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Kuhn's wrong turning

Alexander Bird

Department of Philosophy, University of Edinburgh, David Hume Tower, George Square, Edinburgh EH8 9JX, UK

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Abstract

Why, despite his enormous influence in the latter part of the twentieth century, has Kuhn left no distinctively Kuhnian legacy? I argue that this is because the development of Kuhn's own thought was in a direction opposite to that of the mainstream of the philosophy of science. In the 1970s and 1980s the philosophy of science took on board the lessons of externalism as regards reference and knowledge, and became more sympathetic to a naturalistic approach to philosophical problems. Kuhn, on the other hand, started out with a strong naturalistic streak, employing non-philosophical disciplines, primarily psychology, in order to build his accounts of scientific change and the nature of observation and scientific thought. But by the 1970s Kuhn's work had taken on a much more purely philosophical, *a priori*, tone. His explanation of incommensurability moved from a psychological explanation to one embedded in the philosophy of language. Increasingly he gave his outlook a Kantian gloss. I suggest, nonetheless, that Kuhn's most valuable contribution is to be found in *The Structure of Scientific Revolutions* and not in his later work, and that the naturalistic direction of the former has important links with connectionist research in cognitive science that deserve further study.

Keywords: Kuhn; Scientific revolutions; Empiricism; Naturalism; Incommensurability; Reference

1. A Kuhnian legacy?

Thomas Kuhn's legacy presents something of a paradox. While Kuhn was the most significant influence on the philosophy of science in the last third of the twentieth century, it is nonetheless the case that there is no specifically Kuhnian school

E-mail address: alexander.bird@ed.ac.uk (A. Bird).

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in the philosophy of science. Nor is Kuhn's most characteristic thesis—the thesis of incommensurability—regarded any longer as having the philosophical significance that Kuhn claimed for it. It is true that in the history and sociology of science there are those who would describe themselves as Kuhnians or who would regard themselves as carrying forward a project initiated by Kuhn. Yet, as Kuhn's later work—much of it collected in a new volume *The Road Since Structure*[1]—shows, there is a gulf of understanding between him and many of his would-be supporters. Kuhn remarked that he is often regarded as initiating 'external' history of science—the view that we must seek the causes of scientific change in events occurring outside the realm of scientific activity; yet he regarded himself as strictly internalist.[2] He also repudiated the Strong Programme, taking it to claim that all causes of scientific change are political—a characterization which is at best a poor caricature.[3]

In this paper I want to ask why, despite Kuhn's enormous impact, his legacy seems so thin as regards philosophers and so beset by misunderstanding as regards historians and sociologists. A large part of the answer lies in Kuhn's failure to explain and exploit his central achievement, viz. the identification of paradigms, in the form of exemplars, as the motor of the development of a scientific field. As I shall explain, Kuhn is not entirely to blame. The machinery that Kuhn needed to develop this idea was not available to him when he wrote *The Structure of Scientific Revolutions*. The machinery in question is the product of subsequent connectionist (or 'neural net') research in cognitive science, and is the complement to the psychological research that Kuhn drew upon in his early work. So part of the diagnosis of the lack of Kuhnian legacy is that the central idea of *The Structure of Scientific Revolutions* was before its time in an important respect.

Yet that cannot be the complete answer, since that connectionist machinery was developed during Kuhn's lifetime, but he never chose to make the link between it and the paradigm concept. The reason for this is that Kuhn's research had by this time taken a new turning. Whereas *The Structure of Scientific Revolutions* is naturalistic in approach, drawing upon empirical, scientific discoveries where appropriate, his later work is much more philosophical in style and *a priori* in method. For example, in *The Structure of Scientific Revolutions* Kuhn's explanation of the relationship between observation, theory and reality was informed by gestalt psychology and by the results of research carried out by his Harvard colleagues, the experimental psychologists Bruner and Postman. Later, by contrast, Kuhn supported his view with quasi-Wittgensteinian considerations from the philosophy of language, while he characterized that view in terms of Kantianism.[4]

This was more than just a change of style. The move from a naturalistic to an *a priori* approach was a move in the direction opposite to the prevailing movement of philosophy itself. Naturalised epistemologists, led by Quine, came to deny that questions concerning the nature and possibility of knowledge are part of *a priori*

[1] Kuhn (2000).
[2] Kuhn (2000), p. 287.
[3] Kuhn (2000), pp. 110–111.
[4] Kuhn (2000), p. 104.

philosophy; rather, they are the concern of empirical cognitive psychology. More specifically, Kuhn's philosophical approach betrayed commitments characteristic of the positivists and logical empiricists he intended to be rejecting. Although Kuhn had been instrumental in bringing philosophy to repudiate logical empiricism, by the late 1970s the vanguard of philosophy had overtaken Kuhn is this direction. From the perspective of the new causal theory of reference and causal or reliabilist theories of knowledge, Kuhn's thesis of incommensurability and his rejection of truth-related progress appear to be conservative and positivist.

I shall explain why Kuhn's earlier views would have benefited from a continued naturalistic development and why his later, philosophical approach was a failure for just the reasons that logical empiricism was a failure also. In my view, therefore, Kuhn's philosophical turn was a wrong turning, a wrong turning that explains his lack of contemporary significance in mainstream philosophy, including philosophy of science. In the bulk of the paper I shall discuss the wrong turning that Kuhn took, as illustrated in the writings in *The Road Since Structure*.

I shall also suggest an explanation of why Kuhn made this wrong turning. This is more speculative, but the cause lies in Kuhn's relationship with academic philosophy. On the one hand Kuhn had very little philosophical training. On the other hand he was strongly inclined to be a philosopher. Kuhn started his professional academic career as an historian of science (who had been a Ph.D. physicist) and then moved across disciplines to philosophy. This shift in academic allegiance matched the philosophical turn in Kuhn's thought. Yet, not being thoroughly trained in the philosophical canon of the twentieth century, Kuhn was not aware of the historical, dialectical provenance of the philosophical ideas with which he was working. He was able to identify certain ideas as being characteristic of positivism or empiricism, such as the thesis that observation and perception are pre-theoretical. These he attacked and thereby helped to undermine positivism. But at the same time he was unaware that other (related) theses, which he happily adopted, were also central to positivism, such as the theoretical-context account of the meaning of theoretical terms, or the conviction that truth-as-correspondence is inaccessible. It is the partial rejection and partial retention of positivism that causes Kuhn to expound apparently radical theses such as the thesis of incommensurability. Kuhn's training (or lack of it) plus his academic commitments made it impossible for him to pursue the far more radical and complete rejection of positivism that has subsequently been achieved.

