# "The Structure of Scientific Revolutions" and its Significance: An Essay Review of the Fiftieth Anniversary Edition

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# *The Structure of Scientific Revolutions* and its Significance: An Essay Review of the Fiftieth Anniversary Edition*

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ABSTRACT

Kuhn's *Structure of Scientific Revolutions* is one of the most cited books of the twentieth century. Its iconic and controversial nature has obscured its message. What did Kuhn really intend with *Structure* and what is its real significance?

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## 1 Introduction

Thomas Kuhn's *The Structure of Scientific Revolutions* ([1962]) is in many ways an unusual and remarkable book. It has a strong claim to be the most significant book in the philosophy of science in the twentieth century,

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even though it was written by a man who was not, at that time, a philosopher, describing himself as 'an ex-physicist now working in the history of science' (Kuhn [1959], p. 225). Kuhn's intentions for his book were nonetheless philosophical; yet, its effects have been felt widely beyond philosophy of science. The fiftieth anniversary of the publication of *Structure* provides an appropriate moment to consider the true significance of Kuhn's book.[1]

Thomas Kuhn was trained as a physicist, gaining his PhD from Harvard in 1949. He thereafter turned to the history of science, having taught on James Conant's class at Harvard in history of science for undergraduates in the humanities, part of the General Education program. Kuhn's research interests focused in particular on eighteenth-century matter theory, the early history of thermodynamics, and the history of astronomy. The latter led to his first book, *The Copernican Revolution* ([1957]). Some years previously, Kuhn had been invited by Charles Morris (following a suggestion by I. Bernard Cohen, who was professor of the history of science at Harvard) to contribute a volume on the history of science to the *International Encyclopedia of Unified Science* (Baltas *et al*. [2000], p. 292). The latter was a series of monographs inspired by Otto Neurath, whose principal contributors and supporters were the logical positivists, with volumes on the foundations of the various sciences by Carnap, Nagel, Frank, Neurath, and Hempel, among others. Ernest Nagel was a friend of Kuhn's and assisted Kuhn with comments on a draft of *Structure* (Kuhn [1962], p. xii). Nagel published his influential *The Structure of Science* in 1961, and it is plausible that discussion with Nagel is a principal source for Kuhn's understanding of contemporary positivist philosophy of science—the similarity of the titles of their books may be no coincidence.

## 2 The Central Ideas of Structure

When a book contains so many ideas that have been taken up and developed in myriad and sometimes contradictory ways, any attempt to specify the central ideas is controversial. Nonetheless, here are what I take to be the two key ideas of *Structure*:

(1) *The pattern of scientific change*: The historical development of a mature science shows a pattern: normal science, crisis, extraordinary science, new phase of normal science, etc. Normal science is conservative, with scientists building on rather than questioning existing science. Normal science can be characterized as puzzle-solving, where the puzzles to be solved are those raised by the current

[1] In the following I am referring only to the first edition of *Structure* unless otherwise specified. In the second edition ([1970]) a lengthy 'Postscript 1969' is added. The latter does clarify important issues in *Structure* but it does also add claims and arguments. In this fiftieth anniversary review, I am interested in what Kuhn had in mind when he published his book in 1962.

scientific tradition. For example, in nineteenth-century chemistry, normal science would include the discovery of new elements and the calculation of their atomic weights, working out the chemical formulae and structure of organic compounds, and finding new methods for calculating constants such as Avogadro's number with greater accuracy. The existing tradition of good science made such tasks worthwhile and also gave scientists the tools with which to carry out such tasks. Not every puzzle can be solved in the course of normal science; such cases are *anomalies*. Crisis occurs when a sufficient weight of particularly significant anomalies causes scientists to question the capacity of the current tradition to solve those anomalous puzzles. Extraordinary science is revolutionary; that is, some significant component of the existing tradition (for example, a key theoretical commitment) is jettisoned and replaced in the expectation that the revised practice will solve many of the crisis-precipitating anomalies and provide a fruitful platform for future research (i.e. will generate new puzzles and provide the means of solving them). For example, with the discovery of the structure of the atom, the theoretical significance of atomic weight was reduced and replaced by the atomic number of the element and the mass number of the isotope: discovering the isotopes of an element and their properties became a new set of puzzles that had not existed before.

(2) *Paradigms*: The pattern outlined above is best understood and explained by the existence of paradigms. The key sense of 'paradigm' is *exemplar*: an exemplary instance of puzzle-solving in that discipline that provides a context and a model for future puzzle-solving. Paradigm is also used to refer to a *disciplinary matrix*, a set of commitments shared by practitioners of a particular scientific field, including a special vocabulary and established experimental techniques, as well as accepted theoretical claims. The central commitment of a disciplinary matrix is to the shared exemplars; by agreeing on the exemplars, the field thereby agrees on the other components of the matrix. Exemplars are transmitted and inculcated by the training of young scientists. Training with exemplars allows scientists to see the world in a certain way that enables them to solve scientific problems in ways analogous to those in the exemplars. Normal science is thereby built on and built by the exemplars. Crisis occurs when science modelled on the exemplars fails to answer key puzzles. Revolutions come about when exemplars are replaced by new exemplars; such revisions to exemplars will bring about other changes in the disciplinary matrix.

The claims (1) and (2) together can be thought of as analogous to a scientific theory: the first identifies and describes a pattern (as do Kepler's laws), and the second provides an explanation for the pattern (as does Newton's theory of gravitation). The descriptive claim (1) is historical, whereas the explanatory claim (2) is both sociological and psychological. Claim (2) is psychological in that it maintains that the mechanism of scientific cognition is one of matching puzzles and proposed solutions to pre-existing exemplary puzzles and their solutions. It is also sociological in that it makes a claim about certain shared commitments by members of the scientific discipline. The combination of the sociological and the psychological reflects Kuhn's interest in pedagogy: the community's commitments are transmitted by the training of young scientists; that training draws on the efficacy of learning with exemplars (Kuhn [1959]; see Isaac [2012]).

