

# Three conservative Kuhns

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## *Introduction*

Was Kuhn a conservative? That bald question seems almost absurd. Surely the ‘philosopher of scientific revolutions’ was himself a revolutionary who overthrew the old positivist picture of science and replaced it with a radical new account of scientific change? What not his incommensurability thesis one of the philosophy’s most radical challenges to the rationality of science? Did not his work provide the impetus for a sociological view of science, one of whose inheritors is the ‘leftist’ side in the so-called Science Wars? These suggestions notwithstanding, I believe that there are three respects in which Kuhn may be characterised as a conservative thinker.

First, Kuhn was a conservative in that contrary to what is popularly thought his philosophy was not a root-and-branch rejection of positivism but was rather at most a rather partial revision of positivism that retained many of its most important features. When compared with subsequent anti-positivist and anti-empiricist developments, which Kuhn did not embrace, his thought appears distinctly old-fashioned. Secondly, as David Bloor argues, Kuhn is a conservative in the Mannheimian sense according to which the conservative stresses the importance of tradition.<sup>1</sup> Lastly, we come to Steve Fuller’s accusation that Kuhn’s work had a significantly conservative political role. These three senses of ‘conservative’ are distinct and are not necessarily linked. I shall review each in turn, concentrating on Fuller’s contention, before asking whether there is in Kuhn’s case there is a linking theme.

## *Kuhn as a philosophical conservative*

Kuhn was one of the most influential philosophers of the second half of the twentieth century. His work helped overturn the positivist orthodoxy. His incommensurability thesis struck many as thoroughly radical. And Kuhn’s view of scientific revolutions is, in Lakatos’ notorious words ‘irrational, a matter for mob psychology’.<sup>2</sup>

I believe that Kuhn despite (or perhaps because of) his influence was deeply misunderstood, by friends and foes alike. Both tended to overplay the radical

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aspects of his thinking and to underplay its conservative roots. Let us take, for example, the quotation from Lakatos just given. If it were true that Kuhn rejected rationality and substituted mob psychology for it, then that would indeed have been radical. But Kuhn did nothing of the sort. For many philosophers of science, both the positivists and falsificationists, rationality is a matter of conforming to certain rules. Thus Carnap tried to develop an inductive logic. Popper, rejecting induction, limited rationality to deductive logic. Lakatos tried to relax Popper's application of deductive logic in his methodology of a scientific research programmes, but the overall idea remains that there is a model of rationality to which good scientific decisions should conform. It is only when decisions diverge from the requirements of the canons of rationality that psychology need be invoked.

Where Kuhn differed from this line of thought is in his emphasis on the psychology of scientific decision making in all cases, including, indeed particularly, ordinary, perfectly respectable scientific research. So Lakatos was right in highlighting Kuhn's employment of psychology. But he was wrong in thinking that a psychological explanation is incompatible with the rationality of what is thereby explained. The idea that rationality is a matter of conforming to rules that in principle can be articulated (and articulated *a priori* by philosophers) is a deep-seated one, and one that has been largely rejected by psychologists and philosophers alike (although not all philosophers have given up on it). It was indeed innovative of Kuhn to see that this is an error and to develop an alternative, psychological and social account of scientific rationality that does not appeal to rule and canons. Instead Kuhn emphasizes the role of the paradigm as an exemplary piece of scientific research. Training through practice with paradigms-as-exemplars gives young scientists a sense of what problems are worth pursuing, which angles might prove fruitful, and what a good solution looks like. This 'sense' cannot be articulated in a set of rules any more than the ability to recognise a face can be articulated in a set of rules. But just as the latter can be cognitively effective and entirely 'rational' without rules, so can the former.

Genuinely innovative though this idea was, it was also, as Kuhn complained, both the most important and the least understood aspect of *The Structure of Scientific Revolutions*. Kuhn's critics tended to focus on the notorious incommensurability thesis, and Kuhn himself later sought to develop this rather than the paradigm concept. The radical nature of this thesis has also been exaggerated. Kuhn denied that it means that theories are non-comparable. Even so, the claim that Newtonian mechanics cannot be seen as an approximation to or limiting case of special relativity is a dramatic one. What is significant about this thesis, however, is not that it is a radical departure from accepted views but rather that it is a consequence of an only partial shift in thought. For the thesis depends crucially on the idea that the meaning of scientific terms depend extensively on the theories in which they are embedded. Incommensurability arises when one combines this entirely standard positivist view with the thesis of the theory-dependence of observation. The latter, though a departure from positivism, is not alone sufficient to generate incommensurability.<sup>3</sup> A departure from positivism that *also* rejected the theoretical context view of meaning would be less conservative. But such a rejection of positivism would not generate the incommensurability thesis. Causal accounts of reference are precisely such a departure. Yet Kuhn opposed this move away from positivism precisely in order to hold onto incommensurability. Whether one accepts the causal view or not, it is clear that as regards the history of philosophy in the

twentieth century, Kuhn's adherence to the theoretical context view is distinctly conservative.<sup>4</sup>

