Books reconsidered

The structure of scientific revolutions
Thomas S. Kuhn
Chicago: University of Chicago Press, 1962
2nd enlarged edition, 1970. 3rd edition, 1996.

Thomas S. Kuhn's *The structure of scientific revolutions* is a classic text in the history and philosophy of science. It is one of the best known works in the field outside this area of academic specialization. One need only mention the term 'paradigm' to register the extent to which Kuhn's ideas have entered the vernacular.

Traditionally, philosophers of science have tended to focus on questions about the nature of scientific method. Kuhn brought a historical orientation to bear on such questions. *Structure* opens with the words, 'History, if viewed as a repository for more than anecdote or chronology could produce a decisive transformation in the image of science by which we are now possessed' (p.1). Kuhn proposed a model of scientific change, that emphasizes historical development. As Kuhn foresaw, reflection on its history has transformed our image of science. Science is now seen as a developing process, practised by humans in a variety of shifting historical and social circumstances.

On the model proposed by Kuhn, a science passes through a series of stages. At first, work in a field of science is fragmented. There is little agreement on matters of method or substance. In time, though, consensus forms around a basic viewpoint. A paradigm has emerged. Scientists now focus their research on the specific problems thrown up by the paradigm, which Kuhn calls 'puzzles'. Such puzzle-solving, Kuhn says, is the focus of science in its normal state. Normal science is highly effective. It permits scientists to conduct detailed research without questioning fundamental assumptions. But not all puzzles are solved by the paradigm. Anomalies arise which the paradigm is unable to resolve. A sense of unease may take hold of the field, signalling that the scientific community is in crisis. At such times, scientists propose novel solutions to the anomalies that may eventually replace the reigning paradigm. When the latter occurs, a scientific revolution takes place. and a new period of normal science begins under the banner of the new paradigm.

It is natural to use Kuhn's special terminology to present his ideas, as I have just done. 'Paradigm', 'puzzle', 'normal science', 'anomaly', 'crisis' and 'revolution' are all terms that Kuhn uses to express his account of science. They are Kuhn's keywords. The words have a special place within Kuhn's model of science. If one understands the words, one understands the model. They derive their meaning from their place in the model. They may in turn be used to explain the model.

This illustrates a general point about theory development, which Kuhn himself emphasizes in the case of science. The development of new or modified concepts, and a new or altered vocabulary to express these concepts, is part and parcel of developing a new theory. As a result, change in the meaning of the terms used by scientists, and introduction of novel vocabulary with new meanings, are typically associated with the revolutionary transition between paradigms.

It is fair to describe a paradigm as the basic viewpoint around which consensus coalesces in a field of science. But there is more to be said. A paradigm is in the first instance an exemplary historical achievement within a field of science, for example, an experiment, a discovery, a formula or even a book. Scientists who work in a field of science take this historical achievement as the founding point in their field to which they look for inspiration. They model their research on the achievement. They draw methodological insight from it, and base their research agenda on it. The initial achievement thus comes to serve as the basis for a tradition of research which derives from it. In time, a network of ideas is built up which is associated with this research tradition. Thus, a paradigm is more than the initial achievement on which the field is founded. It also includes the tradition that emerges out of the achievement, as well as the scientific outlook or world-view that scientists immersed in the tradition espouse.

Under the influence of Ludwig Wittgenstein and gestalt psychology, philosophers of science of the 1950s increasingly came to doubt the independence of observation from theory. Kuhn joined Paul Feyerabend, N.R. Hanson and others, in arguing that scientific observation is influenced by the theoretical standpoint of the observer. The resulting 'theory-dependence of observation', as this situation is called, reflects a further aspect of the role played by paradigms in scientific research. Not only do scientists view the world from the perspective of a paradigm. The very nature of their perceptual states is subject to the influence of the paradigm in which they work. As a result, a change in paradigm leads 'scientists to see the world of their research engagement differently' (p.111). Indeed, Kuhn goes so far as to liken change of paradigm to a change in the world in which scientists work: 'practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction' (p.150).

