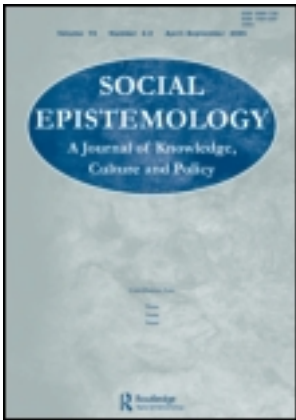


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Kant on wheels*

PETER LIPTON

At a New York cocktail party shortly after the war, a young and insecure physics postgraduate was heard to blurt out to a woman he had met there: ‘I just want to know what Truth is!’ This was Thomas Kuhn and what he meant was that specific truths such as those of physics mattered less to him than acquiring metaphysical knowledge of the nature of truth. Soon afterwards, he gave up physics, but rather than take up philosophy directly, he approached it by way of the history of science. The work that followed, especially *The Structure of Scientific Revolutions*, published in 1962 and now with sales of well over a million copies, makes his the most important contribution to the history and philosophy of science of the twentieth century.

Kuhn was struck by the consensus among those working in particular disciplines during periods of what he came to call ‘normal science’. It isn’t just that they accept the same theories and data, they also have a shared conception of how to proceed in their research, a tacit agreement about where to look next. There is agreement about which new problems to tackle, what techniques to try and what count as good solutions. It is rather as if new practitioners in a particular discipline are covertly given copies of a book of rules, the secret guide to research in their field. But no such rulebooks exist. Kuhn wanted to find out what does the job of the rules that aren’t there.

What he found was that scientists learn to proceed by example rather than by rule. They are guided by what Kuhn called their *exemplars*, or certain shared solutions to problems in their speciality, like the problem sets that science students are expected to work through. (‘Exemplar’ captures the most important sense of Kuhn’s famous multivalent term, ‘paradigm’.) The function of problem sets is not to test students’ knowledge but to engender it. Similarly, exemplars guide research scientists in their work, for although, unlike rules, they are specific in content, they are general in their import. Scientists will choose new problems that seem similar to the exemplary ones, will deploy techniques similar to those that worked in the exemplars, and will judge their success by the standards the exemplars exemplify.

*Review of Thomas S. Kuhn, *The Road Since Structure*, edited by James Conant and John Haugeland (Chicago–London: The University of Chicago Press), 2000; and Steve Fuller, *Thomas Kuhn: A Philosophical History for Our Times* (Chicago–London: The University of Chicago Press), 2000. This review was originally published in *The London Review of Books*, 19 July 2001, pp. 30–31.

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This idea of the co-ordinating and creative power of exemplars provided Kuhn with the basis for his general model of how sciences develop. Any new area of scientific inquiry must do without exemplars to start with and hence without the co-ordination of normal science. If suitable exemplars are eventually found, normal science can proceed. But exemplars sow the seeds of their own destruction, since they will eventually suggest problems that are not soluble by the exemplary techniques. This leads to a state of crisis and in some cases to a scientific revolution, where new exemplars replace the old ones and another period of normal science begins.

A scientific revolution is more disruptive than a simple replacement of one theory by a better one, because the theories held on either side of it are not just incompatible, they are ‘incommensurable’. In *Structure*, Kuhn used that term to refer to various factors that make the evaluation of competing theories problematic. Scientific revolutions are not irrational episodes, they are stages of enquiry where rationality becomes a much more complex and messy business than during periods of normal science. This is so in part because straightforward argument requires many shared premises, which are what normal scientists enjoy and revolutionary scientists lack.

This brings us back to the New York cocktail party. What truth is not, according to Kuhn, is an accurate representation of the world as it is in itself. Scientific theories represent a world, but one partially constituted by the cognitive activities of the scientists themselves. This is not a commonsensical view, but it has a distinguished philosophical pedigree, associated most strongly with Kant. The Kantian view is that the truths we can know are truths about a ‘phenomenal’ world that is the joint product of the ‘things in themselves’ and the organising, conceptual activity of the human mind.