2. Kuhn's approach in *The Structure of Scientific Revolutions*

Kuhn had a multiplicity of aims in *The Structure of Scientific Revolutions* which have been widely discussed. Here I want to highlight not so much the claims he makes but rather the methods and kinds of evidence he employs to support his claims; I'll then contrast this with the approach and method of his later writing. I have described *The Structure of Scientific Revolutions* as theoretical history. Kuhn himself

regarded his book as history written for philosophical purposes.[5] What is interesting is that the evidence Kuhn adduces for his more philosophical claims is mostly not philosophical in nature but comes instead from the empirical sciences, as well as from the history of science. If a philosophical thesis entails that we must reason in a certain way but empirical evidence suggests that we do not, then that evidence is a legitimate tool to employ against the philosophical thesis. But what is interesting, as I shall go on to argue, is that Kuhn later abandoned his use of empirical evidence for a more *a priori* approach.

One of the key areas where Kuhn calls upon empirical evidence is in the argument for the theory-dependence of observation. Famously he refers to the results of the gestalt psychologists such as Wertheimer and Kohler to show that the character of our observations is dependent on what prior beliefs or expectations we may have. He supplements the gestalt examples with a discussion of Bruner and Postman's experiments with anomalous playing cards.[6] Those experiments showed that subjects had much more difficulty in describing unusual playing cards, where the colours of the suits had been swapped, than in describing standard cards. What Kuhn claims is that perceptual experience is influenced by the theory one possesses.

Intriguingly Kuhn also refers briefly to other research he was carrying out in order to support his approach. In the Postscript 1969 to the second edition of *The Structure of Scientific Revolutions* Kuhn remarks that he was experimenting with a computer programme designed to model the operations of intuition.[7] This model is described in a paragraph in 'Second Thoughts on Paradigms' written at about the same time as the Postscript 1969 but not published until 1974.[8] In this model a stimulus (in the form of a string of numbers) is transformed into a datum (also a set of numbers) by a mathematical function.[9] The datum can be thought of as a position in space; so, for example, if the stimulus has two digits, these will be transferred into a datum consisting also of two digits, which may be regarded as representing a point on a two-dimensional graph. Two stimuli might be transformed into data that represent nearby points on the graph. That will depend on the mathematical function being employed by the programme. A different function might transform the stimuli into data that represent distant points on the graph. In a simple programme the function will be fixed and the programme will thereby determine which stimuli are grouped together by virtue of yielding nearby data. In a more sophisticated programme, of the kind with which Kuhn was experimenting, this process can in effect be reversed. The programme can be told in advance which stimuli belong together; the programme itself then selects functions that will, if possible, transform stimuli that belong together into data that cluster together on the graph.

Kuhn says that in this model not every grouping of stimuli can be made to yield data that cluster together (except, I imagine, by a clearly artificial function). Nonethe-

[5] Kuhn (2000), p. 276.
[6] Kuhn (1962), pp. 62–65; Bruner & Postman (1949).
[7] Kuhn (1970), p. 192 n. 12.
[8] Kuhn (1974).
[9] Kuhn (1977), p. 310.

less, it is apparent that different groupings of the same set of stimuli can lead to different functions and so different clusters of data. Once the programme has selected an appropriate function it will continue to classify new stimuli as being similar or different to previous stimuli by virtue of the location of the data yielded. It is clear that the programme does not detect the single salient set of pre-existing, natural divisions among the stimuli; rather it learns to see certain similarities and differences among stimuli; it could have been trained to be sensitive to a different set of similarities and differences. At the same time, Kuhn does not think that the programme can see just any old set of stimuli as similar. The way in which we see the world is not *merely* an artefact of the training we have received. As the Bruner and Postman experiments show and as Kuhn was keen to emphasize in the context of the nature of observation, perceptual experience is a product of the innate system plus training and prior experience plus the stimulus received. Kuhn illustrated the role of training in a parable about a child learning about the differences between various fowl. All the birds in the park are different from one another—no two are identical. The child's parent tells the child that this bird is a swan, and so is the next. The third is a duck, the fourth is a goose, and so on. In due course the child learns to be able to tell whether a particular bird is a swan, duck or goose. This training has developed the child's perceptual apparatus.

While Kuhn thought that this computer model and the results of psychology might give us knowledge of the way that perception actually works, he also thought that one way or another they give some insight into the working of paradigms. The two issues are linked, since it was natural for Kuhn's positivist predecessors to imagine that a piece of scientific knowledge is the result of a rational inference from some untutored perceptual experience. Kuhn's argument is that non-trivial perceptual knowledge can be had that is not the result of an inference but is genuinely perceptual, produced as a result of the training of the visual system with exemplars—exemplary cases (for example of swans, ducks and geese). If non-trivial perceptual knowledge can come about in this way, might not other, non-perceptual knowledge come about in a similar way? Kuhn was concerned to show that scientific thinking in general is not a matter of following the rules of reason but is instead a more intuitive (and learnable) activity, akin to learning to recognize a face or to play the piano. Just as the ability to know that a certain bird is a duck is produced by training with examples of ducks (and of non-ducks), Kuhn thought that the ability to see a certain form of an equation as that required to solve a given scientific problem is produced in a parallel fashion by training with exemplary problem-solutions. It was this that Kuhn regarded as the most novel and least understood aspect of *The Structure of Scientific Revolutions*.[10] Training with and professional exposure to exemplars enable scientists to see puzzles as worth pursuing, to recognize solutions to puzzles, to evaluate proposed puzzle solutions, and so on. This presented a direct challenge to the view of Kuhn's positivist predecessors that the process of justifying a

[10] Kuhn (1970), p. 187.

(reasonable) scientific belief can always be manifested as the result of following the rules of rationality (such as an inductive logic or the scientific method, and so on).

