Diversity of the science ecosystem

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The book *Frontiers of Illusion* by Daniel Sarewitz, an earth-scientist-turned-science-policy-advisor in the US Congress, opens with these lines: 'The age of physics came to an end on 21 October 1993 when the US Congress canceled funding for the Superconducting Super Collider...'. This is a remarkably grandiose claim. Could a subject with such deep roots be brought to an end by a parliamentary decision? The book was written in 1996, and today it is clear that the US Congress decision had a rather different impact: it shifted the focus of high-energy physics outside the US. As a result, the Higgs particle was discovered at a laboratory based in Europe rather than America.

The tendency to make statements that are not properly qualified is regrettably common, among not just policy advisors and journalists but leading scientists as well. The following quote is from the blurb for the book, *Letters to a Young Scientist* by the biologist E. O. Wilson: 'Wilson insists that success in the sciences does not depend on mathematical skill, but rather a passion for finding a problem and solving it'. If 'the sciences' is intended to mean 'the areas of science in which Wilson has worked', then the statement might perhaps make some sense. But taken at face value it is incorrect: it is quite impossible to carry out research in mathematics, or areas of physics like the general theory of relativity, without considerable mathematical skill. Perhaps Wilson did not have these subjects in mind – his own research has been in fields like biodiversity, conservation and the study of ants. If so, the blurb should have avoided using a sweeping phrase like 'the sciences'.

This exemplifies a general problem: scientists tend to extrapolate from their own areas without trying to understand or appreciate the differing realities across the incredibly diverse ecosystem of science. Even if unintended, this approach can be jarring as in the case recounted above. That can have the effect of hardening prejudices and polarizing scientists against each other. But a shared appreciation of concepts and techniques across areas of science is important. This is how cross-disciplinary research is fertilized, and it is also necessary for good science administration. The allocation of resources, recruitment of faculty, designing of degree programmes and selection of achievers for fellowships and awards, all require informed comparisons, however imperfect, on the value of different kinds of research. If scientists in a committee lack a minimum body of shared knowledge, it is likely that extraneous – and undesirable – factors will come into play.

So we cannot afford to neglect the problem, and I would like to examine it here in a little more detail (note 1). It is interesting to observe how polarized views about science are frankly expressed by young students. On a blog website, teenagers recently engaged in the following debate. 'State your favourite(s) between the three sciences' asked the originator of the discussion. 'Explain why you like them. If you hate all three, that's fine too'. One writer said, 'I like chemistry the best because it is the basic understanding of what makes up our universe'. Another voted for biology: 'For organisms to achieve the cell/genetic complexity they have today is pretty cool'. A third one argued for 'physics, because it is the essence of the universe's being'. Each science also came in for its share of negativity. One student proposed that biology is 'most boring. Too much memorization/facts'. Another observed that 'chemistry is hard and pointless and horrible', then added without a trace of irony: 'but I have a bad teacher!' A third student argued that 'physics has nasty ... things that can't be proven but have to be taken as true axiomatically, like Newton's Laws'.

Most of us have experienced emotions along these lines at some time. But while they are understandable in students, when the same emotions are expressed by adult scientists, it becomes a cause for worry. Can we allow ourselves to carry such simplistic attitudes into our careers and rely on them instead of logic and reason when called upon to make important decisions?

Comparisons involving sub-disciplines, such as various branches of physics, can lead to even sharper and more polarized debates despite (or perhaps because of) the closer connections. As beginning graduate students, we got worked up about the merits of condensed-matter versus high-energy physics, and experiment versus theory. Our debates started with solidified – and ignorant – assumptions about the categories being fought over; so we felt quite sure we understood what 'experiment' and 'theory' meant, and what 'condensed matter' and 'high energy' referred to. In fact, recent developments have challenged these distinctions. Quantum information and emulation are areas where the traditional 'theory-confronts-experiment-confronts-theory' paradigm does not apply. Here, experiments almost become a type of theory! Similarly, the differences between the so-called emergent (condensed matter) and fundamental (high-energy) points of view are eroding: the study of strongly correlated systems has become linked to black hole physics via the holographic AdS/CFT correspondence. Not surprisingly, our binary points of view were misleading and in the end rather meaningless.

Approaches to publication and authorship are very different across disciplines. In many areas the order of authors in a publication is important. Also, different authors may have contributed in qualitatively different ways: the person who supplies crucial materials such as a single crystal or a genetically engineered species may become a co-author. As a theorist, this was surprising to me at first, but explanations by workers in these fields have brought out the reasons quite convincingly.

In other areas of science, including mathematics and high-energy physics, alphabetical ordering of authors is the norm. In these areas it is rare to indicate explicitly, or even implicitly, the extent and nature of the contributions by individual authors. An extreme example is the author list on publications arising from the CMS and Atlas detectors at the Large Hadron Collider. Each has upwards of 3000 authors, who are listed as follows: alphabetically by country, then alphabetically by institution, then alphabetically by author. This practice tends to surprise outsiders and has led to the (unjustified) fear that in this area it is impossible to assess the merit of job candidates! Again, explanations by workers in the field can help dispel the confusion (note 2).

Refereeing practices also vary widely. In some areas multiple referees are selected by the editor, while in others a single referee is the norm. Depending on the field, the referee's role may be to address the intricacies of the work, or merely decide whether the paper makes reasonable sense. A referee report may be produced in a day or a year. Some journals accept only a small fraction of the papers they receive. But journals that publish a larger fraction may have a higher impact factor. So a high rejection rate does not automatically attest to the worth of a journal in all fields.

It may be mentioned here that just over two decades ago, scientists started to post their research output on the internet in unrefereed archives. Most of these papers were later submitted to journals. But around the same time, refereed journals themselves started to become 'electronic' (i.e. internet-based). Unfortunately, poor communication among scientists led to a confusion between archived articles (not refereed) and articles published in electronic journals (refereed), leading to unfair judgements about the latter.

The extent to which the issues highlighted above affect scientific committee work in India is quite variable. Committees function best when members show constructive interest in areas other than their own and exchange scientific information. Then good candidates are appreciated by all the members and decisions tend to be unanimous. The worst committees, and sadly they do exist, are those where each member enters the room determined to influence the selection procedure in favour of his/her stream of research, and shows no interest in anyone working in a different area. The results from such committees depend largely on the relative clout of the different members, independent of the merits of the candidates.

The sociological problem within science that has been described in this note may not be possible to correct in a short time, but it is hoped that bringing it out from our subconscious – where it normally dwells – can be of some help. Several partial remedies do exist. The best scientific institutions have a colloquium series that spans many areas of science. This is a relatively easy and pleasant way not just to learn what is happening in other areas, but also to appreciate the diversity of socio-academic practices. In committee work, it can be helpful to formulate a statement emphasizing the joint responsibility of members and the importance of focusing on quality over and above subject divisions. Of course, nothing works quite as well as scientists talking and listening to each other informally with an open, inquisitive mind. This is exactly what we are trained to do, after all.

Notes

1. A disclaimer: the examples in this note will necessarily be based on my own experiences as a theoretical particle physicist, but I have tried to ensure (rather than blindly assumed!) that the observations have wider applicability.
2. In this case, a splendid job has been done by Viyogi[1].

1. Viyogi, Y. P., *Curr. Sci.*, 2010, **99**, 890–892.

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