The sociology of biology as a profession

I felt motivated to write this letter after reading the editorial 'Biology in India: Through the Looking Glass' by Balaram[1] and opinion 'Indian biological sciences: aiming even higher' by Ramaswami[2]. Let me clarify at once that while these two articles address *Indian* biology, my own letter is *not* about Indian biology per se; rather, it consists of my impressions and opinions of the manner in which biology as a discipline is practised.

The reader may well ask what credentials a person like me has to comment about biology. Back in 2002, I began to put together a computational biology group within my R&D centre in TCS – a group that now consists of more than 40 persons. Working with this group has given me a lot of insight into how biologists think, and also into how the biology community works. I can summarize by saying that while I know very little biology, I know a great many biologists!

I was particularly struck by the passage in Ramaswami's article where he says: '… we found that none of us would recommend a career in academic science to our school-going children'. That comment hit home because it *precisely covered* my own situation. My father was a Ph D in mathematics, I am a Ph D in electrical engineering, though my research interests are most accurately described as applied mathematics, and my only child is more than half way through her Ph D in the life sciences. I thoroughly concur with the sentiment expressed by Ramaswami: Though my father never overtly encouraged me to get a Ph D and become an academic, he was mighty pleased and proud when I did so. In contrast, had my daughter expressed any interest in an academic career, I would have done my best to dissuade her. On the other hand, if she had inherited my father's and my inclination to pursue a Ph D in engineering or computer science, I would have most certainly encouraged her to pursue an academic career. So I was quite happy to discover that an eminent biologist such as Ramaswami shared my misgivings about the manner in which biology as a profession is being conducted, especially in academia.

Why would I discourage my daughter from pursuing an academic career in biology? The biggest problem I see with biology research is the over-emphasis on 'big science' to the possible detriment of 'good science'. As a result, many biology 'research groups' now resemble a modern and profitable corporation more than anything else. The modern biology research supervisor is similar to the CEO of the corporation, who has an army of minions working for him. As the CEO, he will have only a vague idea of what any one of his employees is specifically doing, and his main concern is the perpetuation of the enterprise. Except at the very top, thinking and challenging are discouraged, and unthinking drones who are able to follow orders unquestioningly are most in demand.

If Ramaswami is right that most persons cannot aspire to an academic position in biology until they have put in ten years or so of apprenticeship in the form of postdoctoral work (often at subsistence-level stipends), then this should be a cause for deep introspection. The 'big lie' in biology is the pretense that all those wasted years as a postdoc are somehow preparing one to be a better researcher if and when he finally gets his shot at it. This is why the postdoctoral period is described as an apprenticeship, instead of the servitude that it really is. Given the massive over-supply of Ph Ds and the shrinking opportunities (especially in USA, but the Obama 'stimulus' may revive NIH), the sad truth is that most postdocs bounce from one assignment to another, just chasing the money, even though successive assignments may have virtually nothing in common.

The problem is not peculiar to biology, of course. My impression is that chemistry also suffers from the 'professor as CEO', syndrome. Even in physics there are a few exemplars of 'big science', such as CERN and the Large Hadron Collider. However, in physics such examples are an exception and not the rule, whereas in biology big science appears to be the rule and not the exception.

The drive towards 'big science' has overturned the classical paradigm. Historically, the Ph D was a research degree, followed by a postdoc that encouraged largely independent pursuit of a carefully considered research problem. With this background, a newly-minted Assistant Professor then sought grant funds as a means to support her research. But that progression has now been corrupted. Typically, acquisition of grant funds has become the primary objective, with promotion and tenure dependent upon the size of one's NIH 'portfolio'. In the US, most university presidents are CEOs, not scholars, and a clear fiscal imperative has been propagated from top-most level of the administration to the academic enterprise as a whole, filtering down to postdocs and graduate students. The currency of scientific research is – and always has been – new knowledge. To lose sight of this fact is to court insolvency.

It is not as though there are no 'operators' amongst engineering academics. If they try to present a technical paper, usually they can be unmasked via a seemingly innocent question such as 'Can you explain to me how precisely Equation (18) follows from Equation (17)?' So such operators usually stick to 'high-level overviews', which is often by itself telling commentary on the level of their involvement in the work.

If any scientific discipline is to build a proper pipeline of researchers, it is imperative for persons to get a taste of original research as early as possible, instead of learning to obey instructions faithfully. In my own case, for my Master's thesis my supervisor asked me to look at a particular problem. About a month after I started working on it, a short note appeared written by two fairly competent control theorists, stating their belief that the problem did not have a solution. Well, they were wrong – the problem did have a solution, and I found it after a few months of work. This experience was important for two reasons. First, I got my initial taste of the heady feeling one has when at a particular point in time, he is the only one in the whole wide world who knows the answer to a particular question. That heady feeling is what keeps many researchers going, and it is important for young persons to be exposed to it as early as possible. The second and even more important lesson for me was not to accept the printed word at face value, even when it is written by so called 'experts'. In science everyone is equal!