Kuhn's claims and the relationship they bear to the empirical evidence are not without problems. Do the gestalt and playing-card experiments, as well as the computer model and the parable of the child and waterfowl, show that observation is *theory*-dependent? It seems not, for what all those cases show is only that current perceptual experience is influenced by *previous perceptual experience*. Since one can learn or adopt a theory without any characteristic perceptual experience it is unclear whether these experiments lend much support to the thesis that observation is influenced by *theory*. Kuhn nonetheless took it for granted that these experiments do show that observation and perception are theory-dependent. But even he could not explain in any detail how they bear on his more extensive claim that the functioning of paradigms (exemplary puzzle solutions) is a matter of learning to 'see' puzzles and their solutions, rather than a matter of deploying reason. Kuhn was therefore unsure whether these psychological results showed how the mind worked in general, both in perceptual cases and in scientific thinking, or whether they merely provided an analogy for the operation of paradigms.[11] Another problem was that Kuhn's claims seemed, to his critics, to make scientific belief, and in particular change of belief (especially in the analogy of a gestalt shift), a mystery or at least irrational—particularly as it seemed just obvious that an extended process of scientific thinking should be rule-governed. (The view that Kuhn's account made science irrational was reinforced by Kuhn's passing parallel with religious conversion.) Kuhn was at pains to stress that there is nothing mysterious about the nature of the processes bringing about changes in perception and belief. In the Postscript 1969 he points in the direction of an explanation, at least in the case of perception, that he thinks is fully within the domain of the 'neuro-cerebral mechanism'.[12] He distinguishes between *stimuli* and *sensations*. Sensations are our perceptual experiences. Stimuli are the light rays, sound waves and so forth incident on our eyes, ear-drums and so on. Our knowledge of the latter, says Kuhn, is highly theoretical. There is much neural processing between stimulus and sensation. Importantly, the same stimulus need not result in the same sensation—if the neuro-cerebral mechanism is different. And what may produce different neuro-cerebral mechanisms is a difference of training with exemplars; a difference in the training given to the child, the difference modelled by giving the computer programme, the same stimuli grouped in different ways. What Kuhn lacked, however, was any way of spelling out in detail how the neuro-cerebral mechanism could be made to change in this way. This lacuna was not made easier to fill by the fact that the prevailing computational model of the mind made it natural to think that learning could only change a person's memory but not her processing capacities. Furthermore, that model implied that human thinking must be algorithmic and rule-based, just like the operation of a computer programme.

[11] Kuhn (1962), p. 65.
[12] Kuhn (1970), pp. 191–194.

3. Kuhn naturalised?

The use of results from psychology and computer science and the willingness to look on his project as a quasi-historical, quasi-scientific explanation of scientific change, albeit one with philosophical significance, comes to an end at about the time of 'Second Thoughts on Paradigms'. I mentioned that Kuhn himself was not entirely sure whether the gestalt and other psychological examples he refers to provide instances of the same general psychological functions that are employed in the operation of paradigms or merely an analogy for them. Kuhn seems to have concluded that they provide at best an analogy (and a potentially misleading analogy at that). Thereafter he was less interested in what in fact explains scientific change and became more concerned with the philosophical lessons to be learned. He made much less use of empirical examples, even historical ones, in order to teach those lessons, and employed more explicitly philosophical tools. The focus of his interest shifted from the function of paradigms and nature of perceptual experience to the nature of the language we use in science.

How Kuhn's linguistic, philosophically inclined thinking developed will be explored later in this paper. In this section I shall speculate on how matters might have turned out had Kuhn not turned his back on the empirical element in his thinking and had instead developed it, to give a thoroughly naturalistic account of theory-change, world-change and incommensurability. The sorts of psychological and cognitive issues with which Kuhn was concerned in *The Structure of Scientific Revolutions* have since been explored elsewhere, although not explicitly in relation to scientific change, and have provided the theoretical underpinning that Kuhn's account lacked.

Connectionist or neural-net models of brain function show that many mental activities can be understood as rule-less attuning of the system to exemplars. Pattern-recognition is the prime example. Humans can learn to recognise faces easily, after only one or two exposures, no doubt because evolution has trained this useful ability to a high degree. More esoteric skills can also be acquired through training with exemplars, such as the ability to recognise the composer of a piece of music one has previously not heard or the painter of a picture one has not seen, having learned to recognise the style of that composer or painter. One has learned to spot salient similarities. But that ability is typically one that cannot be encapsulated in rules, conscious or unconscious. It is clear that connectionism is well placed to explain the learning-influenced perceptual cases Kuhn discusses. Kuhn also thought that paradigms operate in this way. There are problems with applying a connectionist approach directly to non-perceptual scientific thinking, in that it does not, at first sight, leave room for the role of structured reasoning in science—and it is hard to deny that reasoning does take place. But, using Kuhn's own terms, this may be only a puzzle rather than an anomaly, and it is not impossible to imagine ways of developing Kuhn's account within a connectionist framework that also accommodates reasoning.

Kuhn applies the quasi-intuitive, rule-less thinking under discussion to the operation of exemplars, but he only hints at their role in relation to the topics of world-change and incommensurability. In these areas the connectionist approach has much

to offer also. In the case of incommensurability it is easy to see how the link could be made, since the 'intuitions'[13] fostered by one paradigm might not be fostered by another, and so the scientific value-judgments of proponents of different paradigms might not coincide. This would be analogous to (and produced in the same way as) differing judgments concerning artistic or personal beauty which might differ from one society or era to another. I am sure that Kuhn had this sort of picture in mind in *The Structure of Scientific Revolutions*, but it is edged out in favour of a linguistic (and hence more recognizably philosophical) conception in the 1970s and 1980s. Understanding world-change in terms of connectionist-based intuition might have been even more fruitful. I have suggested that one can conceive of a person's 'world' (in a sense characterized for this purpose) as made up not (only) of their perceptions and language, but (also) of the quasi-intuitive associations they make, the learned similarities and associations that channel our thoughts in one direction rather than another.[14] For example, an evolutionary biologist may see the flying squirrel's 'wing' as a wonderful example of adaptation, while the religious individual might see them as an instance of divine providence and inspired creation (and some may even see them as both). The 'seeing' here is not a *visual* seeing. But we use a perceptual verb because the individuals make an intuitive connection, one that is for them second nature, between what they see and some idea. This intuition is like perception because it is not a matter of inference. It is second nature in that it operates naturally (rather than by the employment of reason) but is acquired (rather than innate). At the same time this provides an account of 'seeing' that allows us to explain the sense in which it is correct to say that Kepler and Tycho see different things when looking at the sun rise or that Aristotle and Galileo see different things when looking at a pendulum. Kuhn and Hanson took these to be differences in perception and hence instances of the theory-dependence of observation. But this is clearly implausible and tendentious. What differs between the scientists is not their perceptual experiences, but what those experiences intuitively, (second-)naturally and rule-lessly prompt them to think and say.

An area where a commitment to naturalism would have made Kuhn's thinking less positivistic is his general epistemology. A relativist view holds that the truth of one's beliefs or what one can correctly be said to know depends on the conformity of one's beliefs to the prevalent doctrine. Kuhn was certainly not a relativist in this sense. Instead he eschewed the concepts of truth and knowledge altogether, thinking that an adequate account of science had no need of them. He was, however, a sceptic, in that he thought that knowledge of the ways things really are is not to be had. Indeed, he goes further, at one point saying that the idea of the way things really are (and hence also the notion of knowing the way things really are) is incoherent. One of his grounds for adopting this opinion is the view that gaining knowledge of the way things are would require seeing the world from an 'Archimedean platform'—

[13] We can call the products of such processes 'intuition', or as Kuhn prefers, following Polanyi, 'tacit knowledge'. But as Kuhn notes this is intuition that is learned and that is shared by members of the same scientific community. See Kuhn (1970), p. 191.