## 3 The Philosophical Targets of *Structure*

In the Preface and Introduction to *Structure*, Kuhn describes the aims of his book. He did not intend *Structure* to be a work of philosophy per se; as he says, it is historically oriented. But he also makes it clear from the first sentence of its introduction that the book is motivated by a concern to correct a mistaken picture of science ([1962], p. 1): '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'. That image is one drawn from scientists and their training, and also from philosophy of science ([1962], p. v). Seeking to bring about a 'decisive transformation' in that image, Kuhn's claims (1) and (2) challenge its philosophical presuppositions, particularly those promoted by logical empiricism. This section discusses the philosophical targets of *Structure*.

(i) The first target is a *simple cumulative conception of scientific progress*. In its most simple form, that conception says that science adds new truths to a stockpile of older truths. Although no one would have thought that all the history of science is like this, many scientists and philosophers may have thought that mature sciences do show precisely that kind of accumulation, with exceptions being rare, usually peripheral, and liable to be eliminated over time. A slightly more sophisticated view may accept the existence of falsehood even in modern science, but maintains that this is because certain theories are not perfectly true, but are instead good approximations to the truth; nonetheless such theories are developed and improved, making them more accurate than their predecessors; science progresses as these theories get ever closer to the truth.

Kuhn's first claim puts this conception under pressure. Science cannot show cumulative progress if it is subject to regular upheavals in which central commitments have to be revised. The cumulative conception can handle erroneous science: such cases can be dismissed as exceptional and uncharacteristic if the case is from an immature field, if it is not central, if it is a matter of approximation, or if it is the product of a failure of rationality by an individual scientist. That is why it strengthens Kuhn's case to point to a pattern that has a rationale. It would be rather less effective to merely point to historical cases that show revisions to scientific beliefs; for then opponents could argue that such cases are among the admissible exceptions. By showing that revolutions are parts of a pattern, the natural life cycle of a scientific discipline, they cannot be dismissed as exceptions; they are central to the nature of science.

Kuhn gives another, distinct reason for rejecting the cumulative picture: incommensurability. If the new discovery, $S'$, is to be a cumulative improvement on an older scientific belief, $S$ (for example, by being a better approximation or by adding detail), then $S'$ and $S$ must say the same sort of thing. If the languages of $S'$ and $S$ use the same terms but with different meanings, it is at best difficult to see how $S'$ straightforwardly adds to or builds on $S$.

(ii) The second target concerns *the means by which scientific beliefs are produced*. The cumulative conception of science is principally historical in nature and not strictly a philosophical claim. But it is a consequence of two claims: (a) the empirical claim that science is by and large rational, and (b) the philosophical claim that scientific rationality consists in following certain rules that produce (probable) truth. Repeated application of the rules should lead to truths to be added to the stock. (The exceptions mentioned above are easily accommodated.) For simplicity, we may call the set of these rules, 'the scientific method'.[2] What exactly the scientific method is, is the subject of philosophical investigation—it is assumed that its nature is knowable *a priori*.

By undermining the cumulative conception of scientific progress, Kuhn's cyclical pattern of scientific change is evidence against the existence of the scientific method in the sense articulated above: an *a priori* method of gaining truth. Kuhn does provide an alternative account of the process of cognition in science. That is the account based on the function of exemplars in (2). I shall discuss this account in more detail below. For now I mention two features that set it apart from the scientific method:

[2] This is my expression not Kuhn's. James Conant, in his Introduction to the *Harvard Case Histories in Experimental Science*, which he developed with Kuhn and Leonard Nash, is explicit: 'A study of these cases makes it clear that there is no such thing as the scientific method' (Conant [1957], p. x). Kuhn himself is less explicit, but the implications of what he says, that there is no unique and *a priori* way of gaining truth in science, is clear ([1962], p. 1; [1963], p. 348; [1980], p. 185).

- Its nature and efficacy are not to be inferred *a priori* but are discovered empirically (in particular by psychology).
- Cognition by exemplars permits scientists rationally to come to divergent opinions when given the same evidence.

(iii) A third target is a certain species of *scientific realism*: a realism that says that science aims at truth and employs methods that achieve that aim. The logical positivists were realists in this sense (although there were anti-realists regarding the unobservable). Kuhn attacks this target in two ways. First, the particular version of this view that says that the truth-tropic methods are 'the scientific method' is rejected for the reasons discussed above. Secondly, science does not aim at truth at all. A discipline aims at solving scientific puzzles. In *Structure*, Kuhn puts forward an evolutionary picture of scientific change. Scientific progress does not consist in getting closer to the truth, just as biological evolution does not require a lineage to get closer to some perfect, optimal version of the species. Yet that does not mean that there is no progress: later members of a lineage are better than their nearby predecessors in certain respects (modern giraffes are taller than their ancestors). Likewise, as paradigms change, their puzzle-solving power grows, and the number and range of the puzzles we can solve grow also.

(iv) A fourth target is *the distinction between context of discovery and context of justification*. This distinction originates with Reichenbach ([1922]), though it has predecessors in the work of others (for example, Herschel [1830]). Not all logical positivists and empiricists would have held that there is a scientific method, as discussed in (ii) above, if that implies a procedure for generating truth. For significant new theories have to be produced by a creative, imaginative process that cannot be reduced to *a priori* rules. But all would have agreed that given a theory, the assessment of the theory against the evidence is an *a priori* logical process (exemplified by Carnap's inductive logic). The two components, theory production and theory evaluation, fall into the 'context of discovery' and the 'context of justification' respectively, and are to be investigated by psychology in the first case and philosophy and logic in the second. Kuhn's account of scientific cognition brings discovery and justification much closer, for both are achieved by the function of exemplars. Exemplars set the standards for evaluating attempts to solve scientific problems—the proposed solution should bear the same relationship to the problem as the exemplary solution does to the exemplary problem. And they also provide the means of discovering the solution, as training with exemplars enables the scientist to see the world in terms of the kinds of puzzle and solution provided by the exemplars. So the shared training with exemplars provides the means of both finding promising solutions and evaluating the solutions proposed. In rejecting the distinction, Kuhn thereby psychologizes justification, whereas all logical empiricists agreed that justification is independent of psychology, being entirely logico-philosophical.

