V*—NATURALIZING KUHN

by Alexander Bird

ABSTRACT I argue that the naturalism of Thomas Kuhn's *The Structure of Scientific Revolutions*, which he himself later ignored, is worthy of rehabilitation. A naturalistic conception of paradigms is ripe for development with the tools of cognitive science. As a consequence a naturalistic understanding of world-change and incommensurability is also viable.

I

Introduction. Although it created widespread interest, Kuhn's *The Structure of Scientific Revolutions* was poorly received by philosophers. From the philosopher's point of view a particularly unusual feature of this book—for its time—and one which caused much of the negative response, was its naturalism. The aspect of Kuhn's naturalism that elicited most attention was his use of historical evidence. Rather than draw upon an *a priori* account of how science ought to develop in order to explain how it does develop, which in effect was the positivist approach as well as that of Popper, Kuhn inspected the historical record and there identified the now well-known pattern of normal science, crisis, and revolution. Furthermore, he explained this cycle by reference to the mechanism of the paradigm. That explanation was both social and psychological. The use of psychological examples in *Structure of Scientific Revolutions* is striking. Kuhn refers to Gestalt psychology, to experiments carried out by his erstwhile Harvard colleagues, Jerome Bruner and Leo Postman, and even to a computer model of judgments of perceived similarity. The familiarity of such elements in philosophy today contrasts with their novelty forty years ago, a novelty that was not well received. Equally striking is the lack of reference to the work of philosophers, with the exception of a few references to those such as Hanson, whose views informed or coincided with Kuhn's own.

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Extraordinarily, for a book that has been regarded as pivotal in the downfall of logical positivism, there are almost no references to the work of any positivist or empiricist philosophers at all.[1]

As Kuhn's career developed, his work became more recognizably philosophical. There is more emphasis on linguistic phenomena and Kuhn's attempts to explain incommensurability drew on familiar work in the philosophy of language—starting with Quine and Wittgenstein. At the same time, Kuhn did not drop his interest in the nature of perceptual experience, but in his later work, especially in the 1980s, his ideas are framed in terms of neo-Kantianism rather than by reference to Gestalt psychologists. Just as there is a more overtly philosophical tone in his work, there is a corresponding diminuendo in the naturalistic, psychological elements. Although incommensurability remains, explained in terms of language and translation or in terms of Kantian categories, the notorious paradigm concept is dropped. Even the naturalism of the famous historical turn is downplayed. As Kuhn wrote in 1991, 'It is taking still longer to realize that... many of the most central conclusions we drew from the historical record can be derived instead from first principles.'[2]

My own view is that Kuhn's increasingly philosophical and anti-naturalistic approach was in part caused by the negative reaction his work caused among philosophers in combination with the desire to be accepted in the philosophical community.[3] When he wrote *The Structure of Scientific Revolutions* he was an historian. But in due course he became employed as a philosopher. And the opinion of many seems to be that his early work, although exciting, is wildly wrong, whereas his later work, if less dramatic, is more respectable.[4] As regards the truth of his views, this is a mistake—I regard the later work as containing little of lasting value whereas the first edition of *The Structure of Scientific Revolutions* contains important insights that have long

1. I refer to the first edition. The only referenced point of disagreement with a philosopher is to Popper (Kuhn 1970; 146).

2. Kuhn 1992, 10.

3. Bird 202.

4. E.g. Newton-Smith 1981; 102–124.

been overlooked. Kuhn's turn away from naturalism was largely sterile. Now that naturalism is familiar to philosophers the time is ripe for a re-evaluation of the earlier work that Kuhn turned his back on.

In this paper I shall give an overview of Kuhn's early naturalistic account of paradigms, before going on to suggest how his insights might be developed in the light of recent work in cognitive science.

II

The Function of Paradigms. The term 'paradigm' has typically been interpreted as a sociological concept, designating a certain kind of consensus. In the Postscript 1969 to the second edition of *The Structure of Scientific Revolutions* Kuhn admits that there is indeed a broad sociological sense of paradigm. He introduces the term 'disciplinary matrix' to capture this concept. But, he says, the core idea is something different, it concerns the object of consensus. In this sense, paradigms are, Kuhn said, the most novel and least understood aspect of his book.[5] This is the sense of paradigm as *exemplar*, of which use of the term Kuhn remarked, 'By choosing it, I mean to suggest that some accepted examples of actual scientific practice—examples which include law, theory, application, and instrumentation together—provide models from which spring particular coherent traditions of scientific research.'[6]

The fact that Kuhn took the exemplar idea to be more important than the disciplinary matrix idea is revealed not only by the above-cited comment but also by the fact that he never mentioned the latter notion again. Exemplars are social in that they are shared exemplary illustrations and puzzle-solutions, and trivially it is that by being shared and hence social that they can explain the development of a social activity such as science. But the key innovative feature of the exemplar idea is not so much the social as the psychological function of exemplary puzzle-solutions.

