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GUEST EDITORIAL

Some Reflections on the Pursuit and Evaluation of Science

Practising scientists are usually very busy practising their craft and most of us also devote a significant amount of time to evaluate what our peers do. However we seldom find the time or have the inclination to reflect on how we pursue our craft and here by craft I mean, both the craft of doing science as well as the craft of evaluating science.

If at all there is any reflection about the methods of pursuing and evaluating science, it is done almost entirely by a separate group of 'outsiders' who belong to the disciplines we label history, philosophy or sociology of science and more recently, science studies. It is not uncommon for practising scientists to disregard what these 'outsiders' have to say about the pursuit and evaluation of science. Steven Weinberg said famously that the philosophy of science is about as useful to scientists as ornithology is to birds! This is funny, but probably somewhat true and if so, rather sad. The only way to make ornithology useful to birds is for birds also to practice ornithology. Hence, scientists must also themselves reflect on their methods of pursuing and evaluating science.

I believe that this is only possible in any long-term, stable manner if we formalize such reflection and make the teaching of such reflection an integral part of science education, at the undergraduate, postgraduate and especially at the doctoral levels. I have always found it most remarkable that by merely teaching our students how to operate some instruments or solve some equations, we expect them to master the arts of choosing a scientific problem, solving it, communicating their findings to specialist and general audiences and act as peer reviewers for other people's attempts to do the same. A little reflection will show that our science education imparts none of these skills.

I am not a great believer in the demarcation between pure and applied science but in the context of the methods of pursuing and evaluating science, I believe that such a distinction is indeed appropriate. What I say therefore is more valid in the context of science for the generation of knowledge itself rather than science for the generation of wealth from knowledge.

The first step in one's scientific career is choosing a scientific problem for investigation. If generation of significant new knowledge is the goal, it seems reasonable to expect that scientists would look for areas of ignorance, areas that have been overlooked or forgotten by others. We all know, however, that this is not how topics are chosen for study. Indeed exactly the opposite seems to be done. People look for fashionable areas, topics that are of interest to many and themes which are easily accepted for publication in prestigious journals.

Here I wish to emphasize that we in the developing world face a special problem which is largely of our own making. If the scientific community was relatively homogenous with a level playing field, this may not be fatal because we could always argue that the smartest scientists will set in new fashions and bring about what Thomas Kuhn has called scientific revolutions while the rest will continue to do normal science. However we live in the real world compartmentalized into developed and developing countries with associated scientific communities with very uneven playing fields. Left to market forces it is inevitable that a disproportionate number of revolutions will originate in the better endowed scientific communities in the developed countries while those in developing countries will be almost permanently relegated to doing 'normal science'.

However I am convinced that there are significant opportunities for the simultaneous development of uniquely different perspectives from different parts of the world especially in biology. But our own institutionalized scientific structures ensure that any prospect of development of a new and different perspective from our parts of the world is nipped in the bud. We reward scientists who work in fashionable areas, we reward those who publish in western, prestigious journals, we have no time and patience to read their work and judge for ourselves, we reward those of our scientists who are applauded by the West, we have no self-confidence to make our own independent judgements of the accomplishments of our scientists. In short, we create, nurture and reward followers rather than innovators. I am not surprised that this suits the developed world but I am surprised that it seems to suit the developing world as well! The net result of all this is that science loses prestige as a career and our bright young people turn to other professions.

The next step in the cycle of the scientific enterprise involves obtaining money. Here our institutionalization and bureaucratization have reached their zenith, or nadir, depending on your point of view. Grant proposals are amazingly bureaucratic documents which defy the very fundamentals of the method of science. Typically they require one to specify every conceivable detail of everything one plans to do with a precision that would be the envy of army generals. We are expected to specify the complete details of every experiment we plan to do every month or quarter and also know the outcome of every investigation and its possible use and also exactly how much every sub-item will cost in terms of money and manpower. I can say without fear of exaggeration that whenever we could honestly and accurately fill out the forms supplied by funding agencies, that research would not be worth doing in the first place. And we scientists have never had the courage to tell the bureaucrats that we cannot be treated in the same way as the income tax or excise departments. So we go ahead and fill the forms with half truths, making the filled forms even more ludicrous than the blank forms.