## 4 Interpreting and Misinterpreting *Structure*

Kuhn's book is multifaceted, involving history, psychology, philosophy, and sociology. For a philosophical audience in particular, its approach, which we would now call 'naturalistic' in its use of empirical science, was unusual. Kuhn's arguments, especially as regards philosophical consequences, are often more implied than stated explicitly. And key terms, such as 'paradigm', are used in multiple ways that appear to be ambiguous. Consequently, it is not surprising that *Structure* has been read in many ways. Some readings of *Structure* are misleading or simply not correct, and in this section I address what I take to be common misconceptions surrounding that book.

### 4.1 Naturalism

I start with the strong current of naturalism that pervades *Structure*. Among most commentators this has been largely ignored, though it has recently been the subject of discussion. To the extent that it was discussed, it was misunderstood and generated hostility. Above I remarked on the fact that Kuhn was willing to draw on research in psychology and to a lesser extent in sociology. It was this combination that led to Lakatos's ([1970], p. 178) condemnation of Kuhn's theory as making scientific change a matter of mob psychology. It may seem odd now that one should not consider psychology when thinking about change in scientific belief. Yet one effect of a sharp distinction between the context of discovery and context of justification (see above) is to relegate psychology more or less entirely from the explanation of the adoption and changing of belief. For then the philosophically interesting question about why a belief was adopted or rejected falls within the context of justification, and the latter was conceived of in aprioristic terms—a matter of obeying the correct logic of inference and confirmation. Only deviations from the latter would need psychological explanation. That is, rational belief requires no psychological explanation; if belief does require psychological explanation, then it is irrational. So Kuhn's appeal to psychology in explaining all scientific belief was seen as indication of his attack on the rationality of science. It was not that at all; rather, it was part of his attack on the distinction between context of discovery and context of justification.

The importance of psychology to *Structure* is much under-appreciated. Although I linked psychology and sociology above, Kuhn was clearly far more interested and better read in psychology than sociology. He makes only two references to sociological research in *Structure*, whereas the references to psychology outnumber those to any other discipline other than history. Kuhn mentions the work of the gestalt psychologists of the Berlin School, Benjamin Whorf, Jean Piaget, Kuhn's erstwhile colleagues at Harvard, Jerome Bruner and Leo Postman, half a dozen currently less widely known scientists, as well as the mathematician Jacques Hadamard, who wrote about cognition and creativity in mathematics. The research of these psychologists, which may itself be seen as providing exemplars for Kuhn's thinking about cognition in science, concerns the nature of perception and, in particular, how perceptual experience can be moulded by training and experience. These set up expectations that cause us to perceive (literally) the world in certain ways. It follows that changes in experiences can bring about different perceptions, and so also that the same object may look different to various observers, depending on their previous experiences. A simple example of one object leading to more than one perceptual experience is a multistable image, such as the Necker cube or the duck-rabbit. Such images may be seen in more than one way, and one can be cued to see them first in one way, then in another. Such images played an important role for the gestalt psychologists (Kurt Koffka, Max Wertheimer, and Wolfgang Köhler) because such images supported their view that perceptual experience must be thought of holistically—the experience of the Necker cube cannot be thought of as being composed of simpler experiential atoms, for if it were, we could not differentiate the two different types of experience of the cube, which seem to have the same atoms. So the same perceptual stimuli bring about different experiences, and the gestalt psychologists proposed several principles by which the brain does this, including the law of past experience. As Kuhn ([1970], p. 193) puts it, 'the same stimulus can produce very different sensations [...] the route from stimulus to sensation is in part conditioned by education'.[3]

Such conclusions are important for two reasons. First, they undermine the logical positivists' belief that observation can provide a neutral basis for theory comparison. Not only, as Kuhn points out, is observation rather more complex and difficult than mere perception, both observation and the perception it involves are dependent on previous experience—i.e. on training with paradigms. Secondly, they provide a model or exemplar for scientific cognition beyond the simply perceptual. The anomalous playing card experiments of Bruner and Postman ([1949]) described in 'On the Perception of Incongruity: A Paradigm', which Kuhn ([1962], pp. 62–3) discusses in some detail, are instructive. Bruner and Postman exposed subjects to images of playing cards for short durations. Most of the cards were familiar, but some

[3] Although this comment appears in the postscript to the second edition, it is clearly an elaboration on the ideas in the first edition rather than a new line of thought.

were modified to be anomalous, such as a red six of spades or a black four of hearts. Subjects, who were asked to describe what they had seen, would, for short exposures, categorize the cards wrongly, assimilating the card to a familiar kind (for example, seeing a red spade as a heart); at longer exposures the subjects become aware of something wrong but were not always able to say what was wrong, or would use a description between what they expected and what was there (brown or olive). At the longest exposures and after seeing a few anomalous cards most subjects were able to describe them correctly, but some continued to find the cards perceptually and emotionally disturbing. This experiment provides clear analogies for Kuhn's understanding of anomalies in normal science, crisis, and revolution: they may at first be ignored, being assimilated to familiar categories, then they produce cognitive dissonance and a sense of something being wrong, and finally they may become accepted and understood for what they are, once they are familiar and our theoretical framework has been adjusted to make sense of them. However, Kuhn speculated that there may be more than just analogy here. For being able to perceive and describe the cards correctly is an act of cognition: an act of placing a new stimulus in a category provided by past experience, recognizing a card as of the same kind as previously experienced cards. Kuhn's account of the operation of paradigms-as-exemplars in normal science is just the same. Familiarity with exemplars breeds expectations that cause us to see the world in certain ways—only 'see' here is now not purely perceptual: the scientist sees a new scientific problem as similar to an exemplar, an act of cognition that will allow her to solve that problem. An anomaly is a scientific problem for which training with exemplars has not provided any such conditioned cognitive response that leads to a solution. As a result of a revolution, new exemplars are introduced, and problems that were anomalous become familiar, problems that post-revolutionary scientists can recognize as requiring solutions of the kind found in the new exemplars. Put simply, Kuhn is asking us to consider that the key cognitive component of science is not the application of logic or some other abstract form of rationality, but is another entirely familiar kind of cognition, the conditioned ability to recognize one thing as similar to another; scientific thinking should be thought of as having more in common with, say, bird-spotting than performing an Aristotelian syllogism.[4]

## 4.2 World-change

'When paradigms change, the world itself changes with them' and 'after a revolution scientists are responding to a different world' (Kuhn [1962],