Readers of *Structure* are sometimes perplexed by Kuhn's talk of 'world change'. Some choose to interpret

the form of words figuratively. They read it as a metaphor employed by Kuhn to emphasize the radical change in theoretical outlook associated with a scientific revolution. But Kuhn seems to have meant something stronger than this.

In his compelling study of Kuhn, *Reconstructing scientific revolutions: Thomas S. Kuhn's philosophy of science* [1], Paul Hoyningen-Huene provides a neo-Kantian interpretation of Kuhn's basic metaphysical outlook. On such a position, there is a mind-independent reality, but it is unknowable by us. Hoyningen-Huene calls it the 'world-in-itself'. By contrast, there is a world of appearances, a 'phenomenal world', to which scientists have epistemic access. The phenomenal world is constituted out of our concepts and sensory input deriving from the world-in-itself. While the latter is fixed, the phenomenal world is subject to variation with scientific revolution, as scientists impose new concepts on the pre-existing reality that they cannot directly apprehend. On Hoyningen-Huene's interpretation of Kuhn, there is a sense in which the world changes with paradigm, though it is the phenomenal world rather than the world-in-itself that changes.

To turn from metaphysics to methodology, *Structure* opened up new directions on this front as well. Traditional philosophers of science tended to suppose that science is characterized by a unique scientific method. Possession of this method is what serves to demarcate science from non-science and pseudo-science. The scientific method was generally taken to be universal and invariant, employed throughout all branches of science, irrespective of historical period or social context. But a number of the doctrines of *Structure* run counter to the traditional idea of a unique scientific method.

In a 'Postscript' appended to the second edition of *Structure*, Kuhn admits the permanence of some aspects of scientific method. But the impression created by the first edition of *Structure* was quite otherwise. There Kuhn emphasizes the paradigm-dependent nature of scientific methodology. The normal scientific puzzles that scientists attempt to solve derive from the paradigms in which they work. This has an impact on method. For the standards of puzzle-solving success, the criteria that distinguish good from bad science, are also dependent on paradigm. 'Paradigms', Kuhn writes, 'differ in more than substance . . . [t]hey are the source of the methods, problem-field and standards of solution accepted by any mature scientific community at any given time' (p.103). Thus, 'when paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions' (p.109).

With this last claim to hand, let me now draw out four main themes of Kuhn's account of science that have emerged so far. First, paradigm change induces change in both the concepts employed by scientists and the vocabulary that they use to express those concepts. Second, the perceptual experience of scientists is influenced by the paradigms that they accept. Third, the effect of paradigm change is so profound that it results in a change in the very world in which scientists pursue their research. Finally, rather than a unique scientific method, there is variation in methodological standards from one paradigm to another.

Taken as an ensemble, these four themes form the basis of Kuhn's most controversial claim about science. This is the thesis of the 'incommensurability' of competing paradigms (pp.148–50).

In the early stages of a scientific revolution, a debate breaks out between advocates of a new candidate paradigm and defenders of the entrenched paradigm. Kuhn claims that such alternative scientific paradigms are incommensurable with each other. Scientists working in different paradigms employ different standards of theory-appraisal and pursue different sets of research problems. They use different vocabulary, and employ shared vocabulary in different ways. When they observe the world, their observation is subject to the influence of their paradigm. In some sense, they even perceive different worlds. As a result, scientists from rival paradigms encounter difficulties in communicating with each other. Given the difference in vocabulary, they may even be unable to directly compare what one paradigm says with what the opposing paradigm says. Moreover, given the paradigm-dependence of evaluative standards, it is impossible to appeal to neutral standards that may arbitrate the dispute between conflicting paradigms. Kuhn sums all this up by means of the claim that competing paradigms are incommensurable.

The thesis of incommensurability leads to a number of extreme consequences that have been of particular concern to philosophers of science. If neither shared observation nor neutral standards exist, there would seem to be no objective basis for choice between competing paradigms. Further, if methodological standards depend entirely on paradigm, the inevitable result is a relativism of rational theory-choice to arbitrarily chosen paradigm. The situation is aggravated if scientists are unable either to communicate between paradigms or to compare their alternative viewpoints. For it would then be impossible to base the decision to adopt a paradigm on an informed analysis of the alternative paradigm that one rejects. Finally, Kuhn's talk of scientists practising their trade in different worlds suggests an idealist view of science on which science fails to have contact with an objective reality.