Similar remarks may be made concerning Kuhn's rejection of naturalism. This is all the more startling a revelation of Kuhn's philosophical conservatism because *The Structure of Scientific Revolutions* is thoroughly naturalistic in a way that was ahead of its time. It was this naturalism that philosophers such as Lakatos failed to understand. By the time naturalism had become respectable in philosophy Kuhn had repudiated it in favour of a more traditional, *a priori*, philosophical (and Kantian) approach.

*Kuhn as an historical conservative*

I have suggested that Kuhn is a conservative in the sense of holding on to older views in the face of a movement to reject them. Karl Mannheim employs a different sense in which a thinker can be a conservative. Mannheim contrasts conservative thinkers and thinkers who exemplify the 'natural law ideology'. Natural law ideologists, the inheritors of the Enlightenment, emphasize reason conceived of as the application of general and sempiternal rules of judgment; the conservative appeals to history and local factors.<sup>5</sup> Translated into the philosophy of science the natural law approach looks to an *a priori* and perfectly general scientific method which will enable scientific beliefs to progress (and in particular, progress towards the truth). As we have seen, the positivists and even the anti-positivist Popper and Lakatos, adopt just this sort of view. *A priori* inductive logic or falsificationism, or the methodology of scientific research programmes provide general rules or structures for assessing the rationality of a choice in science. These rules are entirely independent of history and culture.

In the previous section I did not portray Kuhn as a philosophical conservative in every respect. His important innovation was to highlight the importance of exemplars in determining the development of science. It is ironic therefore that the respect in which Kuhn was genuinely radical (and, I think, right to be radical) is a respect that marks him out as a Mannheimian conservative, contrasting with the natural law ideology of his positivist predecessors and his falsificationist critics. Kuhn depicts science as made up of largely disunited practices that progress according to the rule-less impetus of their current paradigms, adjusted not with a view to representing the facts but instead with the aim of making the best local improvement to the existing tradition.

There is however an aspect of Kuhn's Mannheimian conservativeness that might look conservative in a more general sense. Kuhn first broached the ideas that would lead to the paradigm concept in his essay 'The Essential Tension'.<sup>6</sup> The latter was presented to a meeting of science educators in Salt Lake City. The standard left-liberal view in education generally, including science education, was that young people need to be encouraged to think divergently, to look critically upon existing doctrines. (The philosophical analogue of this is Popper's falsificationism.) Kuhn argued that this ignores an important part of the story. For a science student in particular needs to acquire habits of convergent thinking first, without which the divergent thinking is fruitless. He or she needs to learn to copy with accuracy the standard experimental, investigative, diagnostic, and mathematical techniques. These need to be internalised in a way that is not much more sophisticated than

rote-learning. To that extent, divergent, critical thinking needs to be discouraged, and a conservative attitude that respects and emulates tradition needs to be engendered.

*Kuhn as a political conservative*

Steve Fuller has argued that Kuhn, in effect, served a conservative *political* agenda. Fuller's criticism has several strands. *The Structure of Scientific Revolutions* must be seen, he says, against the educational background that caused Kuhn to write it. In the early 1950s Kuhn taught the General Education in Science curriculum at Harvard. This program had been devised by Harvard's President, James Conant, as a way of informing students, humanities students in particular, about the nature of science and its history. Conant was a central figure in the liberal-conservative establishment, a supporter of Big Science, who left Harvard to become the first U.S. ambassador in West Germany. The motivation behind the program was to build in the minds of America's future leaders a particular image of science: science as an autonomous institution. Science needed to be protected from governmental interference, and so the elites needed to be reminded that the dynamic of science is generated internally not externally. On the other hand science also needed to be defended from criticism directed at the uses of science such as the application of physics in the design of nuclear weapons. So the second reason to reinforce the autonomy of science is directed towards science's public legitimation. Science needed to be divorced in the public mind from technology. Kuhn's image of science certainly fulfils these requirements. Science generates its own puzzles; good science occurs when it sets its own agenda. And what drives science is the desire to solve these puzzles, not the need to produce inventions however beneficial or deadly. If a puzzle-solution happens to be of use in some way then putting it to that use is a part not of science but of technology.<sup>7</sup>