[14] Bird (2000), pp. 133–136.

a point of view independent of current belief, that can assess the relation of current belief to the (absolute) truth.[15] It is true that an Archimedean platform cannot be found. But it is a mistake—an historically ancient mistake shared with Cartesians, empiricists, positivists and so on—to think that an Archimedean platform is required for non-relative knowledge to be possible. According to (one stream in) contemporary 'externalist' epistemology, what is required for knowledge is that one's beliefs be formed as the result of a reliable process of belief formation.[16] The details of the view are for current purposes unimportant (and the subject of much debate). What is important is what is agreed upon, which is that to have knowledge the believer does not have to know herself that the belief-forming process she uses is reliable. The naturalistic approach endorses this. Many of our perceptual processes are reliable—evolution has made them reliable. That is enough for them to give us perceptual knowledge, whether or not we have a belief in the reliability of our perception. Something similar goes for more sophisticated belief-forming processes. We form scientific beliefs as the result of quite sophisticated processes at the heart of which is the current paradigm. The paradigm may be a reliable generator of beliefs—and for reasons analogous to the perceptual cases. It may well be that past selection pressures have ensured that a current paradigm is a reliable source of solutions to certain classes of puzzle, and hence that the scientific beliefs thereby generated count as knowledge. In specific cases that may have happened or it may not. But naturalistic epistemology still permits the possibility of knowledge. Again, naturalism in epistemology is a development that took off just as Kuhn was abandoning his own naturalistic phase. It is noteworthy that Kuhn himself, throughout his career, emphasized the Darwinian, evolutionary nature of scientific change, as, of course, did Popper.[17] This might have led him in the direction I am advocating here. But, unfortunately, Kuhn later denied that an evolutionary epistemology need be a naturalised epistemology, and explicitly regretted the 'overemphasis' on the empirical aspect of his enterprise.[18]

4. Incommensurability

Kuhn's famous incommensurability thesis is the prime vehicle for his shift from naturalistic to *a priori*, philosophical thinking. In *The Structure of Scientific Revolutions* the incommensurability thesis is a fairly general claim asserting on various grounds the lack of a common measure of hypotheses from differing paradigms. The root of incommensurability is the claim that perception and observation are not theory-independent but are influenced by the paradigm within which one is operating. It is these facts about perceptual psychology that Kuhn thought of as undermining

[15] Kuhn (2000), pp. 113, 115.

[16] Note that 'externalist' epistemology is a very different kettle of fish from 'externalist' history of science.

[17] Kuhn (1962), pp. 169–173; Kuhn (2000).

[18] Kuhn (2000), p. 95.

positivism (and indeed the Cartesian tradition in general). Another element in incommensurability is the role of paradigms in setting the standards by which hypotheses are evaluated.

The lack of a common set of observations in turn means that even observational terms cannot be expected to mean the same when used in different paradigms, even if the same word is used. This is significant because a shared observational vocabulary had been regarded by the positivists as the ground of meaning for the whole of a theory and as the basis for comparison between theories. For positivists theoretical terms are not referring—they do not serve to connect to kinds, properties, substances and so forth in the world. Instead their meaning is completely given by their role within the theory; this is the *theoretical-context* view of meaning. This implies that the meaning of a theoretical term may easily (perhaps always) change when there is a change in the theory. Kuhn endorsed the theoretical-context view of theoretical meaning. This is a major part of the explanation of the phenomenon that inspired Kuhn early on, that Aristotle's physics made no sense when interpreted in contemporary terms, whereas if understood in its own terms, it could be seen to be a plausible scientific position.[19] For the positivists, however, this sort of misunderstanding could be straightforwardly circumvented by comparing the observational components of the different theories. These, being couched in a shared observational language, can provide a common basis for understanding the import of the two theories. But if Kuhn is right that the meanings of our observational expressions are dependent on the theories we use, then they too, along with the theoretical terms, are subject to change in meaning when theories change. *All* scientific terms are subject to change in meaning when there is a change in theory.[20] (It is worth noting that the positivists' motivation for the theoretical-context account of theoretical meaning is precisely the foundational role they gave to observation and observational language and their corresponding empiricist rejection of the unobservable. Hence the positivist thesis that Kuhn rejected provides the rationale for the thesis he retained. It is thus not surprising that incommensurability should seem so radical and so difficult to make detailed, coherent sense of.)

At the same time, it is also clear that Kuhn did not, in the early 1960s at least, think of the linguistic incommensurability as the complete explanation of this phenomenon. Making sense of Aristotle is not simply a matter of achieving a better understanding of what Aristotle meant. It is also a matter of seeing why it would be appropriate for Aristotle to make the concrete claims he does. And appreciating that requires understanding what one is committed to by adopting an Aristotelian paradigm. The paradigm does a lot more than fix meaning. It fixes the puzzles it is appropriate to tackle, it provides a heuristic for helping the scientist to solve those puzzles, and it sets the standards whereby attempted puzzle-solutions are judged. Kuhn encapsulates this idea by saying that the paradigm determines the world in which the scientist

[19] Kuhn (2000), pp. 59ff.

[20] For a reconstruction of Kuhnian incommensurability along these lines, see Newton-Smith (1981), pp. 151–156.

works. A corollary of this is that when the paradigm changes, the scientist's world changes also. Some critics (and some would-be supporters) took this to be a sign of an extreme form of social constructivism or idealism. Much later, as we shall see, Kuhn did give the world-change thesis a quasi-idealist, neo-Kantian slant (while others interpreted it within the linguistic framework Kuhn was developing).[21] But in the 1960s, the world-change thesis was neither idealist nor (primarily) linguistic. Instead it was psychological. As I have discussed, the clearest focus of the thesis concerns the function of the paradigm in determining perception and observation, while Kuhn also hoped the idea could be extended to the other non-observational functions of paradigms mentioned above, such as spotting and assessing solutions to puzzles.

Kuhn emphasized that despite what some critics had taken him to mean, he did not intend incommensurability to *mean* non-comparability nor even incommunicability.[22] Proponents of different paradigms can communicate and in many respects their paradigms can be compared. Nonetheless, incommensurability does rule out certain kinds of comparison—the point by point comparison that Kuhn associates with Popper and with the (then) positivist orthodoxy.