Kuhn, however, is Kant on wheels. Where Kant held that the human contribution to the phenomenal world is invariant, Kuhn’s view is that it changes fundamentally across a scientific revolution. This is what he means by his notorious statement that, after a scientific revolution, ‘the world changes’. This is neither the trivial claim that scientists’ beliefs about the world change, nor the crazy claim that scientists can change the things in themselves simply by changing their beliefs. It is the claim that the phenomenal world changes because the human contribution to it changes.

For Kuhn, scientific development is neither cumulative nor teleological: science is not moving towards the goal of a theory of everything. His favoured analogy is with biological evolution by natural selection. Organisms develop under selective pressures from the environment, but development, though tending to an increase in complexity and differentiation, has no fixed goal. Moreover, the selective environments that determine which organisms survive and which die off themselves change and are partially constituted by the organisms’ own activities. Biological evolution is not moving towards an ideal organism, and scientific evolution is not moving towards the Truth. Both processes are pushed from behind, not pulled from the front.

Kuhn’s work makes a signal contribution to our understanding of how science works and what it achieves. Scientists may be good at running science, but nobody understands very well how it works. Our best models of how they test their theories have absurd consequences—the idea, for example, that every observation is evidence for every theory—and our accounts of where those theories come from in the first place are inchoate at best. The possibilities Kuhn articulates and the

arguments he makes are a crucial resource for improving our feeble understanding.

Much of what he says about the power of exemplars to direct and shape scientific research is fundamentally correct. Scientists' problems and their solutions do not come out of thin air, but there are no rules known to scientists or philosophers that can explain their source. Exemplars and the similarity relations they impose are the only plausible alternative. Scientists learn primarily by example, and imitation and analogy are essential motors of research.

What I resist in Kuhn's account is the movement from the exemplar model to the notion of scientific revolutions involving a radical change of worlds, when it in fact appears compatible with the more traditional view that science, though fallible, is nevertheless in the truth business, telling us an increasing amount about a mind-independent world. The reasons Kuhn gives do not seem powerful enough to establish his dramatic alternative view.

In addition to *Structure*, Kuhn did research of the first importance on particular aspects of the history of science, with a book on the Copernican Revolution in astronomy and another on the development of Quantum Mechanics. He also developed and modified his views on the nature of science in a series of articles. A first collection of these was published in 1977 under the title *The Essential Tension*, and now *The Road Since Structure* is a second.

It's sometimes claimed that Kuhn significantly toned down his radical views after *Structure*; but this is a mistake. He did occasionally repudiate earlier material, but the bulk of his later work is a significant articulation and defence of his fundamental views, not a retraction. For example, he extends his Darwinian analogy, to describe a process resembling to biological speciation whereby both old and new scientific traditions may survive, as an alternative to the model of simple replacement through revolution. He also develops his account of the social dynamics of scientific communities, focusing especially on the way in which a period of scientific crisis may serve to spread cognitive risk, with different scientists following different avenues of inquiry.

In his later work, Kuhn also narrows and deepens his notion of incommensurability. To say that two theories are incommensurable comes to mean that there is no scientific language which can fully express both: incommensurability is untranslatability. Theories on either side of a revolution divide up the world in systematically different ways, so that while it may be possible to become 'bilingual', the meanings of the sentences resist principled translation into a common language. Kuhn makes clear that this does not involve any irrationality, he is simply trying to show how complex the rationality of scientific inquiry may become during periods of radical change.

The Road Since Structure ends with a fascinating 68-page interview with Kuhn, recorded a year before his death. This gives a strong sense of his personality and of the development of his ideas and his career. It brings out the extent to which the history of science was for him from the start a vehicle for philosophical inquiry. It showed how much Kuhn relied on what he took to be his exceptional talent for getting inside the heads of past scientists, to see their projects and their worlds in their terms.

Steve Fuller's book about Kuhn is somewhat odd in content, style and tone. There is surprisingly little engagement with his ideas: incommensurability and truth are not much discussed, and the key notion of an exemplar is barely

mentioned. Rather, what counts for Fuller is not what Kuhn said, but what people have thought he said, and the morals they drew. This is reception history, but doesn't in the end serve Fuller's purposes.