A second strand in Fuller's criticism concerns what he calls the 'double-truth' doctrine. (This might be better dubbed a 'double doctrine' doctrine.) The general idea is that the masses are not given the same message as the elites if to do so would be socially destabilising. Fuller identifies this in particular in the kind of history of science Kuhn claims is appropriate to science students and other scientists. Rather than being awarts and all picture of the development of a scientific discipline, history-of-science-for-scientists is cleaned up, with crises and revolutions removed, with old ideas expressed in modern terminology or formalism, so that historically ancient ideas, so long as they are on the path that leads to today's science appear more modern and less contentious than they really are. It is not simply that this differs from the sort of history of science that Kuhn thinks that historians should be writing (and which is illustrated in his own historical work). Rather, what makes this a double-truth doctrine is that Kuhn thinks it is right that scientists be given the cleaned up picture. It would not be good for science if scientists were taught the history of science that the historians write.

Fuller's third claim is that far from encouraging critical attitudes towards science (which he repudiated) Kuhn's work blunted criticism. In general, the process of crisis, revolution, replacement of an old paradigm by a new, revisionary paradigm, shows that science has its own mechanisms for self-criticism, change, and renewal. Hence external criticism is unnecessary. In particular, philosophical criticism is

blunted, first in the emphasis laid on dogma in normal science is highly conservative especially when compared to Popper's critical rationalism, and secondly because the empirical, historical attitude towards science removes the normative element from the philosophy of science. At the same time *Structure* encouraged social theorists to see themselves as scientists rather than as critics of science.

### *Conclusion*

The three kinds of conservatism discussed need not tie up. Edmund Burke was a political and historical conservative who was a philosophical radical. A political radical might be a philosophical conservative, sticking uncritically to Marxist dogma. A leftist might also be an historical conservative, pessimistically noting the power of tradition to thwart rational calls for political change.

However, in Kuhn's own case it is possible to find links between the different kinds of conservatism. As mentioned, Fuller says that the conservative political agenda was served by painting science as an autonomous institution, distinct from technology. Furthermore, charges Fuller, by arguing that science has internal mechanisms for criticism, change, and renewal, Kuhn blunted that force of those who called for science to be subject to external criticism, whether political or philosophical. If these claims are correct, then they are consequences of Kuhn's historical conservatism. For it is because science develops by reference to its own tradition that science is distinct from technology, which is driven by the needs and wants of society. And while that fact is not inconsistent with the rationality of science, it is inconsistent with the natural law thought that science may be judged by reference to *a priori* rules of scientific method (to be developed outside science by philosophers). And so it is clear that some of the features that Fuller identifies as politically conservative are themselves consequences of aspects of Kuhn's thought that are conservative in the Mannheimian, historical sense.

The double-truth doctrine is also a consequence of the historically conservative nature of Kuhn's thinking. As we have seen young scientists need to be inculcated into the scientific tradition by a process of intellectually convergent training. The full, unvarnished truth is not always an effective medium for the transmission of the tradition or for the learning of established techniques. Thus, for example, the exercises given to undergraduate students are drawn from simplified models. And where the problems covered are ones that are long established parts of the tradition (having been first solved by great figures such as Newton or Huygens) their treatments will not only have been simplified but also modernised. This way, what a young scientist learns about this history of her discipline will be a multiply distorted version of the truth, one which is quite different from that learned or uncovered by the historian of science.

Does Kuhn's philosophical conservativeness relate to either his historical or (alleged) political conservativeness? I think it relates to the latter in two ways. Fuller accuses Kuhn of a kind of world-historical myopia. Kuhn's work contributed to Conant's liberal-conservative agenda in a way that Kuhn was at most only dimly aware of. Similarly Kuhn was not entirely conscious of his role in the development of the philosophy of science. As I have explained, Kuhn was only partially aware of what was and what was not a positivist doctrine. As such he was

not able to see that his views represented a continuation of positivism as much as a departure from it. But more telling is Kuhn's response to philosophical criticism. A number of responses were open to Kuhn. One might have been to adopt the radical relativist, even anti-rationalist position of which he was accused (and happily adopted by Feyerabend). Another would have been to stick to his guns and to explain why his naturalistic views do not have the extreme character attributed to them. During the 1960s he stuck to the second strategy, although there is a tinge of the first. But as the volume of discussion increased and his more radical 'supporters' became more vociferous, Kuhn adopted a third, much more conservative strategy. He dropped the naturalism that had been the cause of so much controversy and reformulated what had been naturalistic psychological and sociological theses in philosophical and linguistic terms. These theses thereby became 'acceptable' to academic philosophy. At the same time he repudiated the Strong Programme and constructivist sociology of scientific knowledge.<sup>8</sup> Thomas Kuhn, the physicist turned historian who wanted to become a philosopher, and who unwittingly became a cult figure for intellectual revolutionaries, was able to reinvent himself as a respectable mainstream philosopher.