Kuhn's later thinking on incommensurability centred on the linguistic kind of incommensurability, specifically on the conviction that it could be understood as a certain kind of untranslatability between the language of the new theory or paradigm and that of the old. The search for a conception of incommensurability that is clearly anchored in the philosophy of language had begun in the 1960s, when Kuhn associated his incommensurability thesis with Quine's indeterminacy of translation thesis.[23] He later abandoned this parallel, for Quine's thesis was that *too many* translations could be found for any single translation to be determinately correct, while Kuhn's thesis was that *no* translation is possible between languages of incommensurable paradigms. However, he did retain from this encounter with Quine the thought that what differs between incommensurable languages is the way they divide the world into kinds of thing. Kuhn then moved on to the more Wittgensteinian thought (which we have already encountered in the parable of the child learning the names of waterfowl) that learning kind-terms with visual exemplars would make possible different divisions of things into kinds if different exemplars are used.[24] In the 1980s this was developed as a taxonomic account of incommensurability.[25] A scientist will use a taxonomy in categorising entities—a biologist, for example, will have a taxonomy that divides living beings into different species, and different species into genera, and different genera into families, and so on. The terms used to express the taxonomy, its lexicon, are related to one another in a lexical network. These internal

[21] For a development of Kuhn's taxonomic version of incommensurability, see Hacking (1993). Kuhn responds, fairly sympathetically, in Horwich (1993), reprinted in Kuhn (2000), pp. 229–233.

[22] Kuhn (2000), pp. 35, 60.

[23] Kuhn (2000), pp. 164–165; see also Kuhn (2000), pp. 60–61.

[24] Kuhn (1974).

[25] See especially Kuhn (2000) pp. 13–20, 50–53, 228-237. Kuhn's taxonomic incommensurability is discussed in detail in Sankey (1998).

relations between members of the lexical network are constitutive of their meanings. Hence one cannot change a part of the network without changing all of it. Kuhn identified incommensurability with differences of taxonomy brought about by changes to or differences in the lexical network. Significant theoretical change will lead to a redrawing of the boundaries between kinds of things (or, more strictly, a redrawing of the boundaries between the extensions of the key theoretical terms). There is incommensurability since the old boundaries and old divisions do not line up with the new ones. Avoiding incommensurability would require adding to one lexicon new terms to translate the terms of the other, as one might import a French word into English to facilitate translation of French. However, as regards scientific taxonomies at least, Kuhn claimed that this is not allowed. A 'no-overlap' principle says that the kind-categories of a taxonomy cannot cut across one another.[26] Either the categories are entirely disjoint, sharing no members; or one category is a sub-kind of the other category, so all the members of the first are included in the second.

The idea that incommensurability is brought about by shifting categories is present also in Kuhn's Kantian conception of his views. Paul Hoyningen-Huene interprets Kuhn's thesis that when paradigms change the world changes in terms that are neither merely metaphorical nor thoroughly idealistic or constructivistic—something really does change when the world changes, but not everything; some aspects of reality remain fixed.[27] This interpretation employs a Kantian distinction between the world-in-itself, which is independent of our scientific investigations, and a phenomenal world, which is partly constituted by our minds, in that the appearance of things as being similar to or different from one another is a product of the mental categorization of them. While Kant conceived this categorization, whereby our experience is moulded, as being immutable, Kuhn, on this interpretation, regards this function of the mind as subject to change. An individual's categorizations are, in part, the product of his or her immersion in a paradigm-governed tradition. Hence when the paradigm changes the individual's categorizations are likely to change too. The phenomenal world thus changes, even if the world-in-itself does not. Kuhn himself endorsed this way of understanding his outlook, describing himself as a (post-Darwinian) Kantian with moveable categories.[28]

Why did Kuhn come to adopt this description of his views? And is it a correct description of them, including his views at the time of writing *The Structure of Scientific Revolutions*? I have emphasised the naturalistic element of the latter. Could not one combine the neo-Kantian conception with the empirical observations of *The Structure of Scientific Revolutions*? The (socio-)psychological nature of paradigms could provide the explanation of why categories change when scientific revolutions occur. However, as Hoyningen-Huene points out, a deep tension soon becomes apparent.[29] As we have seen, Kuhn's earlier view was that a full explanation of the way that exemplars work in forming our perceptual experiences and our categoriz-

[26] Kuhn (2000), pp. 91–94, 235, 243–245.
[27] Hoyningen-Huene (1989).
[28] Kuhn (2000), pp. 104, 264.
[29] Hoyningen-Huene (1993), pp. 45–6.

ation of things would have to invoke facts about neurophysiological structures in the brain. Such an explanation will be the result of scientific inferences concerning unobservable entities. So that explanation will be referring not to the phenomena but to the things-in-themselves. But the (neo-)Kantian view says that the latter are unknowable.

The conflict between his earlier appeal to empirical science and his later neo-Kantianism would be a neat explanation of why Kuhn abandons the naturalistic element in *The Structure of Scientific Revolutions*—it had to go if he accepts the Kantian unknowability of things-in-themselves. Even so, there would still remain the question of why it is the naturalism that has to go rather than the Kantianism. As I shall explain, I think there is more to the story than this explanation suggests. After all, the neo-Kantian interpretation is a rational reconstruction of Kuhn's thinking. Kuhn did not talk of himself as a Kantian in the 1960s. Kuhn had very little formal philosophical training and so there is no reason to suppose that he was deeply acquainted with Kant's philosophy beyond an undergraduate survey course in the History of Philosophy.[30] Although Kuhn much later called his undergraduate acquaintance with Kant 'a revelation' and links this to his self-ascription as a Kantian with moveable categories, it seems that Kuhn's conscious Kantianism is apparent only from the late 1970s on. Its first appearance, in 1979, is very brief and explicitly metaphorical.[31] Later we are offered rather more detail, but this is after his extensive discussions with Hoyningen-Huene and the publication of the latter's detailed neo-Kantian interpretation of Kuhn. More importantly, Kuhn's position in *The Structure of Scientific Revolutions* is not a worked out philosophical theory that can be mapped straightforwardly onto a Kantian or quasi-Kantian view. It is not as if Kuhn was Kant with a different terminology. Instead Kuhn's views are explained through illustrative analogies, and are hedged with disclaimers such as: 'In a sense that I am unable to explicate further, the proponents of competing paradigms practise their trades in different worlds'.[32] Furthermore, if Kuhn had consciously worked out a position with the structure Hoyningen-Huene describes, then the tension between the naturalism and the Kantianism would have been clear to him.