Fuller's book is a mixture of peppy prose and convoluted argument. The discussion moves from Plato to NATO and back again, with dizzying speed and an extraordinary density of citation. (The bibliography contains about 600 publications, including 44 by Fuller himself, and the text more than 900 mostly discursive footnotes). The tone is almost unremittently hostile to Kuhn; in a couple of places it is offensive.

For Fuller, the point of studying scientific practice is not to interpret science but to change it, and bring it under the control of society. The primary source of his allergic reaction is the belief that Kuhn's influence has been powerfully conservative, insulating science from criticism, both internally and externally. The reaction is excessive but the central charge should be taken seriously, and Fuller can be a canny prosecutor.

According to Fuller, the reception of Kuhn's work has had two types of pernicious effect, the first on his readers generally, the second on those professionally involved in science studies. Kuhn's model of normal science is one of a small community of inquirers who agree with each other and are uninfluenced by the rest of society. Many of his readers have taken this to be the way science both is and ought to be, and the moral drawn is that scientists *ought* to be as conservative and isolated as Kuhn says they are. Thus we are wrongly encouraged to leave scientists alone.

Things are supposed to be even worse for sociologists of science. They tend to suffer from science envy and, accepting Kuhn's model, attempt to turn their discipline into a science by acting like normal scientists. This means that they see themselves as an isolated community, and in particular fail to criticise the real scientists. They look, but they don't touch.

The second case is not a strong one. It's a familiar claim that some social scientists suffer from physics envy and accept the Kuhnian picture, hoping to make their disciplines more scientific by finding themselves a paradigm. But this doesn't explain their disinclination to engage in a critique of science. Kuhn's normal community is indeed a group that likes to pick its own problems without outside interference, but that doesn't require that its members be disinclined to criticise or advise those outside it. The desire to assimilate the sociology of science to normal science doesn't explain why those who practise it would forebear advising scientists.

This is not to say that accepting a Kuhnian model shouldn't influence the kind of advice one might give them. For example, since scientists are guided by exemplars rather than by rules, even if philosophers of science were able to come up with a reasonably good general account of some aspect of scientific research, it isn't clear that teaching this to scientists would directly improve their practice, any more than learning physics is likely to improve one's ability to ride a unicycle.

The first effect that Fuller claims Kuhn's ideas have had is more interesting, because not only do his normal scientists get to choose most of their own problems, they must do so if the exemplar principle is to function properly. They will always select new problems that resemble the exemplary ones, whereas problems chosen by outsiders are unlikely to resemble them, and thus be unlikely to yield to the exemplary techniques.

In an article published while he was writing *Structure*, Kuhn made this point well by inverting the stereotyped contrast between supposedly conservative engineers and free-thinking pure scientists. Engineers or applied scientists typically have their problems imposed on them from without, by government or industry, and so they have no reason to suppose that the techniques that have worked previously will also work smoothly on the freshly imposed problems. They may in consequence have to be more inventive than the pure scientists, who have every reason to believe that something like old solutions will be applicable to new problems.

Does the Kuhnian model in fact discourage people from undertaking what would be a legitimate and constructive critique of science, from both within and without the scientific community? Fuller's blanket charge is unwarranted. Kuhn's account of science is compatible with extensive public control over the funding of research and the application of technology. Moreover, it allows for radical change. He was after all writing about scientific revolutions, or times when the consensus ends and criticism comes both from those within the old tradition and those who want to overthrow it. Fuller's answer to this is that Kuhnian revolutions are irrational, leaving no room for a rational critique. This is inadequate. Revolutions are cognitively complex, but one of Kuhn's central claims is that this is the nature of rationality, not a failure to meet some imaginary standard of tidy evaluation.

Not everything is permitted, however. Kuhnian science does have strongly conservative elements. Kuhn is claiming that the distinctive fruits of science can arise only among communities of inquirers whose members have considerable freedom to choose their own problems and who enjoy relatively long periods of cognitive consensus, allowing them to push their theories, techniques and exemplars as far as they will go. If he's right, then the autonomy and conservatism could not be abandoned without paying a high cognitive price; it would be the end of science as we know it. At the same time, they also have a hefty social and political price. It's not enough to investigate the causes and effects of Kuhn's claims, therefore, we need to figure out whether they're right.