A brahminical culture in science

Recently I had an occasion to sit in a panel answering questions from young students of science in schools and colleges. It was part of a workshop organized to commemorate the 150th birth anniversary of Acharya Jagadish Chandra Bose. Seated in the audience were these students – mainly for whom the workshop was organized – and a few interested people of all ages. There was no fixed topic set as such, and, anybody could ask anything. The first question coming from a young student of undergraduate science set the trend. It was: which one is presently the most important thing for India – to win a Nobel prize or fight poverty?

None of us was prepared for the onslaught, though the boy certainly did not mean it. A pin-drop silence reigned for a split second, and then, several voices spoke from our side, one at a time. What we then said, in fact, amounted to nothing, trying only to skirt around the question. At the end of it, neither the student nor the audience was any wiser.

Truly speaking, there is no one-word answer to this query. Nobody would welcome or stand regimentation in Indian science. None of us would like it said from a pedestal: do this, and, do not do that. It has to come from deep within. External agencies may at the most help it to the fore.

The question posed by the boy, however, had a deep effect. It set us athinking. Why the question? What made the boy raise the question? Was it naivety? Or, did he catapult his feeling of what we, the Indians generally do in science? Fighting poverty, of course, is a different proposition, and science is not necessarily the paramount factor there. But, the question certainly rang out a warning for us. Do we, the Indian scientists, honestly care for our national causes? Are we trying hard to address them?

It takes me back to the early 1980s when in India the phrase 'surface physics' was not in vogue. One of our theorist-friends came back from Germany. He took up a problem in chemisorption – the study of how floating atoms or molecules or radicals bind to material surface. What was the relevance? As he explained, it helps us fight pollution hazards. It enables us to reduce the toxic content of emissions (carbon monoxide from motor cars, for example) by a clever conversion to substances of lesser harm. Given that, the proposed study was eminently relevant to the hazards of modern times. The study was interesting also, from the viewpoint of both the basic beauty of its logistics and its immediate applications. The success of my friend's endeavour notwithstanding, it was how I saw surface physics making an inroad into Indian science. And I believe, most of the people saw it the same way. It was a branch of science opening new and consolidating old routes to a noble end.

With time, however, the ambit of surface physics widened, as it should. And with that another thing happened. The subject started edging away from its primary objective. Multitude of projects of varying importance were proposed and passed – the required justifications being not too hard to find. Once the funding authorities got satisfied, these projects found hosts at several Indian universities and research institutions. New departments were created and fresh manpower engaged. Costly instruments were bought from abroad. Crores of rupees of the exchequer were spent, and, in return, hordes of research papers were written. Some of them won places in prestigious international journals and advanced several careers as a result. But that was, and still is, the end of it – very little of it was ever noticed by the industry, in India or abroad.

I have nothing against surface physics. It is picked up merely to illustrate how with time the avowed objectives of basic sciences gradually get diffused, if not lost, in our country. High temperature superconductivity or nanoscience could equally be a glaring example in this class, and I am sure, there are many others.

Does it, then, mean all we did was a complete waste of time and effort? There is a large section of scientists ready to jump to an emphatic no! They have a sound argument that every bit of the work enriches our knowledge. We may not have any problem with that, though, given the maze of the generated theories and data, one is not sure how long it will take for this knowledge to prove useful. But it is easily seen that the strength of their argument gives way to a kind of weakness too. When a country, or, so to speak, the whole world is riddled with a particular problem, namely environmental pollution, and is in express necessity of fighting it, it is this strength that provides an excuse for not addressing these. It also gives us a license to pursue, instead, paths of pure knowledge – the kind of knowledge that may not cater to our immediate needs.

If we agree to identify this as a weakness, an obvious remedy is to prioritize. For example, we may choose to decide that in the next five years or so we shall completely tackle the pollution problem. Such a priority may not have to block paths to pure knowledge altogether – there can always be room for it. But it shall not become all of basic sciences. In case we ourselves cannot set such a priority, we must be ready to abide by the authorities who will do that for us. Regimentation is one thing and prioritization another.

In a bid to do a thing of the latter kind, the funding authorities at times identify thrust areas depending on urgency and the contemporaneous nature of a certain class of problems, but then, there is a snag, and that makes the whole exercise futile. Regular assessment of what is produced against what is aimed at is hardly ever done. Accountability being nobody's concern, the objective of a project more often than not gets diluted or betrayed in the end.