Nonetheless, Kuhn was happy to adopt the neo-Kantian description of himself. And this is despite the up-front scepticism and partial idealism of such a view—idealist to the extent that phenomena are not objective but subject-dependent, sceptical because the world-in-itself is unknowable.[33] Although Kuhn rejected the charge of idealism that his remarks on world-change and incommensurability had fostered among his critics (and among supporters of a constructivistic inclination), the neo-Kantian self-ascription seems to be an admission that they were not very far off the mark. While there are differences between the idealism of which he was accused

[30] Kuhn (2000), pp. 263–264.
[31] Kuhn (2000), p. 207.
[32] Kuhn (1962), p. 150.
[33] Kuhn (2000), p. 104. Note, however, that there are points when Kuhn seems to want to do without the world-in-itself at all. This would remove this element of scepticism and make the position more thoroughly idealist. See Kuhn (2000), pp. 120, 207.

and the Kantianism he willingly adopted, one significant difference is that the former is philosophically unsophisticated while the latter is eminently respectable. The idealism and relativism ascribed to his empirically inspired world-change and incommensurability theses seemed weak in the face of his philosophical critics, while Kantian idealism benefited from the authority of Kant himself not to mention two hundred years of scholarship as well.

5. Incommensurability: the critics

One reason why Kuhn may have developed the taxonomic conception of incommensurability was that he tacitly acknowledged that the earlier version based on the theoretical-context account of meaning was untenable. At the very least Kuhn recognized that the philosophical tide was flowing against the earlier conception, for the theoretical-context account of meaning was itself under attack. The attack was indirect; it originated with anti-Fregean arguments from Saul Kripke and Hilary Putnam to the effect that the meanings of proper names and natural kind terms could not in general be regarded as residing in reference-determining senses or intensions, so long as those senses/intensions are regarded as items known by a competent language-user.[34] The lesson was quickly learned for key scientific terms, such as the central terms of a scientific theory. The theoretical-context account of meaning, as employed by Kuhn, took the sense of a theoretical term to be given by its role in a theory. For the incommensurability thesis to work, this sense had to be what I have called 'thick'—the 'role in a theory' of a term depends on a *large* quantity of the theory in a fair amount of detail.[35] This is why Kuhn is able to say that the word 'mass' has a meaning in Einstein's theories that is different from its meaning in Newton's theories, simply on the ground that Newton took mass to be conserved while Einstein did not.[36] After Kripke and Putnam the inclination of philosophers was to deny that natural kind terms (and hence theoretical terms picking out natural kinds and natural quantities and properties) have any sense at all, and *a fortiori* to deny that such terms have a sense that depends in a thick way on their role within a theory. Instead, they have a reference that is fixed by some causal connection between the use of the term and the reference itself: it is because the word 'electron' has a causal connection with electrons themselves (via the use of a theory employing the term 'electron' to explain phenomena such as cathode rays that are caused or constituted by electrons) that 'electron' refers to electrons.

Not only did this causal account undermine the theoretical-context account of meaning, it also permits the incommensurability thesis to be bypassed altogether. The causal account entails that reference is insensitive to changes in the details of a theory. Hence there is in general no problem in supposing that a term *t* in one

[34] See, for example, Putnam (1975), Kripke (1971, 1972).
[35] Bird (2000), p. 167.
[36] Kuhn (1962), p. 102.

theory refers to the same entity or kind of entity as does *t* when used in another theory, even if the two theories are associated with different paradigms. The latter thesis, call it the Continuity of Reference thesis, is combined with a second thesis, the Significance of Reference thesis. The latter claims that when it comes to theory evaluation and comparison (what, for example, constitutes the truth of a theory) what counts is the *reference* of the key terms, not their meaning in some other sense (i.e. not meaning as 'sense' or 'intension').

Kuhn sought to resist the causal account of reference.[37] That he attempted to do so, in the way that he did, marks him as more positivistically inclined than his leading philosophical contemporaries. That his attempts were so unsuccessful marks him, in my view, as lacking the training and forensic skills of those contemporaries. Kuhn first complains that the story Putnam tells suffers from a scientific implausibility and secondly objects to the essentialist identification of water and H_2O on the grounds that what we call water is at best a subset of stuff that is H_2O. But these are irrelevant to the core of the Putnam's claim, that two speakers can be in the same 'internal' states yet be referring to different stuffs. That implies that whatever fixes reference can be at least in part independent of the theoretical beliefs of the speakers (something like an external causal connection between stuff and use of the word in question). If that belief-independent component in reference-fixing is all or most of what fixes reference (as suggested by the simple causal theory) then a difference in theoretical commitment between speakers would not be enough to show a difference in reference—in which case Newton and Einstein might well have been talking about the same quantity when using the word 'mass', even though they might have had different beliefs about whether mass is conserved.

It is worth noting that the causal account of meaning, as applied to theoretical terms, is generally regarded by philosophers of science as not entirely successful—but not for Kuhn's reasons. Problems for the simple causal account, such as non-referring theoretical terms (e.g. 'phlogiston'), required bringing back something like the sense or meaning of a term as fixing its reference, where the sense is itself given by some part of the theoretical context. The causal component was retained to the extent that the theoretical context in question would be that part of the theory which asserts, fairly generally, the causal or explanatory role of the item being referred to; for example, the sense of 'electron' might be 'electrons are those subatomic particles responsible for electrostatic phenomena'. The two important things to note are first that the quantity of theory employed in fixing the extension is small and its content is general, not specific. The sense of a theoretical term is 'thin', not 'thick' as in Kuhn's account. The rest of the theory of electrons may change without changing that part of the theory that is responsible for the sense of 'electron'. Hence there is no reason to suppose that a change in theory must lead to a change in sense. And if there is no change in sense there will be no change in reference either. The Continuity of Reference thesis remains intact. Secondly, the Significance of Reference

[37] Kuhn (2000), pp. 77–86; Kuhn (1990).

thesis still holds. Reference is what matters in theory comparison. Hence there is no problem raised for theory comparison by theory change.

I have suggested that Kuhn's attack on causalism fails and that even if the causal theory is wrong for other reasons, the thesis of incommensurability is still undermined by subsequent developments. But it is also important to put this exchange into context. Kuhn regarded the incommensurability thesis as part of his attack on positivism. Does the rejection of the incommensurability thesis mean a return to positivism? In fact, quite the opposite is the case. It is only because Kuhn has retained a key positivist thesis—the theoretical-context view of meaning—that the incommensurability thesis can even get off the ground. It is the Significance of Reference thesis that is genuinely anti-positivist.