Of course, my Master's thesis was not exactly earth-shattering – the problem I solved was not that important! But it *was* original and publishable, and that is the key point. Nowadays, in certain branches of mathematics (for example, number theory and algebraic geometry), it is considered necessary to inflict 3–4 years of graduate courses on a student before he is allowed to start research, just to ensure that the research is 'deep'. I think this is a mistake, and especially so in biology. Understandably, in some disciplines like physics, it may take years to acquire the pre-requisites needed to understand important problems. However, in biology many cutting-edge research problems can be appreciated readily by a college freshman.

Assuming Balaram is right that there are 1100 postdoctoral fellows at the University of California at San Francisco alone, this is a truly astonishing statistic. It appears that postdoctoral fellowships in biology are akin to MBA programmes at 'famous' universities. In many – perhaps most – cases, one does not actually learn anything useful; rather, the 'brand name' of having passed through hallowed portals and the 'networking' from rubbing shoulders with present and future bigwigs constitute the actual rewards. And the system just keeps perpetuating itself, just like the MBA programmes. (As an aside, the USA produces 150,000 MBAs per year, and a mere 65,000 engineers with Bachelor's degrees. No wonder that 'American manufacturing' is now an oxymoron!)

Because manpower is so cheap, I fear that biological research has not made any real attempt to modernize itself. One of India's most eminent biologists (whom I will not name) told me with a straight face that biology today is where physics was before people knew that $F = ma$. I would not like to go that far, but I will say that biological research reminds me of aeronautical engineering in the days before supercomputing made wind tunnels obsolete. Many biologists I know still prefer to keep on using their versions of wind tunnels instead of working out reliable reductionist models of their subject.

The university's fiscal imperative spills over into the biotech industry, where many academics have a vested interest (and a second job!), and claims to the contrary notwithstanding, the profit motive often inhibits good science. For example, in matters of scientific openness, I find a distinct contrast between biology and engineering. In engineering, if we find a novel solution to a problem, our first impulse is to tell everyone about it. There are places such as arxiv.org, where one can deposit even a working draft of one's paper, so that issues of priority can be resolved. After that, the details of one's discovery are open to one and all! The flip side of this is that secret 'proprietary' algorithms are anathema to the engineering community. The only way for the community at large to accept that algorithm A is better than algorithm B is for both algorithms to be freely available and open. Patents and/or documented prior publication (even non-reviewed, as in arxiv) protect the inventors of the idea. In contrast, practically every biotechnology company stakes its claim to fame though secret 'proprietary' solutions. Personally I find this kind of 'witchcraft' approach to science to be most disconcerting.

From the standpoint of intellectual honesty, I find the involvement of academics in biotechnology companies to be a cause for concern. It is by now accepted that most technological breakthroughs are made either in academic laboratories or in small start-ups. Until about a decade ago, people who founded start-ups stayed with them until they turned profitable. Then the IPO (Initial Public Offering) became the exit point for the promoters. To have a successful IPO, it is not always necessary for the start-up to have a profit; it is sufficient for the company to have some revenue. Hence the emphasis shifted to short-term 'boosting' of the finances of the start-up, often at the expense of its long-term viability. With the IPO market now in doldrums, 'takeovers' have become the preferred exit. I like to call this the 'bigger fool' model of investing – you don't mind buying a lemon of a company, provided only that you can unload it on a still bigger fool. To arrange for a takeover, even revenues are no longer a prerequisite for the start-up – it is enough for the company to have 'good prospects'. This has spawned a cottage industry whereby the start-up assembles a star-studded 'Scientific Advisory Board', the members of which will tell anyone who asks that the company has great prospects and will revolutionalize the industrial segment in which it operates. All this is done in return for a handsome fee, of course. We ought to call it by its proper name: prostitution. This kind of nonsense goes on because of the long gestation periods for biotechnology industry, and because no one has the patience to hang around until the very end. In engineering the success or failure of an idea is often known within months, and certainly within years. So no 'famous professor' can get away with consistently peddling lemons. But somehow this kind of reckoning does not seem to take place in biotechnology.

I do not pretend to have any solutions to the long list of problems that I have mentioned. If I have provoked some persons to actually do something, then I believe my purpose will have been served.

1. Balaram, P., *Curr. Sci.*, 2009, **96**, 625–626.
2. Ramaswami, M., *Curr. Sci.*, 2009, **96**, 639–640.

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