And I must emphasize that here there is nothing special about developing countries. Although I have not received grants from any funding agency outside India, I have been a reviewer for many grant proposals from many parts of the world. I can say from my experience that the bureaucracy and the absurdity of grant applications are even more in developed countries! How can we change this situation? I believe that even merely reflecting on it will help. And if we are required to teach students a formal course on how to obtain grants, the embarrassment we will have to go through will guarantee that things will definitely change for the better.

I am going to gloss over this quintessential step of the scientific enterprise and my justification for doing so is as follows. A great deal can be said about sloppy science or even about fraud but mercifully these are the exceptions rather than the rule; what I am focussing on in the other steps of the scientific enterprise are the rule rather than the exception, and hence they deserve my attention more.

One of my all-time heroes, Sir Peter Medawar gave a brilliant talk on BBC in September 1963 with the title: 'Is the scientific paper a fraud?'. I am afraid his answer was in the affirmative. Medawar dubbed the scientific paper a fraud not because it misrepresents facts but because 'it misrepresents the processes of thought that accompanied or gave rise to the work that is described in the paper'. This is because of the insistence of scientific journals on a strict demarcation between and order of appearance of 'Introduction', 'Methods', 'Results' and 'Discussion', which gives the mistaken impression that all scientific discoveries are 'inductive' processes. Medawar was right in 1963 and continues to be right in 2006 – almost nothing has changed.

To make matters worse, new fashions have taken root since Medawar's time. Today the most important criterion for evaluating the quality of a piece of scientific work appears to be, where it is published. *Nature* is rank 1, *Science* is rank 2, *PNAS* is rank 3, and so on. Many people, especially those who evaluate scientists and scientific institutions appear to have stopped reading scientific papers; they are guided by the impact factors of the journals in which the papers are published and by the number of citations they receive. This of course has led to a mad rush to publish in the most prestigious journals and the desire to undertake research that is likely to be accepted for publication in the most fashionable journals. The extent to which scientists have surrendered their right to evaluate science to the publishers of a few journals is appalling.

Creative intellectual activity is a complicated business. It is necessary to be both 'correct' and 'creative'. By means of the peer review system we have created strong forces that prevent one from being original or creative and rightly so, because what is original and creative can often be wrong. The publication and acceptance of almost anything is based on peer review and acceptance. This has the function of ensuring that too many falsehoods are not perpetuated in the name of science. But at the same time, this often curbs necessary departures from widely accepted positions. Fortunately we sometimes find individuals who rebel against the peer-review system and it is these individuals who make the transition between 'normal science' and 'scientific revolution'.

My favourite example is that of Amotz Zahavi of the Tel-Aviv University in Israel and his handicap principle. Biologists since Darwin have wondered why the peacock has such an elaborate tail that must surely be a handicap to him while running away from predators. Zahavi made the radical suggestion that the peacock's long tail is selected precisely because it is a handicap, not in spite of being a handicap. By carrying around such a handicap of a tail and by not yet having succumbed to a predator, the peacock reliably demonstrates to peahens that he is indeed fit enough to survive despite the handicap. Zahavi derived from this idea a far-reaching general principle called 'honest signalling' and attempted to explain almost everything in animal and human behaviour with this principle.

The enterprise of attempting to explain everything with the handicap principle will surely fail at some point but we will never know exactly where it will fail unless someone pushes it past the precipice and, very likely, falls along with it. I think we should be grateful to Zahavi for altruistically doing this for us. But how do we allow space for the Zahavis in the framework of the peer review system? Personally I would like to see the scientific community become more tolerant of such radical scientists. But of course if everybody is allowed to be a radical, there will surely be chaos. What we need are impeccably competent radicals. We should set our thresholds very high and demand the highest possible level of competence before we become tolerant of radical scientists pursuing their radical positions. For the rest of us there is always the harsh peer review system!

By and large, but of course with some famous exceptions, scientists do not write for a lay audience. Writing for a lay audience has very little social prestige among scientists. Students who sometimes indulge in this enterprise are told that they are wasting their time and professors who do so are told that they have run out of ideas for doing science. This is at least one important reason for the widespread public misunderstanding of science and also for the very narrow specialization of scientists.

I have quite deliberately focussed exclusively on some of the many things we do rather badly. But I have great optimism that if only we generate a culture of reflecting on the methods we use in the pursuit and evaluation of science, we can overcome many of these shortcomings. I am even more optimistic that by making it mandatory to formally teach the pursuit and evaluation of science in all science courses, we can get onto this job on a fast track.

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