RESPONSE TO REVIEW OF *A TALE OF SEVEN SCIENTISTS*

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I would like to thank Helge Kragh for taking the trouble to review my recent book (1).

After a few flattering remarks about my previous work in the history and philosophy of chemistry, Kragh immediately goes on the offensive and objects to the fact that my book has two prefaces and a biographical section. Unlike Kragh, I write for a general audience of chemistry educators and the general public, in addition to experts in the history and philosophy of science. I therefore asked a leading chemist, Peter Atkins, to say a few words on behalf of the book. Since Atkins restricted his comments almost entirely to the chemical aspects of the book, I also asked an expert on the philosophy of science to write a few introductory words to address the philosophical issues.

My project of proposing a new philosophy of science is admittedly rather ambitious. Perhaps I could have waited for a further five or so years in order to dot all the i's and cross all the t's, but I chose to go into print and see what others thought about the new direction that I have launched into. Of course, I welcome corrections and suggestions as well as the opportunity to elaborate a little on the ideas that I proposed in the book.

I also chose to include a biographical sketch on my own intellectual journey. Many readers as well as reviewers have told me that they very much appreciated hearing about how I arrived at my current views but obviously one cannot please everybody.

I regard myself as one of the 'little people" in the philosophy of science in general, even if I have succeeded to make a small mark in the far more restricted field of the philosophy of chemistry, as the reviewer so generously acknowledges. My own views on general philosophy of science are like a drop in the vast ocean of diverse opinions in this far larger arena. I believe I am part of the organic web of authors and researchers that I espouse in my book. I think it is therefore quite appropriate for me to forge ahead in the hope that others, might subsequently comment.

Kragh begins by writing,

> One motivation for Scerri's project is, somewhat strangely, his dissatisfaction with standard histories of quantum mechanics which he suggests overrate the contributions of German-speaking physicists and underrate those belonging to the English-speaking world. He does not elaborate and perhaps wisely so. Whether one likes it or not, with the exception of Paul Dirac the emergence of quantum mechanics was almost entirely due to physicists from Germany and Austria.

I find it rather odd that Kragh should feel the need to imply to readers that I did not elaborate. In fact, I explained my statement a good deal further. Here is what I actually wrote on the question of quantum mechanics and German-speaking physicists.

> As the history of quantum mechanics is usually presented, it appears as a mainly German affair. Of

> course, if we think of Schrödinger and Pauli then national allegiances must be widened a little to encompass Austria, Switzerland—and Denmark in the case of Bohr. Certainly, the Frenchman Louis De Broglie is given due coverage as is the Englishman Paul Dirac (who was of partly French origin). Nevertheless, quantum mechanics is generally regarded as a Germanic affair in the wider sense. As I see it there was a great deal of influential work being carried out in the English-speaking world during this period but this is only evident if one drops the emphasis on the heroic approach to the history of science.

I explain why I focus on the seven particular scientists that I do a few pages later.

> … in terms of nationalities they consisted of four from Britain and one each from Germany (Abegg), France (Janet) and The Netherlands (van den Broek). Needless to say, the predominance of authors who wrote mainly in English reflects my own linguistic limitations and perhaps an unfair bias toward the Anglophone world.[18]

What I am saying is that I am concentrating mainly on authors who wrote in English or French for the simple reason that I cannot read German! I am not attempting to claim that "the Brits were somehow better than the Germans."

Kragh bemoans what he sees as my wanting to deglamorize famous scientists when he writes,

> The reason why Scerri focuses on these marginal figures is that they illustrate one of his main theses, that the contributions of the lesser, even obscure figures are no less important to the overall progress of science than those of the famous scientists. This thesis he takes quite seriously, even denying that there are any "outstanding personalities" in science. According to this view there is no reason to celebrate scientists such as Newton, Lavoisier, Maxwell, Darwin and Einstein, for they all belong to the same crowd as the thousands of scientists who have not achieved historical recognition.

Of course there is no harm in celebrating these and other similarly well-known figures, provided that one also acknowledges that each of them stood on the shoulders of what I call the little people, rather than the giants in the famous quotation attributed to Newton. In the final analysis perhaps the famous scientists should *not* be celebrated, although that would make for a rather dull world.