[4] See Margolis ([1987]) for an extended account of the centrality of pattern recognition in scientific reasoning.

p. 111). Such partial quotations may suggest that Kuhn is proposing some kind of idealism or social (metaphysical) constructivism—the world is just what the community believes it to be and when the community's beliefs change, the world changes. It is, however, entirely clear that Kuhn does not intend this at all, and that his use of 'world-change' is metaphorical: the chapter in question is entitled 'Revolutions as Changes of World *View*' (my emphasis), and the quotations are prefaced respectively by 'the historian of science may be tempted to exclaim that...' and 'we may want to say that...'. Nonetheless, as Sismondo ([1992]) points out, a constructivist reading of Kuhn is persistent. One reason may be that the constructivist caricature made Kuhn an easier target for those who took his position to be that science is irrational. Another may be that the emphasis laid by Kuhn on the idea of world-change demands an answer to the question, 'what did Kuhn have in mind by "world-change"?', to which the constructivist answer seems the most straightforward.

The correct answer, in my opinion, to that question is *psychological*. Kuhn's first mention of world-change comes when he articulates the defining features of a scientific revolution: 'each [revolution] transformed the scientific imagination in ways that we shall ultimately need to describe as a transformation of the world within which scientific work was done' ([1962], p. 6). The possibilities we are able to conceive are limited not only by an individual's innate imaginative capacities but also by the background beliefs and experience he or she has. It is not that Albert the Great or Hildegard of Bingen just didn't believe that humans, insects, and plants have common ancestors; it was psychologically impossible for a medieval thinker to have such an idea. Scientific revolutions change what it is possible for people to think because with a revolution come changes in background beliefs and, thanks to changes in training, what experiences scientists undergo. Furthermore, our experience of the world is not merely sensory, but is heavily coloured by quasi-intuitive emotional and cognitive responses: if one suddenly comes across a poisonous snake, the perception of the snake, fear, and the recognition of it as a threat are all part of one's experience; the last component does not present itself as an inference drawn from the first. Rather, the connection is an intuitive one (possibly innate). Such intuition-like cognitive responses can be learned, and these cognitive habits are part of what is acquired in learning with exemplars. For example, when a physicist or engineer sees the equation $\ddot{x} = -kx$, he or she immediately thinks 'simple harmonic motion' and knows without doing any mathematics that the equation of motion has the form $x = Asin(\sqrt{k}t + \phi)$. Like other habits, good and bad, these responses can be difficult to acquire and to lose. This explains the psychological difficulty scientists may experience in the face of a revolutionary change in a paradigm. For that requires a change in these quasi-intuitive connections, especially among older scientists in whom such habits of thought are more entrenched. In summary, a change in paradigm can bring with it a range of important psychological changes that have cognitive (and emotional) consequences, affecting what a scientist can think or imagine, how he/she immediately responds to a stimulus, and what he/she intuitively infers from facts and stimuli. It is these psychological changes that Kuhn is referring to with the metaphor of 'world-change', changes that are not captured by thinking (for example, as Carnap might have proposed) in terms of changing probabilities attached to a proposition.

The fact that Kuhn likened the changes brought about by a revolution to a gestalt switch (also to a religious conversion) was a source of considerable critical comment, being held as showing that Kuhn thought that science is irrational. But Kuhn's intention with the parallel is to draw attention to the important psychological character of revolutionary change, in particular the fact that how we experience the world can depend on training, previous exposure, and so forth. In the gestalt images case, 'experience' is perceptual experience, whereas in the scientific case, the scope of 'experience' is broader: the Aristotelian experiences the pendulum in one way, while the follower of Galileo experiences it in another way, for the first habitually conceives of the bob as seeking its natural lowest point in a tortuous constrained way, while the second quasi-intuitively thinks of the bob as almost succeeding in continuing its swinging ad infinitum (Kuhn [1962], p. 120).

Having denied that Kuhn had idealist or constructivist intentions, I now want to mention one interpretation of Kuhn's view that is idealist in nature, albeit of a rather more sophisticated kind than social constructivism. Hoyningen-Huene ([1993]) proposes that we distinguish, *à la* Kant, between a phenomenal world of appearances and the world-in-itself. The Kuhnian twist on Kant is that key features of the phenomenal world change with scientific revolutions—indeed, Kuhn later described himself as a Kantian with moveable categories ([1991], p. 12). So this reading of Kuhn is idealist in a Kantian sense, yet like Kant retains the idea that behind the phenomenal world there is a mind-independent world-in-itself. This is an important way of looking at Kuhn, not least because Kuhn himself endorsed it. Nonetheless, I am not sure it is correct regarding what Kuhn had in mind in 1962, and I suspect that Kuhn was himself influenced by Hoyningen-Huene's giving his earlier thought a (particular species of) philosophical sophistication that it did not really have. Kuhn's naturalistic interest in the cognitive science of how stimuli, background beliefs, and previous experience produce a visual impression is at odds with the Kantian view that the things-in-themselves and their relations to the phenomena are unknowable. Furthermore, the Kantian phenomenal world is a world as presented to us in sensory appearances, and while that meshes with Kuhn's particular focus on the effect of world-change on visual experience, world-change cannot be limited to the latter. It is not especially plausible to say that Aristotle and Galileo have different visual experiences when looking at a pendulum, and it is even less plausible to think in terms of changes in sensory experience when we turn to the relativistic revolution or other more recent scientific revolutions that are highly theoretical in nature and rather removed from the objects of experience. It may be that there are interesting ways of reconciling the Kantian interpretation with naturalism and a broader notion of what the phenomena are. But to attribute such a view to *Structure* is to read into that work a philosophical viewpoint that is not there, even if Kuhn did adopt something of the sort later on.[5]

### 4.3 Incommensurability

Kuhn's introduction in *Structure* of the concept of incommensurability, alongside Feyerabend's use of the concept, was an important moment in intellectual history. Incommensurability became the focus of Kuhn's philosophical thinking in his later work, as well as the point of contention for many critics. Kuhn later regarded incommensurability as a defining feature of scientific revolutions. Yet those facts should not lead us to overemphasize the significance of incommensurability in *Structure*. It is an important idea but, unlike the paradigm concept, it is not a key idea (in the sense of a keystone that plays a crucial structural role). This is reflected in the fact that Kuhn uses the terms incommensurable and incommensurability nine times in the first edition of *Structure*, which contrasts with hundreds of uses of 'paradigm'. The principal philosophical arguments of *Structure* do not require invoking incommensurability. The argument against the cumulative conception is bolstered by appeal to incommensurability, but can get by on the basis of the cyclical pattern of science alone.[6] And in *Structure*, Kuhn does not include incommensurability as a defining characteristic of scientific revolutions ([1962], p. 6). He says that the normal-scientific traditions either side of a revolution are *often* incommensurable (implying that they are not always incommensurable) ([1962], p. 103).