Kuhn's legacy

When Kuhn died in 1996, he left the field of history and philosophy of science a different field from the one he entered. Trained as a physicist, Kuhn moved into the history of science when he was invited to teach a general education course at Harvard in the early 1950s. These were formative years for the history and philosophy of science. For much of his subsequent career, Kuhn wrote and taught primarily as a historian of science. But, as is apparent from *Structure*, he had philosophical aspirations. In time, Kuhn won recognition within the philosophy of science. At the close of his career he even held a position as professor of philosophy of science at MIT. Where the philosophy of science was once detached from the history of science, it is now standard for philosophers of science to work closely on topics in the history of science. This shift in approach owes much to Kuhn's efforts to bring a historical perspective to bear on the question of the nature of science.

No general survey of twentieth century philosophy of science could fail to mention Kuhn or come to grips with his influence. Questions about observation, scientific rationality, theory-change and the meaning of scientific terms all bear his imprint. Whole new disciplines, such as the sociology of scientific knowledge, derive their inspiration from his work. Yet few of Kuhn's positive philosophical ideas survive unscathed.

Among historians and philosophers of science, the term 'paradigm' is rarely, if ever, employed to describe any actual component of science. The term is part of popular speech. But it is not a technical term employed by specialists in the analysis of science. In part, this is due to the elasticity of Kuhn's original use of the term. In an attempt to develop a more precise vocabulary, Kuhn himself abandoned the term. In the 'Postscript', he spoke instead of 'exemplars' and 'disciplinary matrices'. Still later, he tended to speak simply of theories, where once he might have used the term 'paradigm'. In his final work, he often used the term 'lexicon' to describe an integrated complex of theoretically defined terms. Instead of the incommensurability of paradigms, the locus of incommensurability had become the lexicon.

No doubt, most historians and philosophers of science agree on the importance of traditions of research within the history of science. But few would accept Kuhn's account of these traditions in terms of paradigms in an unqualified form. In *Structure*, Kuhn wrote as if a science is normally dominated by a single paradigm that is immune to challenge or change. This errs on two fronts. First, the normal state of science is more complex. Occasionally, a single theory may come to dominate a field of science. But often there is a plurality of competing theories, whose relative fortunes wax and wane as they meet with varying degrees of success. Second, Kuhn tended to treat paradigms as unchanging monoliths, to be applied by scientists, but not revised. But scientific change is more piecemeal than this suggests. Rather than being applied as inflexible wholes, theoretical structures may be modified, divided and combined in ways not consistent with the monolithic character of paradigms.

Kuhn's treatment of the decision that scientists make between competing paradigms initially seemed to many to be a wholesale assault on the rationality of science. In part, this rested on a misunderstanding of Kuhn's main point. Kuhn did not wish to show such decision-making to be necessarily irrational. His point, rather, was that the superiority of a paradigm is unable to be unequivocally demonstrated in a way that would convince all parties to a debate between rival paradigms. For, as he wrote in the 'Postscript', 'there is no neutral algorithm for theory-choice' (p.200). To put the point somewhat differently, there are limits on what can be established by evidence and methodological considerations in the context of interparadigm debate. But it is not just that Kuhn's original point was not initially well understood. Equally, Kuhn's denial of a unique scientific method and fixed standards now seems less radical than it once did. As Alexander Bird has argued in his recent book, *Thomas Kuhn* [2], current naturalistic epistemological theories allow variation of methodological standards without the implications of irrationality and relativism to which Kuhn's original discussion seemed to lead.