I am criticized for saying that Janet did not catalyze the work of others which leads to the reviewer asking why I included this amateur scientist in my band of seven "little people." Janet's left-step table has been the basis of many studies aimed at finding the optimal form of the periodic table and of the theoretical foundations of the Madelung rule. The point is that Janet's table seems to represent the Madelung rule in a better way than the more traditional formats do (1-, 2, 3, 4, 5, 6). I included Janet's ideas because he represents an excellent example of a little- known scientist who made notable contributions, even if they were not appreciated at the time of his writing.

After finding all manner of faults, Kragh finally turns to the main ideas in the book.

He complains that I have nothing to say on ordinary workers and technicians who are surely equally entitled to be called "little people" in science. Although I agree with Kragh about the value of technicians, this is not a point I chose to make in the book. I deliberately chose to focus on little known individual chemists and physicists. My account regards the unit of evolutionary change to be individuals rather than groups in the way that Kuhn does. This is why I try to distance myself from the sociological approaches to the study of science, which seems to be another aspect that the reviewer finds puzzling.

Support for my choosing this course of action comes from an editorial piece in a recent issue of *Perspectives on Physics* that is appropriately entitled, "On Minor Scientists" (7):

> For decades, historians of science have realized the shortcomings of focusing narrowly on extraordinary individuals—a tendency often called the "great man myth," long recognized by historians in general. Yet this realization has not translated into significantly greater treatment of under-recognized scientists. Rather, it has generally meant scrutinizing large-scale social and institutional currents, collaborative efforts, the role of instruments, and other such processes. When attention has fallen on underrecognized individuals, they have tended to be the technicians, assistants, members of marginalized groups and other scientific laborers whose contributions went uncredited for reasons other than a lack of prize-winning breakthroughs. Scientists who fit squarely within the scientific establishment and did the type of work validated by traditional reward systems yet have long been eclipsed by their more illustrious colleagues find themselves neglected in the very stories that reject the myth of the great man. Can we tell the stories of underrecognized figures without seeking to cast them as secondary to larger processes or to elevate them (with the benefit of hindsight) to the pantheon of greatness? What might we learn from such studies?

Kragh takes issue with my view on truth and ridicules my assertion that scientific ideas should not be regarded as being right or wrong. The view that water is composite is "right" Kragh argues, while the view that water is elemental is simply wrong. However right and wrong, and admittedly the terms are not ideal in this context, should always be asserted within a particular framework. Elements themselves *are* composite as seen from the perspective of the fundamental particles that make up its atoms as the reviewer knows only too well.

My reason for downplaying the view that developments in science are right or wrong is best illustrated by means of a biological analogy. In the animal kingdom the gradual evolution of a new limb in some species, for example, cannot be said to be right or wrong. The new limb, which has resulted from random mutations in the DNA of the animal, may confer an evolutionary advantage in the individuals that possess the mutations. In retrospect we can claim that this development was "right" but always within an environmental context that the animals find themselves in.

Nevertheless, I agree with the reviewer that I have not provided a mechanism for my proposed evolutionary view of the growth of science and will attempt to do so briefly now. Like Kuhn I maintain that the development of science is non-teleological rather than being directed at an objective "out there" reality. But whereas Kuhn believes that the development of science is just analogous to biological evolution, I consider it to be more than an analogy. My appeal to evolution is not merely to biological evolution but to evolution writ large, by which I mean the evolution of the entire universe, the solar system, the geology of the earth as well as the evolution of life on earth. Each form of overlapping evolution of this kind presumably has a different mechanism.

The mechanism for the evolution of science must surely be of a psychological kind and it is not one that I am in a position to spell out at this stage. What I will say, and this *is* by way of an analogy, is that the mechanism may be similar to random mutations that are known to occur in the DNA of biological organisms. Such mutations govern biological diversification followed by natural selection of those organisms that best fit the environmental niche that the organisms find themselves in. So it is with intellectual ideas entertained by scientists. They are not, I suggest, arrived at deductively through clean logical arguments but first emerge in much the same way that biological mutations take place.

References and Notes

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