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Does Thomas Kuhn have a 'model of science'?

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There is no 'foolproof' method of presenting a set of ideas, especially in an environment where readers are often likely to be impatient, casual and unsympathetic. One cannot blame an author for the fact that many readers have been misled by their reading, especially if they have been misled by preconceptions that are projected onto, rather than derived from, the author in question. Steve Fuller, though he may have attempted a more rounded survey of Kuhn's background and character than is usual, has been no less impatient, casual and unsympathetic in his reading of Kuhn than have those who, in his view, have—as a result of Kuhn's malign influence—taken a wrong turning in their understanding of the political situation of science.

In relation to Fuller's account of Kuhn, much turns upon Kuhn's supposed model of science, especially in the way that it (allegedly) reifies the features of one (relatively brief) period of science into a general model of science. The vast changes which recently, through and after the warfare state of the Cold war, have transformed the context within which science works do not appear in Kuhn's account. At best, then, Kuhn's model of science is seemingly outmoded.

But does Kuhn *have* 'a model of science'? Not really. We will not deny that there is the simple schematic composed of the terminology Kuhn sets out in *The Structure of Scientific Revolutions*. Readers of his book seem to think they have adequately understood his ideas if they have (a) read that book and (b) got a handle on this vocabulary. The words 'paradigm' (later decomposed into 'disciplinary matrix' and 'exemplar'), 'normal science', 'revolution' and 'incommensurability' are the key words, and the ones on to which most readers settle their attachment, quite unlike Kuhn himself.

Do these terms comprise a 'model of science'? For Kuhn himself they are more a heuristic, and one which is to serve the purposes of (a) deflating the late-empiricist conception of scientific growth and (b) of guiding the writing of the history of *singular episodes in the development of science*. Kuhn was sufficiently relaxed about his distinctive terminology to make no use of it in his only major scholarly study of an historical episode in the development of science, his account of Max Planck's wrestling with the problem of black body radiation and the 'quantum'. Soon after its publication Kuhn had to write a supplement to the volume explaining that though the *words* were not to be found in the book, the ideas that had attempted to express

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were present in it. However, the important thing was to avoid converting these into a dogma about the history of science.

Is Kuhn's an account of *science*? Kuhn certainly emphasises, as Popper had done before him, that science is a 'social institution', and that scientific thought can be affected by the nature of the involvement of that institution with the other institutions and the culture of the wider society, as was manifestly the case with respect to the Copernican revolution. Again, though, the point was not to be converted (*pace* SSK) into a dogma, for the extent to which the development of scientific thought was directly and strongly shaped through the influence of extra-scientific social forces *was variable*, depending upon the configuration of social, cultural and scientific situations. There was a tendency for the degree of influence from 'the social' (in the sense of the wider sociocultural context) *onto the content of paradigms* to diminish in proportion to the (associated) increase in the technical character of the problems and in the professionalisation of the field around a paradigm. The difference between the two cases in manifest in the contrast between Kuhn's own *The Copernican Revolution* and his work on Planck, for while the former makes much play with broader cultural influences on astronomy, the latter encompasses little of the 'sociocultural' kind, save the difference that being expressed by an established figure can make to the acceptance of an idea. Kuhn also stressed—but did not especially follow up—the point that science involves more than just theories and ideas, that the development *of instrumentation* for empirical work is an important element in the development of science.

Whatever avenues might have been thereby opened up for 'sociological' treatment, Kuhn retained a focus on his own project, which was—consistently throughout his career—what at one point he identified as understanding 'change in scientific belief'. But what kind of problem is this? Is it an empirical-cum-sociological one, or is it a philosophical one? It was *always* a philosophical one, though it was only latterly that Kuhn came to regret that he had spent more time than he now thought was necessary on his historical concerns. This goes against the idea that Kuhn's 'model' was an invitation for historical/sociological elaboration, but leaves this question: what is the relation between the empirical/historical and the philosophical here? Sociologists and historians might understand Kuhn as asking an empirical/causal question: what are the *causal conditions* for a change in scientific belief—what conditions will bring an area of science into a state of crisis, and what conditions will determine the way in which the crisis is resolved, what will determine which party will be victorious in the revolutionary struggle? Given, of course, that one can find instances of crisis, revolution and so on, anyway.

We would not want to insist that Kuhn does not make (occasional) causal comments of this kind, nor need we do so, to make our point, which is that Kuhn's problem is not empirically causal in this sense, but is a philosophical one. His inquiry into 'change of scientific belief' centrally asks *this* question: what *kind of change* is a change in scientific belief. It is, therefore, historical examples of instances of change that provide not *evidence* for the testing of Kuhn's (causal) claims, but *material for reflection* upon *what we can intelligibly say* about the kind of changes that ensue from the appearance of a piece of 'revolutionary science'. The mainspring of Kuhn's career is then his insistent denial that the succession of scientific ideas involves a logically continuous replacement of earlier by later, thus giving pride of place to the theme of incommensurability, and the attempt to specify the kind(s) of discontinuity involved.

Put another way: if a new scientific idea is not logically compelling, then what kinds of attractions can give it appeal? Whilst recognising that an idea may appeal to a diversity of preferences—religious, ideological, aesthetic and so on—and stressing that these might be decisive to the adoption of the idea in a particular case, Kuhn nonetheless centres attention on the preferences that pertain to an idea's scientific status. The question is, again: *what kind* of change is involved, what standards are used in science to assess progress there? He maintains that the requirement for fundamental change is not primary (hence the concept of normal science) and outlines the 'normal' achievements of science, such as determining fundamental constants, increasing precision and the like.