As for the issue of incommensurability, little remains of the original idea of incommensurability due to semantic, perceptual and methodological variation between paradigms. In Kuhn's later work, some of which may be found in the posthumous volume, *The road since structure* [3], incommensurability has become a narrowly semantic issue. Incommensurability is a relation of localized translation failure between the special vocabulary of theories. The special vocabulary of a theory is a set of technical terms introduced within the context of the theory. These terms form an integrated complex of terms which are defined in relation to each other. Because the same semantic relations that hold within one theoretical complex of terms do not obtain within alternative theories, it may not be possible to exactly translate a term from one such complex by means of terms of another such complex. But while such terms may not be translatable between theories, Kuhn denies that this necessarily leads to problems of mutual understanding. For scientists may understand the content of an opposing theory, even if it cannot be translated into the language of a theory that they accept. (For further

discussion of this topic, see my *Rationality, relativism and incommensurability* [4], as well as the volume edited by Hoyningen-Huene and myself, *Incommensurability and related matters* [5].)

Finally, the question of 'world change' is an interesting case. The dominant tradition in Anglo-American analytic philosophy is still realist in the sense of adherence to the existence of an objective reality, to which we have at least some epistemic access. For this tradition, talk of 'world change' can be at most a strained metaphor. But both within analytic philosophy and without, there are strong antirealist currents. Philosophers have proposed various arguments against the idea that truth is an objective correspondence relation between language and reality, as well as against the idea that reality may be conceived as entirely independent of human cognition. For those of an antirealist temperament, Kuhn's talk of 'world change' may seem less outlandish than it once did.

References

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The structure of scientific revolutions (1962)
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An obstruction to wisdom and a recipe for science

The structure of scientific revolutions by Thomas Kuhn was the most influential book on the nature of science in the second half of the 20th century. It has sold a million copies in 20 languages since it first appeared as the final instalment of the logical positivists' International Encyclopedia of Unified Science (IEUS) in 1962. A peculiar feature of the book's reception is that it has been most popular with humanists and social scientists, even though Kuhn thought he had nothing of interest to say to them – except that their knowledge pursuits failed to fit his cyclical model of normal and revolutionary science. In striking contrast, Kuhn singularly failed to persuade the physicists in whose subject he was professionally trained and whose history provides the primary data for his model. Indeed, I believe that future historians will treat the massively unintended significance of *Structure* as symptomatic of the West's neurotic relationship to science in the 20th century. Nevertheless, it must be admitted that *Structure* is a remarkably self-exemplifying text, since Kuhn correctly – if again unwittingly – identified the key psychosocial mechanism responsible for his own book's success.

If, as Socrates believed, the recognition of one's own ignorance is the first step on the road to wisdom, then *Structure* has proven to be an obstruction. The book did much to establish the relevance of the history of science to an understanding of contemporary science, but without encouraging its readers to check the accuracy or applicability of Kuhn's particular version. As someone who has recently published a comprehensive critique of *Structure* [1], I am frequently faced with readers who claim to have found in Kuhn's account of paradigm formation a compelling model for their own disciplines. Typically, these readers come to *Structure* with a rather patchy and personalized understanding of the histories of their own disciplines. More importantly, they have no other general account of the history of science with which to compare Kuhn's account or, when they do, the alternative account is a highly simplified and judgemental version of positivism that casts their own disciplines in a harshly negative light. Not surprisingly, they quickly embrace Kuhn and never look back. In this respect, it actually helps that Kuhn's model is anchored in physics, since the same applies to the positivist model, and so it becomes easy to conclude that Kuhn marks a significant improvement.

Kuhn appeared to provide a salutary recipe for turning one's activities into a science. One only had to obtain agreement on a common theoretical and methodological framework within which permissible problems and their solutions are clearly defined. In a word: a paradigm. In contrast, positivism in its vulgarized form seemed to demand that a science also exercise instrumental control over some part of the natural world. This additional requirement proved to be a step too far for most humanists and social scientists. Not only did they lack the conceptual and material resources to render their inquiries 'instrumental' in the relevant sense, but many also objected to the moral implications of such a world-view when applied to human beings. As it happens, the latter turns out to be not so far from Kuhn's own interest in defining progress in science by referring only to criteria that the scientists themselves have designed. To many of Kuhn's philosophical opponents, this move smacked