alone can hope for a Nobel.

But how does one get to those creative individuals having heterodox attitudes? Unfortunately, our selection procedure for the admission of research students which has been using a cut-off based on the class obtained, either for the sake of expediency or some other necessity, favours those candidates who, having gone through a rather conformist system of education, have been well polished and rounded into beings, who are essentially conformists, and are driven by conformists' motivations and ambitions. Such a procedure could virtually select out those who could not score so well, not because they were inefficient in grasping the existing knowledge, but because they tried to absorb it, questioning it as they went along. There are any number of examples which corroborate this thesis – the most notable being Einstein himself who was not regarded to be a 'good' student by his teacher – 'good' being equated here to being 'regular' and not asking discomforting questions. He was regarded by his professors at ETH as 'cocky though clever', because he took nothing for granted, not even Newton. Perhaps, it is because of this that he could not get a regular academic position in a university, and had to be content with a clerk's position in the Swiss patent office. But that did not deter him from pursuing his nonconformist ideas.

Big science versus small science

We have developed over the years a lot of fascination for the 'big money–big science' relegating the 'small and the beautiful science' to the margins. Yes, big science too is important. But unless it belongs to really the 'cutting edge' and competes internationally it does not bring much dividends in terms of internationally recognizable achievement. It is 'the small and the beautiful' science which can bring a lot of scientific delight, and if chosen imaginatively, a Nobel too, without the expenditure of big money. Examples abound, starting from our only Nobel, the Raman effect, to my other favourite Nobel winners: the Mössbauer effect, the quantum Hall effect, vacuum tunnelling microscopy, and the latest, the graphene wonder. It is not the big money that was required in each of these, but big imagination and high experimental ingenuity. Each one of these effects is so beautiful.

Mössbauer effect was more of a serendipitous discovery, but its great importance was quickly realized leading to the award of the Nobel within three years. Vacuum tunnelling microscopy, for example, is based on a well-known process, quantum tunnelling, but its experimental realization across vacuum gaps involved careful experimentation carried out over several years, with well-thought-out ideas and application in mind.

The quantum Hall effect was discovered while exploring some very specific issue relating to the 'characterization of silicon field effect transistors' and to see how one can increase the mobility of these devices. Of course, one would never know what surprising thing can pop out of an investigation. But when one deals with such unusual conditions, as a nano-scale film, a two-dimensional system, in a strong magnetic field of ~ 20 T at low temperatures of ~4 K, there is a strong possibility that something interesting may happen. Thus was discovered the quantum Hall effect with its date of birth proudly recorded as being on 2 a.m. midnight of 4–5 February 1980.

Finally, it is highly pertinent to talk about what may be described as the 'graphene wonder' in the context of small science, which involved imagination and innovation, rather than big money, and led to the Nobel. It is interesting to note that the graphene Nobel laureate Geim[2] confesses in his article, 'Graphene prehistory', that he had not much to do with graphene before his 2004 paper in *Science*, but which proved to a watershed leading to the 2010 Nobel Prize within six years. With some imagination, a lowly Scotch tape can achieve what big expensive equipment cannot.

Barring the vacuum tunnelling microscopy, each of these effects was marked by 'surprise' and 'unexpectedness' and each one of them, including the vacuum tunnelling microscopy, led to the opening of a new window. That is what qualifies for a Nobel. The question is, what lacks in our scientific ecosystem which prevents us from generating such unexpected results.

Manner of assessment of scientists

Our manner of assessment of scientists for their career advancement has much to answer for our woes. In committees after committee we are asking for 'how many papers' rather than 'what quality of papers, if few' produced by a scientist. The reason may be that perhaps it is easier to count the number of papers published than to go through them to find out 'what is in them in terms of quality and innovation'. Now the task has been made even simpler through outsourcing the evaluation to the proxy of *h*-index, fortified by the impact factors. The *h*-index is just another measure of 'quantity' as against the quality. What are we then encouraging? Quality or quantity?

It would be presumptuous on my part to believe that I have listed all the factors which, if taken care of, would help us to hope for a Nobel. A Nobel Prize, or any prize for that matter, cannot be planned for. But people do try by choosing potential problems that could lead to a Nobel. For instance, we now know of the intense competition between various groups: the Cambridge group, the Kings College group and the Linus Pauling group, in the chase for the structure of DNA. There have been other cases of groups competing in a 'hot pursuit' in more recent times. Some Nobel Prizes do belong to serendipitous discoveries. But even serendipity requires that we pursue some heterodox ideas and situations.

So where do we go from here? I believe that we need to do a lot of introspection. The following is the summary of my introspection.

1. It seems to me that we need to reinvent the Indian scientific scene, starting with the science education which needs to be revolutionized by banishing the standard mode of education, which only drills into the young minds the current knowledge in a 'closed' manner without exposing them to the spirit of scientific adventure, and inculcating in them the spirit of inquiry. This requires a complete reorientation of our approach to science education and scientific research so as to bring out the inquiring spirit in young students, which gets suppressed in the standard mode of education. They ought to be encouraged to pose unusual questions which should be discussed under the guidance of a competent teacher. A researcher too ought to be encouraged to explore heterodox ideas and points of view, so as to develop a boldness of approach towards any investigation undertaken. Clearly, such transformation cannot occur overnight and needs to be undertaken for the future. High level of creativity is almost synonymous with heterodoxy.
2. Consistent with the above proposition, one ought to devise more creative ways of assessing the performance of scientists, away from the standard mode pursued so far, which is largely based on numbers, and which has further degenerated into a proxy *h*-index evaluation.
3. The importance of small – the beautiful science – ought to be emphasized, which has led to big dividends, including some delightful results leading to Nobel Prizes. However, even small science needs to be watched for innovation, and should be driven by ideas, rather than by expensive equipment.
4. Finally, in the selection of research students, more creative methodologies must be used, rather than using cut-off percentages or cut-off division/rank, which could fail to select the creative individuals.

Let me finally conclude by saying that nurturing of science talent is a demanding and at the same time a sensitive enterprise, where one has to strike a right balance between imparting knowledge emphasizing the rigour required for the veracity and acceptability of scientific results on the one hand and on the other hand, ensuring that the pedagogy remains an 'open' process so as to encourage vigorous questioning of even the existing knowledge.

The same spirit holds for researchers as well. They too require a scientific ambience which encourages free flow of ideas undeterred by unthinking criticisms which tend to inhibit the exploratory spirit. Unfortunately, quick criticism of new ideas, rather than a careful scrutiny is more often than not the case in our ambience as it obtains today. Safer current streams get thereby encouraged.

A rejuvenated science culture is the need of the hour.

1. Chandrasekhar, S., *Nature*, 1990, **344**, 285–286.
2. Geim, A. K., *Phys. Scr. T*, 2012, **146**, 014003.

Ram K. Varma is in the Physical Research Laboratory, Navrangpura, Ahmedabad 380 009, India.
e-mail: ramkvarma@gmail.com