5. Kuhn 1970; 187.
6. Kuhn 1970; 10.

Kuhn's central claims about the functioning of paradigms are as follows:

(i) Selecting puzzles, solving them, and assessing the quality of a proposed puzzle-solution are driven by perceived similarity to exemplary puzzles and their solutions (paradigms);
(ii) The ability to perceive such similarities is acquired by training with paradigms;
(iii) That ability is primarily an ability to recognise patterns and relevant similarities;
(iv) It is therefore not an ability that is mediated by following rules.

It had been widely thought that in order for science to be rational scientists must follow certain *rules* of rationality, at least in the so-called context of justification. Optimally philosophers of science should be able to reconstruct the *a priori* rules of scientific method by which our theories are justified and hence accepted. Kuhn himself rejected the sharp distinction between the context of discovery and the context of justification since paradigms play a role in both (see (i) above). Moreover, justification and the explanation of scientific change do not depend on scientists following, even unconsciously, the rules of scientific method. Rather, what drives theory-choice is perceived similarity to an exemplar. This is most clear during normal science but is true also even in revolutionary science.

Kuhn compared normal science to solving puzzles such as crossword puzzles. One feature of puzzle-solving to which Kuhn sought to draw our attention is the fact that one can learn to do crossword puzzles quicker and more easily simply by practising them. Although one can write very effective computer programmes that play chess by implementing an algorithm, good human players recognise patterns in positions in a non-algorithmic way that is acquired by past exposure to many similar positions. Kuhn also likens practice with scientific puzzles to finger exercises on the piano or other musical instrument.

The most obvious cases of learning by repeated exposure and practice in science will be found in the mathematical sciences where students learn from a textbook and lectures and are given problems to solve. The easier problems will be very similar to the problems that are solved in the textbook and lectures. Harder problems will be less obviously similar. Practice with the former will give a student an improved ability that will in due course allow them to tackle more difficult problems. As suggested, what makes a difficult problem more difficult will be not just increased complexity but also the fact that it is less clearly similar to a problem the student has seen before. What, according to Kuhn, repeated practice with exemplary puzzle-solutions provides is an ability to spot a similarity between a new puzzle and one that the student has seen before.

Although we are most familiar with the idea of a non-rational power of recognising similarity in the perceptual sphere, for example, recognising the similarity between faces of members of the same family, or hearing that two tunes are similar, it is also true that we can recognise similarities between more abstract patterns and structures and this similarity recognition is likewise a non-rational one. Of course, to say that this capacity is non-rational is not to say that it is irrational. It is clearly not irrational to be able to spot the similarity between a mother and her daughter. Rather what is meant is that this capacity is not mediated by a process of following rules.

Clearly this is a naturalistic approach to understanding how scientists solve scientific problems. Whether it provides genuine understanding of what occurs in puzzle-solving depends on confirmation by psychologists and cognitive scientists. Kuhn did draw on some psychological evidence. First he mentions Gestalt images. Clearly, recognising a duck-rabbit as a duck involves a recognitional capacity. But, famously, Kuhn was more interested in using these images as illustrations of what occurs in a scientific revolution. In effect Kuhn asserted the following conditional: *If* puzzle-solving is a matter of pattern-recognition, like recognizing the duck in a duck-rabbit, *then* a scientific revolution involves a change in such a capacity, so one can recognize new patterns or similarities, just as one can switch to recognizing the rabbit in the duck-rabbit. However, Kuhn admitted to not being sure whether this is just an analogy or it shows us something important about the working of the mind common to puzzle-solving and the Gestalt cases. This was because Kuhn was in no position to provide direct psychological evidence in favour of his hypothesis; nor was he able to point to anything like an underlying mechanism that might support both phenomena. Indeed, the dominant thinking in artificial intelligence (AI) at the time, the classical computational, symbolic approach to cognition, went directly against the thrust of Kuhn's claims. Consequently, Kuhn was unable to establish the truth of the antecedent in the above-mentioned conditional. Nonetheless, cognitive psychology and cognitive science today provide a much more congenial environment for the development of Kuhn's ideas, as we shall now see.