Kuhn's few and scattered references to incommensurability in *Structure* do not amount to a systematic theory of incommensurability. Various entities may, according to Kuhn, be said to be incommensurable: normal-scientific traditions, solutions to problems, standards, vocabulary, apparatus, paradigms, worlds of research, and viewpoints. While subsequent discussion of incommensurability has focused on one of these elements above others—

[5] Although Kuhn did later talk in Kantian terms, he made no systematic attempt to articulate what this really amounts to.

[6] I surmise that Kuhn later became less convinced by the cyclical pattern, and transferred the weight of the argument against the cumulative conception to the argument based on incommensurability.

vocabulary and incommensurability of meaning—Kuhn's use of incommensurability in *Structure* places greater emphasis on the incommensurability of viewpoints in a way that relates to the idea of world-change discussed above. Kuhn does not give a definition of incommensurability. The original use of the term itself refers to the lack of a common unit that would allow both the side of a square and its diagonal to be measured in integer multiples of that unit. But that gives at best an indication of what Kuhn has in mind. The lack of a definition need not be an oversight. In science, a definition is often the outcome of discovery, not its precursor: a phenomenon is identified and referred to using a term, and once the nature of the phenomenon is identified (often its cause), we can provide a definition of the term. Kuhn is aware that there are differences between different periods in the history of a science that are not captured by saying that at $t$, the community, $C$, believed $p$, whereas at $t'$, their successors, $C'$, denied $p$ and held $q$ instead. That is straightforward incompatibility. Incommensurability goes beyond this in that $C$ and $C'$ fail in an important way to be able to see the world from the other's point of view. Kuhn experienced this himself in reading Aristotle's physics: it was not that Aristotle believed things that we do not, but that Aristotle seemed to be a bad physicist, using arguments that made no sense to Kuhn. Kuhn was eventually able to see how this assessment was wrong and that Aristotle's viewpoint did have a coherence he was not initially able to appreciate. Kuhn proposes that the same kind of phenomenon can be identified in (at least some) cases where research traditions are contiguous or even overlapping in time, when a scientific revolution intervenes. So, in *Structure*, the thesis that revolutionary changes are often accompanied by incommensurability is much the same claim as the claim that revolutionary changes often involve world-change. Precisely because Kuhn is unable, as he himself admits, to give a full account of what world-change really is, he is not in a position to define incommensurability.

That said, 'world-change' and 'incommensurability' are not interchangeable terms. 'World-change' focuses on the psychological consequences of a scientific revolution, whereas 'incommensurability' focuses on the corresponding epistemological consequences. Incommensurability reinforces Kuhn's refutation of the thesis that science shows cumulative progress. One version of that thesis holds that later theories may improve on the scope and accuracy of earlier theories: the fact that Newtonian mechanics is a limiting case of special relativity as velocity approaches zero illustrates this. Kuhn demurs on the ground that the meanings of the relevant terms have changed as a result of the revolutionary change. The claim is contentious. Does change in meaning (sense) rule out an accumulation of truth? And why should we agree that meaning has changed? Kuhn gives no argument, but it is easy to reconstruct one. According to the double-language model promoted by his friend Ernest Nagel, the meanings of theoretical terms are given holistically, by their role in the relevant theory. A revolutionary change in theory will imply a corresponding change in the meanings of the terms used in the theory. That model did anchor those theoretical meanings in an unchanging observation language. Yet on Kuhn's account of revolutionary change, change in theory is part of a larger set of changes, which will include change in how scientists see the world, both in a literal and a metaphorical sense of 'see'. That is a plausible reconstruction of an argument that Kuhn might have had in mind. Kuhn himself says little. The most we are able to say on the basis of *Structure* alone is that because revolutions involve significant changes in world-view, it is at the very least not obvious and straightforward that scientists after a revolution are adding to the truths of their predecessors or improving the accuracy of their theories. Even if Kuhn does not present us with an argument that incommensurability means that this does not happen, his reference to incommensurability is a challenge to the received view to show that it does.

### 4.4 Progress and the nature of revolutionary change

Above I characterized the pattern of change in Kuhn's view as cyclical—which would accord with the original notion of revolution, as a change that brings us back to a starting point (as when a wheel or planet makes a revolution). If that were the case, then a scientific revolution would lead to science being placed in a completely new starting place. That place would be different from, but not better (or worse) than, where it was after some earlier revolution. Science starts completely afresh. Similarities to what went before are superficial and illusory.

Kuhn does not, however, say anything that implies that revolutions are such drastic changes. He lists three characteristics of scientific revolutions ([1962], p. 6):

- The community rejects a time-honoured theory for one incompatible with it.
- There is consequently a shift in the scientific problems on which scientists can work.
- There is a transformation of the scientific imagination.

These features do not imply the kind of drastic change under consideration. It is true that each implies some degree of incommensurability. As mentioned, theory change (it may be argued) can lead to meaning change. A shift in scientific problems can lead to incommensurability on Kuhn's problem-solving account of progress. And a transformation in the scientific imagination, a world-change, will also induce incommensurability, as discussed above.