Lack of priority and accountability over the years has the effect of promoting among us a culture which is brahminical at best. Under its spell, one sets aside priorities even when they are laid and is at peace by pursuing academic interests only. Barring chemistry and biochemistry, where we have fared better in relating to the chemical and biotechnology industries, the effect is most acute in physics. Sometimes, it permeates beyond the realm of basic sciences. In IITs, most of the research-active professors of mechanical and electrical engineering are now content with doing theoretical research having little or no bearing on our industries. The one-time trend sighted among our young researchers of preferring theory to experiment (particularly in physics, which, happily, is receding now) is another fallout of this brahminical culture. Perhaps, it is all rooted in the legacy left to us by our forefathers. At a time when people in the

West were devising artefacts to utilize the heat from an earthen oven most effectively, India remained mostly engaged in thinking of the abstracts. It has become our second nature to amass pure knowledge and leave it to the 'lesser talents' to consider its practical utility, if at all. The trouble is, in our country such talents are not too many, nor are there sound policies to rear them in the arena of basic sciences. This is very sad, especially when it is proved that motivated, we deliver well. In recent times, the successful design and production of the MANAS chips and the photo multiplicity detectors are worthy of mention. They are being used in the large hadron collider experiments at CERN. Unfortunately, that is more an exception than a rule.

For once, we shall do well to bear the boy's question in mind, not in the form it was put, but in the spirit it was set.

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Craig Venter, and the claim for 'synthetic life'

On 20 May 2010, *Science* announced the results of the work of Craig Venter and his group on the 'Creation of a bacterial cell controlled by a chemically synthesized genome'[1]. This publication has received extensive media coverage as the first example of artificial creation of life, of man playing God, and so on.

What exactly has been achieved by Venter, and how important is it? Here is one assessment, in a brief Q&A format.

How momentous is Venter's achievement?
Venter's work represents a tremendous technological feat, requiring as it did success in three difficult and sequential steps: the chemical synthesis and assembly of a DNA molecule of length 1.1 million base-pairs; its cloning into a yeast cell as a yeast artificial chromosome (YAC); and finally and perhaps most difficult, the introduction of the YAC by a process which the authors name as 'genome transplantation' into a suitable recipient bacterial cell where the genetic instructions encoded in the transplanted DNA could be decoded and rendered functional. For convenience, these steps may be referred to as 'A', 'B' and 'C' respectively.

Although it is indeed a stupendous technological achievement, it could also be argued that conceptually it was somewhat ho-hum or routine. Furthermore, the three steps 'A', 'B' and 'C' have been individually successfully demonstrated by Venter himself in earlier path-breaking papers that were published in *Science* in 2007 (C)[2], 2008 (A + B)[3], and 2009 (B + C)[4]; hence, in patent office terminology, the combination of 'A + B + C' being reported now would be considered 'obvious' or non-inventive, since the whole in this case has not been greater than the sum of its parts.

From a conceptual point of view as well, most biologists would agree that our accumulated knowledge and wisdom of genetics and molecular biology in the last 50 years would have predicted or foreseen the present results that were obtained by Venter, once the technological hurdles were overcome (as they have in the last three years). Hence, there is certainly no 'Eureka' moment here. One should also keep in mind that the synthetic genome used in this work was virtually identical in its sequence to that of a natural bacterium (that is, with an almost certain likelihood of it being functional), with very few 'cosmetic' modifications.

And the claim that a 'synthetic cell' (or 'synthetic life') has been created?
There is an issue of semantics here. What has certainly been achieved, and is rightly mentioned in the title of Venter's paper, is a 'chemically synthesized genome'. Now, this synthetic genome was introduced into a pre-existing living bacterial cell, where the former hijacked the host's machinery (including its proteins, ribosomes and membranes) to decode its own information and thereby substitute the host machinery in its entirety, by what one may term as the process of 'infinite dilution'. Some may argue that creation of an authentic 'synthetic cell' would require the artificial synthesis of proteins, ribosomes and membranes as well without making use of the pre-existing living bacterial cell, which has not been achieved here. At the same time, Venter is correct in claiming that once the hijacking had been completed, there was no trace left of the original host and hence the resulting living entity can indeed be referred to as a 'synthetic cell'. The analogy could be to that of the construction of an arch as a structure in stone or concrete, which cannot be done without a scaffold but then becomes a free-standing entity once its keystone is in place.

To give Venter his due, the new organism could also be hailed as the first living entity in this world without an ancestor (if one assumes that the host cell that was used for genome transplantation was not ancestral, since its genome is no longer represented in the new organism; and, further, if one does not subscribe to traditional Christian beliefs!).

Any other comments?
Just a note of caution that Venter's is a private enterprise, and that there is certainly likely to be an element of profit-seeking in the publicity that he has been trying to generate with this work.

1. Gibson, D. G. *et al.*, *Science*, 2010; doi: 10.1126/science.1190719.
2. Lartigue, C. *et al.*, *Science*, 2007, **317**, 632–638.
3. Gibson, D. G. *et al.*, *Science*, 2008, **319**, 1215–1220.
4. Lartigue, C. *et al.*, *Science*, 2009, **325**, 1693–1696.

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