These links may lend some support to Fuller's thesis. Even so, I believe that the charge of political conservatism is not only more controversial than the other two claims of conservatism, it is also less well confirmed by the evidence. First of all, it is not clear that *Structure* had quite the conservative impact Fuller suggests. Is it really true that there would have been more and better criticism of science had it not been for that book? The fact that it has been used for some conservative legitimacy purposes (for example by Francis Fukuyama) does not really show that Kuhn's work significantly furthered those purposes, let alone that Kuhn shared them. It is the fate of iconic texts to be pressed into the service of all sorts of causes, and in Kuhn's case it is clear that citations of him by science's critics are not meagre.

Fuller extracts the double-truth doctrine from Kuhn's remarks concerning the teaching of the history of science to science students. The latter receive a cleaned-up, Whiggish kind of history, unlike the story Kuhn tells us of crises, revolutions, incommensurability, and so on. On that reckoning *Structure* contains the elite message. But that is incompatible with the role ascribed to the book in Fuller's other criticisms, as conveying its message to the public, which will include the very scientists from whom the elite message must be hidden. In any case, it is not clear that Kuhn thinks that scientists should not be given a warts and all history of science if they happen to be studying the history of science for its own sake. Rather, if history of science is to be used at all in teaching scientists to be *scientists* the tidied up image is more helpful. As a matter of fact, as Kuhn points out, the historical element in science teaching is thin, unlike in the humanities, and Kuhn's remarks on the topic are scarcely central to his account; even less do they amount to a plank in a political agenda. While Fuller's treatment of Kuhn and his context is full of erudition, telling parallels, and insightful suggestions, ultimately his case against Kuhn rests on association rather than documentary proof; the evidence is circumstantial rather than concrete. And even if Fuller's account of the true nature of Kuhn's work were correct, that would not obviously impact on our assessment of his philosophy *as* philosophy or his history *as* history.<sup>9</sup>

*Notes*

1. D. Bloor 'The Conservative Constructivist' *History of the Human Sciences* 1997, 123–5.
2. I. Lakatos 'Falsification and the Methodology of Scientific Research Programmes' in Lakatos, I. and Musgrave, A. (eds.) 1970 *Criticism and the Growth of Knowledge* London: Cambridge University Press, 178.
3. See, e.g., W. Newton-Smith *The Rationality of Science* London: Routledge and Kegan Paul, 1981.
4. For a detailed account of Kuhn's philosophical conservativeness see A. Bird 'Kuhn's Wrong Turning' *Studies in History and Philosophy of Science* 2002, 445–463. This theme is given added impetus by the portrayal of the similarity between Kuhn's thought and that of Carnap in G. Irzik and T. Grünberg 'Carnap and Kuhn: Arch Enemies or Close Allies' *British Journal for the Philosophy of Science* 1995, 285–307.
5. K. Mannheim *Essays on Sociology and Social Psychology*, London: Routledge and Kegan Paul, 1953.
6. T. S. Kuhn 'The Essential Tension: Tradition and Innovation in Scientific Research' in Taylor, C. (ed.) *The Third (1959) University of Utah Research Conference on the Identification of Scientific Talent* Salt Lake City: University of Utah Press, 1959, 162–174, and in T. S. Kuhn *The Essential Tension. Selected Studies in Scientific Tradition and Change* Chicago: University of Chicago Press, 1997, 225–239.
7. Fuller locates this line of thinking in a tradition going back to Whewell. He cites Planck as a key exponent of it *contra* Mach, and links it (not entirely perspicuously) to the realism-instrumentalism debate.
8. T. S. Kuhn 'The Trouble with the Historical Philosophy of Science' *Robert and Mawrine Rothschild Distinguished Lecture 19 November 1991 An Occasional Publication of the Department of the History of Science* Cambridge MA: Harvard University, 1992.
9. Some of the material in this essay has been taken from my book *Thomas Kuhn* Chesham: Acumen and Princeton: Princeton University Press (2000).