6. Kuhn and contemporary philosophy of science

The developments discussed in the preceding two sections highlight two important features of the relationship between Kuhnian thinking and contemporary philosophy of science. First, a distinctively Kuhnian thesis, one that marked philosophy of science out as having problems different from the rest of philosophy, was undermined by central developments in metaphysics and the philosophy of language. Secondly, we can see Kuhn's unacknowledged positivist genealogy at work. Kuhn's incommensurability thesis had two components: (i) a thick conception of theoretical meaning as informed by much of its theoretical context, and (ii) an emphasis on meaning over reference; both of these views he inherited from his positivist predecessors. The theoretical-context account of meaning was developed by Carnap and others (in the form of the double-language model) as a response to their failure to provide a reductionist account of theoretical meaning (whereby all theoretical meaning was to be analysed in terms of observational expressions). Kuhn took this on board, adding the thesis of theory-dependence of observation.

Kuhn's disdain for reference is also inherited from the positivists. Kuhn does use the term 'reference'. But his use is very weak. If Kuhn meant by 'reference' what is normally meant by it—a connection between a linguistic expression and some independently existing individual entity or kind (such as the relation between a name and the person named)—then he did not consider the consequences of such a usage carefully. For he says that *both* Newton and Einstein referred using 'mass', but to *different* quantities. What reason is there for supposing that there are two different kinds of mass (Newtonian and Einsteinian) out there to be referred to? And in any case his assertion seems at odds with his own view that the meaning of 'mass' is determined *thickly* by the relevant theories, including the competing claims that mass is conserved and that mass is not conserved. If the two theories are genuinely incompatible, they cannot both be true. In which case, as regards the false theory (Newton's, say) 'mass' would be a term whose reference is fixed by a sense that incorporates a false set of claims—Newtonian 'mass' would be like 'phlogiston', a term that fails to refer at all. On the other hand, we might plausibly interpret Kuhn's use of 'reference' as meaning some sort of 'internal reference'—reference is not

reference to an entity existing independently of the theory; rather, what is referred to is the hypothetical entity posited by the theory (which might not actually exist). This would make Kuhn's remark about the shifting reference of 'mass' less prone to immediate difficulty. But note then that 'reference' does not have a meaning very far from the meaning of 'sense' or 'intension', in which case the thought that reference changes when theories change no longer has the dramatic appearance it initially had. (In particular it does not conflict with the Continuity of Reference thesis, which is directed to the external sense of 'reference'.) And although there may remain a weak kind of incommensurability—terms from different paradigms may not be perfect translations for one another—any more radical thesis concerning theory comparison remains unproven.

The thought that there isn't any 'real' (external) reference links to Kuhn's Kantian scepticism concerning the world-in-itself. This parallels the positivists' desire to avoid any commitment to theoretical entities, as witnessed by their original efforts to analyse all theoretical expressions in terms of observational ones. Even with later developments such as the double-language model and the theoretical-context account of meaning, the positivists did not think that theoretical terms ever referred to anything. The issue of reference (in the normal, external sense) as the basis of theory comparison just never arose for them. So Kuhn shared the positivists' aversion to the reference of theoretical terms: where he differed from them was in his insistence that because observation is theory-dependent the reference of even observational terms is not established in a shared way for all scientists.

Thus the resurgence of reference and the arrival of the causal theory and its successors represented a far more radical departure from positivism than anything Kuhn proposed. Kuhn's incommensurability thesis can be seen as positivism plus the theory-dependence of observation.[38] From this angle, for all the apparently dramatic implications of incommensurability, it can be seen as decidedly conservative and old-fashioned in its commitments. The same positivist legacy in Kuhn's thinking may be seen in his more general epistemological outlook. Here too, as I have suggested, Kuhn's version of relativism and scepticism, and his rejection of knowledge as the aim of science, show an adherence to empiricist and Cartesian thinking, even though he considered himself as reacting against these traditions.

7. Kuhn, philosopher manqué

It is worth asking why Kuhn thought he was rejecting positivism when from the perspective of the last two decades of the century it was clear that he endorsed so much of positivism and the Cartesian-empiricist tradition of which it is a part. Part of the reason is that he was a product of his times, and that those who instigate a revolution are not always able to complete it. Kuhn himself describes Copernicus as the instigator of a revolution the conclusion of which he would have been unable

[38] Newton-Smith (1981) promotes this view.

to recognise. The same may be true of Kuhn. But in Copernicus' case the revolution was only completed by Newton over one hundred years after Copernicus' death. The revolution against empiricism and positivism was well under way in the 1980s and yet Kuhn rejected it. Kuhn resisted Putnam and Kripke's causalism and took nothing from parallel developments in epistemology. If he was genuinely anti-positivist, why did he not embrace these changes? A partial explanation may be a natural inclination to defend the edifice one has erected. One would expect Kuhn to defend the incommensurability thesis with which his name was so closely associated. But, I suspect, other factors were at work.

First, Kuhn was not trained as a philosopher. As mentioned, while an undergraduate Kuhn took a history of philosophy course (finishing at Kant).[39] He reports that as a graduate student in physics he took philosophy courses for a semester which he found unfulfilling (he describes them as 'undergraduate chicken-shit'[40]). There is little evidence that he knew or understood the details of the philosophical heritage he was working in and against. There are very few footnotes in the first edition of *The Structure of Scientific Revolutions* that refer to philosophers (but many to historians)—and the few references that there are are to kindred spirits such as N. R. Hanson. There are no references to Schlick, Carnap, Feigl, Nagel, Hempel, or to any other positivist. My suspicion is that Kuhn's conception of positivism could be summarised in two claims: (i) positivism takes observation to be foundational both epistemologically and semantically, and (ii) positivism is optimistic about the epistemology and history of science: scientific knowledge is achievable, and the history of science is one of increasing knowledge and improving proximity to the truth. Kuhn would have been right in thinking the former a characteristic thesis of positivism, even if the latter is a thesis acceptable to many non-positivists. But positivism embraces rather more than these two claims. And so Kuhn would have been wrong in thinking that a standpoint that rejects both theses is thereby a root-and-branch rejection of positivism. The theoretical-context account of meaning is also a characteristic positivist thesis, but Kuhn embraced it all the same. Had Kuhn known the positivist literature better, he would have known that the theoretical-context account of meaning is a product of the positivists' observational foundationalism. Correspondingly, he would have seen referentialism (the causal theory and its successors) as a further, very deep attack on positivism. Instead he noticed, I conjecture, only the fact that referentialism allows for theory-comparison, undermining incommensurability. It thus permits one to re-adopt the second part of Kuhn's conception of positivism, the optimism about scientific knowledge. From Kuhn's perspective referentialism must have looked like a return to (an important aspect of) positivism. But, as I said, optimism about scientific knowledge goes well beyond positivism, and so a position that permits it cannot justly be accused of being positivist.