III

Developing the Naturalistic Paradigm Concept. In this section I shall say more about developing the naturalistic conception of paradigms, in particular in the light of research in cognitive science and psychology. I will argue that this in turn can illuminate what was right in Kuhn's talk of world-change and incommensurability.

<u>Case-Based Reasoning</u> Kuhn's disadvantage in promoting the paradigm view of scientific research was that it went against both philosophical and psychological/AI wisdom of the time. The latter was dominated by the expert systems approach to problem solving, which encapsulated the rule-based approach that Kuhn rejected. Early AI started with systems that employed general rules, such as rules of logic, but were poor at solving problems. Later systems incorporated subject-specific knowledge. Either way, problem-solving is a matter of applying rules. Long after Kuhn had given up an interest in the functioning of paradigms and exemplars, Case-Based Reasoning (CBR) emerged as a very different approach in which reasoners employ known solutions to old problems to help them solve new, different problems. As David Leake puts it, 'A case-based problem-solver … solves new problems by retrieving traces of relevant prior problems from memory, establishing correspondences between those problems and the new situation, and adapting the prior solutions to fit the problem at hand.'[7] CBR is clearly exemplified by the kind of learning that Kuhn was interested in. As Leake makes

7. Leake 1998; 465.

clear, CBR is essentially *analogical* reasoning. It is difficult to see how drawing an analogy could be rule-governed. On the contrary, drawing an analogy is clearly a matter of seeing relevant similarities of just the kind that Kuhn says are learned by practice with exemplars.[8] Thus Kuhn's account can be seen as an instance of CBR, where repeated practice with well-chosen examples will give the young scientist not only a stock of cases and their solutions but also trains the young scientist to be able to see the analogies between those cases and new problems and so apply and adapt the old solutions to the new problems in an effective manner.

Although it is clear that we do reason analogically, most CBR research has focused on developing computer models, stimulated by shortcomings in expert-system and the earliest script-based approaches. CBR models have yet to be applied to the complexities of scientific reasoning. Nonetheless, that analogical reasoning is important in science is borne out by independent research. The psychologist Kevin Dunbar analysed extensive data gathered over many months from the laboratories of four leading molecular biologists. He concluded that analogical reasoning is central to the way scientists actually reason.[9] The analogies are both local and regional. A local analogy is employed when, for example, a technical problem is overcome by adapting a successful solution to a similar problem in the same field. Regional analogies are employed when 'scientists mapped entire systems of relationships from one domain onto another.'[10] The two domains are distinct but related (e.g. phage viruses and retroviruses). Dunbar's work both corroborates Kuhn's claims concerning the functioning of paradigms as exemplars and also points towards the potential fruitfulness of applying CBR to science.

<u>Complex scientific reasoning</u> Student exemplars are unlike real research problems. For one thing, they are much simpler. Furthermore, research suggests that they can inculcate problem-solving skills that are limited in scope without providing real

8. The relationship of CBR to reasoning in Kuhnian normal science is discussed in Nickles 2003.

9. Analogical reasoning, in science in particular, is discussed in detail also in Holyoak and Thagard 1995.

10. Dunbar 1996; 382.

insight into the underlying principles of the field in question.[11] So the straightforward case of textbook problems needs to be expanded to include the sort of learning that a student acquires as a graduate. It is not difficult to see how this extension should be possible, since what a postgraduate scientist gets to see is real puzzle-solving in action.

A more important problem is that even for the undergraduate, seeing the answer is not typically just like recognizing a face or suddenly seeing the duck-rabbit as a duck. On the contrary, solving such puzzles involves cogitation—the extended process of thinking and reasoning. We need to ask therefore, how does the extended, multi-step process of reasoning that is involved in genuine puzzle-solving relate to the rather simple, one-step recognition of similarity?

Pattern recognition in science may operate at more than one level.[12] First, let us consider that a process of reasoning (however construed) leads one to see the objects of thought related to one another in a structure of ideas (which we may take to be a mental model). For example, one's reasoning might tell one that this hypothesis with a certain structure, explains such-and-such evidence but not that evidence, for these reasons etc. It might be that recognition of similarity applies to these structures of ideas as wholes. So just as one recognizes a face as a whole by perceiving the spatial relations of eyes, nose, mouth, and so on, one recognizes a puzzle or puzzle-solution by understanding the abstract relations of its parts. The process of reasoning delivers that understanding. So the process *starts* with reasoning, which allows us to detect an abstract structure, *then* Kuhnian pattern recognition allows us to see the similarity of this abstract structure to others we have encountered before. Something like such a picture is implicit in the CBR approach. It may be then that reasoning (however construed) is required again to provide the precise answer, once the pattern recognition process has delivered the procedure required for a solution.