We are not arguing about the specifics of Kuhn's case, here, but about its character. It points to factors which have determined the course of (a) science or the outcome of certain revolutionary struggles, but it makes no attempt to give a systematic account of the forces—'external' or 'internal'—that direct the course of science. It is not that kind of inquiry, but one that centres on the specification of what is involved (in the sciences) in identifying one piece of scientific work as an advance over another, identifying the main bases on which such a judgement is made. The appreciation that the science is predominantly a matter of problem solving (under conditions of normal science, of the sub-type 'puzzle solving') and that an important element in the appeal of a new idea is its capacity to 'raise the game' and present more demanding problem-solving challenges means that there will be a tendency—across areas of science—for the level of work to become more sophisticated and technically difficult. Further, the long term development of the natural sciences has resulted in proliferation, and in accumulation of a very substantial—very detailed, and very technical—knowledge of nature, in significant part as an outcome of the puzzle-solving, paradigm-shifting nature of the exercise. Such long-term development has seen change in the institutional setting of science, changes in the balance between science and other cultural systems within the societies of the west, in the organisational settings of scientific workers, in the social location and status of those who become scientists, in the connections between science and other organisations in the society—universities, companies, the state, but—if Kuhn's is a reasonable description—then the patterns of paradigm-shift is one that is entirely compatible with substantial changes in the institutional form of scientific activity.

It would not be in the spirit of Kuhn's treatment of his scheme as a heuristic to convert it into a dogma that would insist that scientific development *must* continue to follow the pattern of paradigm-shift (after all, Kuhn himself points out that some areas of science do cease to change in this way, and 'become engineering'). However, it is entirely possible that there may be changes in the nature of *natural* science that bring to an end the pattern that Kuhn describes, but were that to happen it would not invalidate Kuhn's account. If scientific paradigm-shifts ceased to take place or ceased to involve the kinds of preferences that Kuhn identifies, then change would no longer conform to Kuhn's so-called 'model', but that would not mean that it *never* did so.

There have been massive changes in the institutional structure of science throughout the twentieth century, The fundamental changes in physics at the beginning of the twentieth century are perhaps the most fundamental changes in the nature of physical sciences during that time (which, rather than any nefarious political evasiveness, as wildly and conspiratorially alleged by Fuller, might explain Kuhn-the-historian's interest in this period), marking a fundamental change in the very nature of physical science, and—eventually—giving great impetus to the institutional shifts that have taken place, especially the involvement of science with military and state secrecy, not to mention the massive growth of the universities, and the increasing incorporation of science into commercial laboratories. However, there is no clear or substantial reason to suppose that these changes have made a difference to the issues which were the focus of Kuhn's concerns, the characterisation of the grounds upon which an area of science will fundamentally change its loyalties. That fact that military or commercial preferences might be important drivers—along with, though perhaps in the contemporary world, instead of, religious and ideological ones—of scientific direction may be a valid observation, but it is already accommodated in Kuhn's observation on the role of extra-scientific influences upon the balance of preference within a scientific grouping. It is not easy—and perhaps impossible—to uniquely extrapolate from Kuhn's 'model' any conclusions about the significance of such institutional changes for natural science as problem solving, nor can one extrapolate from it Kuhn's personal views on these matters.

One final observation. Kuhn emphasises that the natural sciences tend to become very detailed, and very technical. He (rather regretfully) notes that there may be a price to pay for the knowledge of nature that has been acquired, in terms of the exclusion of laypeople from a proper understanding of what goes on in any reasonably advanced area of science. The increasing professionalism of an area of (natural) scientific work is presumably as much a consequence of the access-restricting feature of front-line scientific work, that it typically—if not quite invariably—requires intense familiarity with the work-and-results-so-far, which work and results are packaged in highly technical forms, as it is that which produces the exclusiveness. There is nothing to stop anyone accessing the major journals for any significant field of scientific work. Whether they could then read them is another question. The exclusion works both ways, of course, for the obvious reciprocal of Kuhn's point is that the scientists themselves are, outside the area of their special and specific competence, only just another member of the society. So 'scientists' in general can claim no monopoly of competence on how to politically control science. One does not have to choose between them ('the community of scientists') on the one hand and the new rhetoricians of the 'construction' of science, namely the so-called 'social epistemologists', on the other. Rather, science policy must always be a matter of the reconciliation of a very specific scientific specialism on the one hand and the social polity as a whole on the other. Kuhn's adaptation of Conant has offered one of the very best ways ever devised for some at least of the polity to be educated as to the actual nature of scientific change. Fuller's attempt to poison Kuhn's reputation, through bizarre and unsubstantiated claims as to Kuhn's alleged hidden political agenda, was presumably designed to undermine the possibility of people learning from Kuhn about the history and philosophy of science. That is one reason among many why we agree with Thomas Uebel, in his masterly paper demolishing the pretensions of the totality of Fuller's recent writings of science, 'The poverty of "constructivist" history (and policy advice)',[1] that *it would be a terrifying thought that someone like Steve Fuller might be giving a lead in matters of science policy.*

Note

1. In M. Heidelberger and F. Stadler (eds), *History of Philosophy and Science* 2002, pp. 379–389.