If incommensurability were a particularly strong phenomenon, implying no comparability with what went before, then a view of revolutionary change as root-and-branch change would be justified. However, while Kuhn might be criticized for not saying more about incommensurability and what it involves, nothing he says in *Structure* implies that incommensurability means total non-comparability. Indeed, what he does say about incommensurability and world-change implies the opposite. For example, he gives the phenomenon of gestalt-switches as an analogy for world-change. Such changes provide new views, but the new views can be compared with the old views and have features in common—we can still identify eight vertices on the Necker cube, whether we see it as facing down and to the left, or up and to the right. Likewise there are continuities and points of comparison between pre- and post-revolutionary science. Furthermore, these allow us to say that the new science is an improvement on the earlier science—there is progress through revolutions, and Kuhn devotes the final chapter of *Structure* to showing how that can be.

As science aims at solving scientific puzzles, progress, according to Kuhn, is a matter of science increasing its ability to solve such puzzles. During normal science, that progress is unproblematic—a field just accumulates solutions to puzzles. Revolutionary science is of course the difficult case, as the replacement paradigm rejects the earlier paradigm's puzzle solutions. It does not, however, reject all the previous puzzle solutions, just some of them. Because science is in the business of solving puzzles, that rejection is a loss (sometimes known as Kuhn-loss). Therefore, for scientists to tolerate that loss, the new paradigm must make up for that loss with the promise of even greater compensating puzzle-solving power. In particular, the new paradigm should in the best cases be able to solve the crisis-inducing anomalous puzzles that the earlier paradigm failed to solve. Kuhn points out that in its early stages, the new paradigm may not have in fact achieved this. That is one reason why scientific revolutions are contentious: it need not be clear that the new paradigm will solve the anomalous puzzles as well as provide fruitful resources for solving new puzzles. Nonetheless, in the long term, a successful replacement paradigm will justify the Kuhn-loss with an abundance of new and proven puzzle-solving power. In that case, we can say that there has also been progress, in that there is an overall net gain in puzzle-solving power, even though that has not taken place in a straightforwardly cumulative way.

As we have mentioned, Kuhn's conception of progress is evolutionary: scientific progress is not a matter of getting closer to the truth but rather a process of improving the capacity of the disciplinary matrix to solve scientific puzzles in the given scientific environment. Because all change is made with the aim of making puzzle-solving progress, whether incremental in normal science or revisionary in extraordinary science, there is long-term growth in the range of the puzzles a science can address and in the precision with which it does so. The appropriate picture, therefore, of scientific change, according to Kuhn, is not so much the cyclical one I initially outlined, but is a helix, where each instance of the cycle has greater puzzle-solving power than its predecessor.

### 4.5 Relativism, rationality, and realism

One of the commonest charges directed against Kuhn is that he is a relativist. Although there is some substance in the charge, Kuhn's relativism is often misleadingly caricatured: all evaluation is relative to a paradigm; therefore, there is no paradigm-independent means of evaluation, and in particular there is no means of evaluating paradigms (for example, Weinberg [1998]). If that were the case, then there would be no progress through revolutions and the outcomes of revolutions would not be decided by factors internal to science. This may be seen as part of an interpretation of *Structure* that sees revolutions as maximally radical changes divided from one another by unbridgeable incommensurability, driven by social and political forces. But, as we have seen, Kuhn does think that there is progress between revolutions and that their outcomes are determined by science itself. The central demand on science is that it solves puzzles, and that demand is common to all eras of science. It is true that what counts as a puzzle and what counts as a solution are dependent on the paradigms used at a particular time. But these cannot be chosen at will; even in extraordinary science, the choice of new paradigm is constrained by the previous era of normal science: a candidate paradigm must at least promise to retain most of the puzzle-solving power of the preceding era of normal science and resolve some of the more worrying anomalies that led it into crisis. Incommensurability is only partial. Most puzzles from the previous era must recognizably remain the same puzzles or at least the successors of earlier puzzles. (It may be helpful to think of Kuhn's relativism as implying relativism to a tradition as much as relativism to a particular paradigm within that tradition.) So paradigms are answerable to extra-paradigmatic standards: the general puzzle-solving imperative and the particular implementation of that imperative in a specific scientific tradition at a specific stage of its development.

Furthermore, this kind of relativism only looks to be any kind of threat to rationalism and realism about science if one thinks that these require standards of theory evaluation that apply to all theories at all times. That indeed was the hope of the logical positivists, that an inductive logic could follow deductive logic in providing *a priori* rules of inference. Many now believe that the search for an inductive logic is a wild goose chase; it is at least controversial that rationality consists in following any such rules. Externalism in epistemology allows that rationality and justification can be obtained by using methods that are not known *a priori* to be truth-conducive; for that matter, some versions of internalism hold that rationality and justification can be relative to a set of background beliefs. So, it is far from obvious that Kuhn's relativism implies that science is irrational, given that respectable epistemologies do not yield that verdict on Kuhn's view. While Kuhn was not especially concerned with such epistemological questions, it is, I think, plausible to see him as promoting a conception of scientific rationality that is an alternative to the aprioristic view of the logical positivists. A rational scientist, seeking solutions to her puzzles, is one who models her solutions on exemplars selected because of their previous efficacy in solving scientific problems. (And a successful tradition is one that trains its scientists to acquire such habits of thought.) When seeking a replacement paradigm in extraordinary science such constraints are loosened. But they are not abandoned altogether. The looser constraints provided by the tradition mean that it is possible for rational scientists to disagree about which replacement paradigm is best or even whether a replacement is required; it is Kuhn's opinion that rational individuals can disagree.

Just as Kuhn's relativism does not directly imply irrationalism about science, it does not imply anti-realism. The cognitive processes of science, as Kuhn understands them, involve the modelling of proposed puzzle solutions on exemplary puzzle solutions. Those processes might well be truth-conducive if the exemplars themselves have high truth content. Kuhn himself says nothing on this point, which I raise simply to affirm that Kuhn's position in *Structure* as regards the processes of cognition is consistent with realism. He notes (not quite accurately) towards the end of *Structure* ([1962], p. 170), 'It is now time to notice that until the last very few pages the term "truth" had entered this essay only in a quotation from Francis Bacon'. Kuhn's own view is strictly neutral, and the relativism he does avow is a methodological one: the historian does not need to appeal to the notion of truth to describe and explain the processes of scientific change.