Secondly, although Kuhn was not trained as a philosopher he aimed to be accepted as one. He prepared and wrote *The Structure of Scientific Revolutions* while

[39] Kuhn (2000), p. 263.
[40] Kuhn (2000), p. 273.

employed as an historian of science, but, as has been mentioned, he wrote it as history for philosophical purposes. '[M]y objectives in this [working in the history of science] were to make philosophy out of it', he reports; 'I wasn't going to go back and try to be a philosopher, learn to do philosophy. . . But my ambitions were always philosophical. And I thought of *Structure*. . . as a book for philosophers'.[41] While that book has famously been hugely influential in all sorts of fields, it was the interest among professional philosophers that Kuhn most appreciated. (Correspondingly, Kuhn was clearly very hurt by his treatment by the senior philosophers at Berkeley, around the time of the publication of *Structure*, who were happy for Kuhn to be promoted—but in the history department rather than in philosophy.[42] Shortly afterwards Kuhn left for Princeton.)

I believe it was Kuhn's desire to be accepted among philosophers, to be taken seriously among philosophers, that led to his subsequent lack of emphasis on the empirical, naturalistic side of *The Structure of Scientific Revolutions*, replacing its appeal to the discoveries of empirical science with philosophical, *a priori* theses. His use of gestalt psychology in the book had not been well received or well understood for a number of reasons. First, philosophers such as Lakatos held that the gestalt parallel made scientific change mysterious and irrational. Secondly, Kuhn himself was not sure quite what the significance of gestalt psychology was for his own theses concerning paradigms. I have suggested that Kuhn would have done well to stand his ground; but his own uncertainty regarding the psychology, plus the lack of a theoretical underpinning for it, meant that it was difficult for him to do so comfortably and convincingly. A psychological conception of incommensurability gave way to a linguistic one while a psychological conception of theory-dependent observation and world-change were replaced by a neo-Kantian one. In both cases a naturalistic thesis was supplanted by a philosophical thesis. One result of this shift was that Kuhn's most important contribution, the thesis that scientific activity can be explained by the immersion of scientists within a paradigm and their adherence to an exemplar, and that this activity can be (in part at least) rule-less without being irrational, was no longer the main focus of Kuhn's work from the mid-1970s, after which the philosophically re-cast thesis of incommensurability was the centre of attention. The loss of interest in the paradigm (exemplar) concept is explained by a combination of three facts: first, that it was so widely misunderstood and misappropriated; secondly, that there was no obvious philosophical replacement for a clearly socio-psychological hypothesis; and, thirdly, the details of the psychology could only be worked out in a way that, as Hoyningen-Huene pointed out, would be in tension with Kuhn's newly adopted philosophical neo-Kantianism. Extraordinarily, in his paper 'The Road Since Structure'[43] Kuhn makes no mention at all of paradigms, nor of exemplars nor disciplinary matrices, despite having stated in the second edition of *The Structure of Scientific Revolutions* that the idea of paradigm-as-exemplar was

[41] Kuhn (2000), p. 276.

[42] Kuhn (2000), p. 302.

[43] Kuhn (2000), pp. 90–104. This paper was delivered as Kuhn's presidential address to the Philosophy of Science Association in 1990.

the most novel aspect of that book. Instead the paper is devoted to incommensurability (about which he was writing a book that was never finished) and to evolutionary epistemology, culminating in his self-ascription of post-Darwinian Kantianism.

8. Conclusion

Kuhn was a physicist turned historian who wanted to become (and became) a philosopher. This had several effects. The most positive effect was that it enabled him to be an iconoclast. As Kuhn himself notes it is often the newcomer to a field who initiates the revolution that finally overthrows the existing paradigm. Kuhn himself played such a role with regard to a basic component of the positivist view—the independence of observation from theory. The second and third effects were, in my view, less constructive. Lacking a proper philosophical training Kuhn could not see the historical and argumentative roots of the philosophical positions he was dealing with, whether they were ones he rejected or ones he held onto. So he rightly identified the primacy and independence of observation as a positivist thesis, and rejected it. But he was happy to hold onto an anti-referentialism and a theoretical-context view of meaning, even though these too were products of positivism. The third effect was to encourage Kuhn to replace the empirical arguments for his views with 'philosophical' ones. This meant that he turned away from a naturalistic approach that would today be seen as genuinely post-empiricist. The philosophical position Kuhn adopted instead had much more in common with the epistemology of the Cartesian-cum-empiricist tradition that he saw himself as objecting to than with epistemology in the twenty-first century.

Thus although Kuhn himself may be credited with having been a prime cause of the downfall of positivism, there were many aspects of his thought that contained a significant residue of that positivism, as well as of empiricism more generally. Thanks to Kuhn in part, philosophy in general has repudiated much of empiricism and even Cartesianism, but in doing so has not only rejected those views that Kuhn himself explicitly criticized but has also gone further by rejecting other empiricist and Cartesian assumptions that he implicitly endorsed. Consequently, philosophy, having freed itself of the commitments of positivism in particular and of empiricism more broadly, has been able itself to provide a basis for the philosophy of science that no longer conflicts with the lessons of the history of science. It is possible therefore to see Kuhn not as providing a new paradigm in the philosophy of science; his achievement, rather, like the achievement of Copernicus, lay in inaugurating a revolution that, because of its significance, went beyond what he himself foresaw.

However, my conclusion is *not* that Kuhn's only lasting contribution to the philosophy of science is (or ought to be) his early (if only partial) undermining of positivism. Kuhn's other, and potentially more important contribution is his conception of paradigms as exemplars: concrete, shared problem solutions that help direct the thinking of scientists in a manner that cannot be reduced to the following of universal rules of rationality. Connectionist thinking has tackled simple 'one-step' rule-less cognition (such as cases of visual pattern recognition) with success. Much work is

yet to be done on abstract pattern recognition or on 'multi-step' processes of reasoning. The full vindication of Kuhn's work on paradigms is thus yet to come. But this application of cognitive science to scientific cognition, along Kuhnian lines, would appear to be immensely fruitful. This line of thought is clearly thoroughly naturalistic; it is a line of thought along which Kuhn himself might have made significant progress had he pursued it. But from the 1970s Kuhn turned his back on his earlier naturalism. Not only did he fail to follow this promising direction of research, but his subsequent journey up the dead end of incommensurability meant that the significance of what Kuhn did achieve in *The Structure of Scientific Revolutions* has not been fully recognised. The road since *Structure* has been a wrong turning.

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