It would be sufficient for the truth of the central component of Kuhn's thesis, that pattern-recognition takes place in scientific

11. Cf. Nersessian 2003; 189.

12. I sketch the following proposals in Bird 2000; 90–95.

cognition at the large-scale as just described. However, it is useful to see that it may take place even at the most basic level, for example in the drawing of inferences in the process of reasoning. In such cases what is recognized is a pattern of relations between premise propositions and a conclusion proposition. A process of extended reasoning, cogitation, would involve sequential pattern recognition. The benefits of extending the pattern-recognition approach are first that a promising model of how reasoning is implemented in a connectionist system, as we shall see, involves interaction between modularised neural networks which embody pattern recognitional capacities at both the large and small scale. Secondly, it is plausible that what is learnt when learning with scientific puzzles is not only an ability to recognize overall patterns, but also patterns at the level of detail.

At the level of detail it may not be particularly useful to contrast pattern-recognition with rule-following. For example, in some cases the patterns will correspond to familiar rules of logic, such as *modus ponens*. Applying the rule will require spotting a pattern (e.g. to apply *modus ponens* requires seeing that the premises have the form $P \rightarrow Q$, P). In other cases there is a transition over time and as a result of repeated use from an inference using an explicit premise to inference using the same premise but as a tacit assumption that can now be regarded as hard-wired. Such tacit assumptions are acquired in the process of learning paradigms. For example, proofs in physics will frequently fail to state commonly known truths such as '$\mathbf{F} = m\mathbf{a}$' or '$\sin\theta \approx \theta$ for small θ' even when they are essential to a proof. The explicit and frequent use of an assumption will establish a neural pathway which can continue to operate without mention of the assumptions. So one can argue thus:

(i) $d^2\theta/dt^2 = -k^2 \sin\theta$
(ii) $d^2\theta/dt^2 \approx -k^2\theta$ for small θ
(iii) $\theta \approx a \sin kt + c$ for small θ

without the need for considering the intermediate assumptions that a beginner must use explicitly.

In yet further cases, the exemplars themselves make tacit assumptions, so the learner learns a patter of inference that in formal terms is invalid (but which may well be contingently truth-preserving). Someone could acquire the disposition to move from propositions like (i) to those like (ii) or (iii) without having explicitly considered the justifying assumptions. Merely seeing a step repeated many times and repeating it oneself might be enough to make this transition second nature.

In all these cases, it is possible to represent the inferences as applications of general rules of logic to explicit premises plus enthymemes (which express the tacit assumptions). Nonetheless, it is implausible to regard such a representation as modelling cognitive reality. For example, most of us learned calculus for the first time employing an incomplete and logically inadequate justification (the one correctly criticized by Berkeley). There is no reason to suppose we unconsciously supply the missing steps that Cauchy was the first to provide explicitly. We find the simplified justification intuitively satisfactory and exposure to that pattern of thought soon makes the relevant transitions second nature.

Even if vague, the tacit-explicit distinction is significant. Its significance lies in the ease or difficulty with which the subject can consciously recognise or recover the assumption in question. To represent an inference as employing an enthymeme that the subject cannot himself consciously supply and may not event readily assent to is misleading. It is particularly misleading in the current context, because the difficulty in recovering a tacit assumption is part of what is involved in explaining incommensurability. For example two scientists may draw incompatible conclusions from the same data. If this occurs because they are using different assumptions that are explicit, then they will easily be able to see where their differences lie. There may be disagreement, but there will be no problems of understanding. If, however, the assumptions are not easily recovered, and perhaps have never been explicit to the subjects, then it may appear to each that the other is being irrational, employing an inference which makes no sense. The difficulty in recovering a tacit assumption is related to the difficulty involved in replacing it by another assumption (explicit or implicit), which in turn may explain some of the difficulties involved in revolutionary scientific change. Plausibly the extent to which a tacit assumption is difficult to recover and to replace is related to the degree to which, at the level of neural implementation, it is hard-wired.[13]

So pattern-recognitional capacities may play a part in the complex process of scientific reasoning at two levels. First, at the local scale the basic processes of reasoning may involve pattern recognition, even when they involve conscious applications of rules, but also when an inference pattern is employed that embodies a tacit assumption. Secondly, the processes of reasoning about a problem will deliver an understanding of the problem in terms of an abstract structure or mental model. So pattern-recognition can play a role at the global level in the perception of similarities between such structures or models. As we shall now see these hypotheses receive corroboration from an elaboration of the connectionist approach to the neural architecture of pattern-recognition.