Is Kuhn neither a realist nor an anti-realist then? It is worth noting that in the Postscript 1969 to the second edition of *Structure*, Kuhn is now explicit in rejecting the notion of truth as the realist requires it. However, that is a marked difference with *Structure*, where the stance is neutral and 'non-realist'. If the neo-Kantian reading is correct, then Kuhn would be a metaphysical anti-realist of sorts, but I think that the psychological reading of world-change is the more plausible way to think of *Structure*. The revisionary nature of revolutions is inconsistent with a simple cumulative realism (with regards to truth), and likewise the claims about incommensurability rule out a simple convergent realism. Yet a more sophisticated realism may well be able to accommodate these Kuhnian points.

### 4.6 History and sociology of science

One important effect of Kuhn's *Structure* was to give an impetus to a certain kind of science studies that emphasizes the social and political causes of scientific belief (Geertz [2000]). The classical view, best exemplified by Merton ([1938]), is that socio-historical studies can illuminate the conditions that influence the development of science but that they have little to say on the detailed content of the science produced. For example, there may be sociological reasons (for example, relating to Protestantism) that explain the success of the scientific revolution in seventeenth-century England. But such studies cannot explain why it was an inverse square law that Newton proposed. The latter kind of question can only be answered by studies that are *internal* to science that ask questions such as the following: What was previously believed? What was the evidence? What were the conceptual problems that needed to be overcome? and the like. History of science after Kuhn has frequently taken a more consciously *externalist* line, in looking outside science for the causes of the content of science. For example, Pasteur's germ theory and his rejection of spontaneous generation, and the success of those ideas, are to be explained by their role in supporting the views of the religiously conservative leadership of Louis Napoleon's France (Farley and Geison [1974]; Farley [1978]).

Kuhn has frequently been cited (for example, MacKenzie [1981]; Barnes [1982]) as an inspiration to such externalist history and sociology of science, one of whose most influential manifestation is the strong programme in the sociology of scientific knowledge (for example, Bloor [1991]). Although Kuhn may have unintentionally encouraged externalist science studies with a few remarks in *Structure* concerning the effects of nationality and the like on the outcomes of revolutions ([1962], pp. 152–3), it is important to bear in mind that these remarks come at the beginning of several pages in which Kuhn discusses in detail the play of internal factors that influence revolutionary science, above all the power to resolve anomalies and to provide the basis for future puzzle-solving. Kuhn himself ([1992]; Baltas *et al.* [2000]) explicitly rejected the strong programme and endorsed internalism. Although these statements came later in his career, the Kuhn of *Structure* has to take a predominantly internalist approach.

One way to understand why Kuhn is an internalist is to note that he is a historicist in two respects. We have already seen how Kuhn emphasizes the importance of tradition in science, especially in normal science, but also, as I have argued above, through revolutions as well. This is the *conservative* strand of Kuhn's historicism. There is also what I call a *determinist* strand, one that sees patterns and laws in history—in the case of *Structure*, that is the pattern of normal science, crisis, and revolution. These are related: the tradition in question is a tradition of puzzle-solving, which in normal science involves adherence to certain exemplars (paradigms) of good science that are transmitted in the pedagogy of that field. Even in revolutionary science, the puzzle-solving power of the existing tradition represents a standard that any replacement paradigm must at least promise to match. It is the nature of this exemplar-centred, puzzle-solving tradition that explains the cyclical (or helical) pattern Kuhn observes in science. That story is essentially internalist: it is the *scientific* traditions and commitments of scientists that drive scientific change. Indeed, if Kuhn's account is to be correct, it cannot contain a significant externalist element. For if it did, how could there be any pattern to science? If scientists' preferences among theories were to a significant extent influenced by external factors, we should not expect to see periods of stable normal science with less frequent crises and revolutions. Crises in science would be precipitated by social factors, not by anomalous puzzles, and revolutionary debates decided by political rather than puzzle-solving advantage. We would not expect to see any pattern in this process unless those external factors were also subject to a pattern. But a general determinist socio-political historicism (such as Marx's) is not a credible view today. Kuhn's pattern is plausible only because it is driven by an internal motor.

### 4.7 Wittgenstein

A number of commentators have seen parallels between the ideas of Kuhn and Wittgenstein and have even inferred a significant influence of the latter on the former (for example, Collins [1991]; Malone [1993]; Kindi [1995a], [1995b]; Barnes *et al.* [1996]; Andersen *et al.* [2006]). Read ([2012]), for example, calls Kuhn 'a Wittgenstein of the sciences'. Although some such work can provide illumination, one should not overstate the similarities nor overestimate the degree to which Kuhn's ideas in *Structure* had origins in Wittgenstein's thought.

Wittgenstein is one of the few philosophers referred to by Kuhn in *Structure*. Over a page or so ([1962], pp. 44–5), Kuhn discusses Wittgenstein's notion of a family resemblance concept. Is this the kernel of a significant Witgensteinian influence on Kuhn, and in particular does this influence Kuhn to advance a theory of concepts along Wittgensteinian lines (Andersen *et al.* [1996]; Nersessian [2003]; Nickles [2003])? The thesis is implausible for two reasons: it ignores the history of the composition of *Structure* and it misunderstands the passage in question.

In 1961, the year before publication, Kuhn circulated a typescript of *Structure* that is almost identical to the published version. The principal difference lies in a chapter, 'The Priority of Paradigms', that appears in the published version but is absent from the nearly complete typescript. It is in this chapter that the passage mentioning Wittgenstein appears, in all probability as a result of Kuhn's being introduced to Wittgenstein's *Philosophical Investigations* when Kuhn's colleague Stanley Cavell read the typescript. So, Wittgenstein cannot have been a central influence on the ideas that led Kuhn to *Structure*. Nonetheless, it may well have been that the discussions with Cavell and reading the *Philosophical Investigations* did cause Kuhn to think differently about some important features of *Structure*. But these do not include any theory of concepts.

Wittgenstein uses the example of the concept GAME to show that such a concept cannot be characterized by a set of necessary and sufficient conditions for its application (and so *a fortiori* the relevant term cannot be substituted by a synonymous expression, its analysis, that expresses its meaning). Although it may be natural to infer that Kuhn therefore shares and builds on a Wittgensteinian theory of concepts, examination of the passage in question shows no such intention. In the chapter in question, 'The Priority of Paradigms', Kuhn is not interested in developing a theory of concepts or of meaning. Instead, Kuhn wishes to explain what it is that a scientist is committed to when she is committed to a paradigm, in order to articulate how it is that a paradigm functions. A simple and partially correct answer is rules. But one problem here is that an historian, on directly inspecting the paradigm, typically fails to find a full set of rules that rationalizes the paradigm. So what, if not rules (or rules alone), guides and constrains the scientist? Kuhn turns to an analogous question raised by Wittgenstein, although, as Kuhn says, in a *different* context: given that we cannot find a set of rules (necessary and sufficient conditions) that determine the application of a concept such as GAME, what then does guide and constrain our use of that concept? Kuhn takes Wittgenstein's answer to be that experience with exemplars of a concept can allow us to cognize a set of similarities among them; we do not extract rules from these similarities but acquire an ability to see a family resemblance among them. Going back then to Kuhn's question, we see that, analogously, what guides a scientist is not a set of rules but the acquired ability to recognize resemblances between new research problems and the field's established achievements. So Kuhn is using Wittgenstein's discussion of concepts *not* to inform his own thinking about concepts but to inform, by analogy, his own thinking about the processes of cognition that guide scientists in their research.

## 5 After *Structure*

An original, wide-ranging, interdisciplinary, and bold book, *Structure* inevitably raises more questions than it answers. For example, what exactly are the key characteristics of a revolution: a psychological transformation, generating a new phase or tradition of normal science, a revision in scientific belief, or incommensurability? Kuhn associates all these with revolutions, but it is not obvious that they must all occur together. For that matter, while Kuhn does tell us about the origins of scientific fields in 'pre-paradigm' science, we are given little to go on regarding the proliferation of scientific specialties in modern science (there are later hints of speciation as an analogy, developing the evolutionary approach). That raises the question of the size of revolutions: Are they just the major upheavals associated with the great names (Newton, Lavoisier, Darwin, Einstein)? Or can the general picture be applied also to much smaller changes in more focused fields? How do paradigms develop over time? For it is not the case that the exemplars of *Principia Mathematica* are the exemplars with which late nineteenth-century physicists are trained: if there is a single tradition of normal science, how does that tradition develop and change its paradigms in a non-revolutionary way? Can the central ideas of *Structure* survive a blurring of the distinction between extraordinary and normal science? Or do they invite a less radical conception of the difference? For, as I have suggested, Kuhn never intended incommensurability to be total, marking an unbridgeable gulf between paradigms. Yet, how exactly can partial incommensurability occur, and does a precise account support Kuhn's claim that there is puzzle-solving progress across revolutions? The model of scientific cognition is recognition—seeing a puzzle as like an exemplar. Familiar cases of recognition are one-step processes: I see that the child resembles the parent, for example. But it is undeniable that scientific thinking involves complex multistep processes of reasoning. Does that fact refute Kuhn's claims about exemplars, or does it just require more detail to be added to the explanation of how exemplars work?

*Structure* was initially well received with favourable reviews from historians, scientists, philosophers, and historians.[7] Nonetheless, as the book became better known, it also became the target of sustained criticism from a range of philosophers. Yet most criticisms did not address the questions raised above. Kuhn's claims about incommensurability, even though not a central theme of *Structure*, were a frequent target, but most often because they were associated with the charges of relativism (for example, Shapere [1964]) and irrationalism (for example, Lakatos [1970]; Newton-Smith [1981]). Kuhn's positive evaluation of normal science was criticized by the critical rationalists (Popper [1970]; Lakatos [1970]; Feyerabend [1970]; Watkins [1970]). Unclarity about the notion of paradigm was another target (Shapere [1964]; Masterman [1970]).

[7] For an excellent introduction to *Structure* and its context see Hacking ([2012]).

Kuhn's writings after *Structure* increasingly concentrated on responding to such criticisms, including adding precision to the notion of a paradigm-as-exemplar (the most novel idea of the book, he tells us). Yet from the late 1970s, the paradigm idea was largely absent from Kuhn's thinking while incommensurability took centre stage. At the same time, the nature of Kuhn's work changed. The naturalistic flavour of *Structure*, centred on the exemplar idea, is dropped, and Kuhn's discussions of incommensurability, which are focused on the linguistic or conceptual aspect of incommensurability, are more clearly *a priori* in approach and recognizably philosophical in tone. Kuhn even held that ideas that he had early come across empirically (from history) could have been discovered 'from first principles' ([1992], p. 10). Naturalism in philosophy was utterly unfamiliar when Kuhn published *Structure* (Quine's 'Epistemology Naturalised' appeared seven years later). And so it is perhaps unsurprising that it was not understood and led to accusations of irrationalism. It is also unsurprising that Kuhn was not well equipped to provide a theoretical defence of his approach, and that he abandoned it for a more familiar philosophical mode of thinking that would be comprehensible to the philosophical community of which he was increasingly a part and which was always his intended audience.[8]

It is my view that Kuhn's later work is something of a wrong turning (Bird [2002]). Even as regards incommensurability, Kuhn's linguistic approach ignores important psychological aspects of the fact that scientists operating with different paradigms can think about the world in incommensurable ways (Bird [2007]). More importantly, Kuhn's linguistic turn and the philosophical hostility to *Structure* that precipitated it may have prevented Kuhn from developing the ideas of *Structure*, especially those surrounding the exemplar concept, in a philosophical environment that became increasingly open to the mixture of history, psychology, and philosophy embodied in Kuhn's book. Nonetheless, Kuhn's failure to do so presents us with an opportunity. Although *Structure* is in many ways a product of its time, reassessing that book half a century later should allow us to see which of its ideas have enduring significance. In my opinion ([2005]), the exemplar idea is ripe for renewed investigation and development with the tools of current psychology and cognitive science, in a climate that is more receptive than that which Kuhn himself faced.

[8] Which is not to say that he abandoned history of science, for on the contrary he published important historical work, but of the familiar detailed, scholarly kind, rather than the synoptic nature of *Structure*. That said, Kuhn increasingly presented himself as a philosopher, and as Hacking ([2012], p. x) points out, 're-organised his own past'. We should also note that Kuhn returned to naturalism in an as-yet unpublished book he was working on in his last years, in which developmental psychology plays an important part.

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