<u>Connections with connectionism</u> Since the Kuhnian picture relies on pattern-recognition in place of rules, it is natural to appeal to connectionism (neural network models, parallel-distributed-processing (PDP)) as showing how it is realised in the scientist's brain. This need not be a commitment of those seeking to naturalize Kuhn. They may be content to work with the following commitment: whatever it is that explains pattern-recognition in general will be what explains puzzle-solving in normal science. If that is connectionism, so be it; if something else, then that is fine too. This is the approach taken by Howard Margolis, who proposes pattern-recognition as the basis of all cognition.[14]

Nonetheless, the aptitude of PDP for pattern-recognition makes it worth exploring its capacity to handle scientific puzzle-solving. I surmise that the lack of a model, such as PDP, for

13. This relationship may be somewhat rough. Hard-wiring is best associated with ease of replacement rather than ease of recovery. Recovering an assumption may be no guarantee that it can be replaced. If the Müller-Lyer lines demonstrate something like a tacit assumption in the visual system, this assumption is not replaceable, even when consciously rejected. It is perhaps less easy to imagine a purely propositional equivalent.

14. Margolis 1987. As I do, Margolis applies pattern recognition to scientific thought and also uses it to explain scientific revolutions. This paper can be regarded as a contribution to Margolis's programme of explaining all judgment in terms of pattern recognition.

implementing Kuhn's idea was one reason why it was rejected by philosophers as irrationalist or even mysterious. This is especially so when classical AI with its symbolic computational approach to cognition seemed so much more obviously a candidate for modelling the reasoning found in science.

It is noteworthy that connectionists have difficulties in precisely the same place that the Kuhnian model has, as outlined above. As Andy Clark puts it 'PDP seems to be nature's gift to pattern recognition tasks, low-level vision, and motor control. But as we proceed to higher, more-abstract tasks the PDP approach becomes less and less easy to employ.'[15] And scientific problem-solving seems to be the exemplification of a high-level abstract task par excellence. The fact that scientific reasoning is a multi-step, sequential, largely conscious process makes it difficult to see how it can be captured by a model designed to account for one-step, more or less immediate events of pattern recognition with no conscious sub-events or processes.

Nonetheless, connectionism *can* account for sequential thinking. Let us start with the most simple case. Consider someone carrying out an arithmetical calculation using paper and pencil. This is a sequential task that is constituted of the repeated application of simple rules to the marks on the paper. Each application of a simple rule can easily be modelled à la PDP. In each case there is an external input, the marks on the paper, and an external output, new marks on the paper. What is important is that the output from one stage becomes the input for the next stage. This sequential process can be accounted for as a iterated application of pure PDP. Now consider the same calculation carried out as an exercise in mental arithmetic. In such a case the sequential reasoning is just as in the pencil and paper case, except that where the latter has marks on paper the latter has the result stored in short-term conscious memory. So the iterated PDP is the same with the only difference being that the output and input for each stage are within the brain rather than outside it.

There needs, however, to be more to the PDP account of sequential thought. For the sequence of steps must be appropriate. The sequences will typically not be purely recursive,

15. Clark 1989; 127.

repeating the same operation, but will usually involve a variety of operations. An operation at one point in the sequence will take the outputs of previous, *different* operations as their inputs. This will be true even for the operations involved in a simple arithmetical calculation. In a long multiplication, some steps will be additions, some will be simple multiplications, some involve carrying. So there must be some mechanism that puts in place a sequence of operations appropriate for the task. Some PDP theorists suggest that we employ mental models to enable us to do this. A mental model, in this context, is just another network, one which deploys the various required operations. The reason for calling them models is that we may think of at least some of them as modelling external, physical patterns. In the case of multiplication the mental model replicates the sequence of operations involved in doing multiplication with pencil and paper. Nonetheless, it does not seem necessary that every mental model should replicate a physical process. In the case of physical exemplars, such as paper and pencil multiplication, we acquire the model by seeing the exemplar. But in other cases we will acquire it from an abstract source, such as a worked exercise in solving a scientific puzzle. Thus we will acquire mental models in science through the training process that Kuhn describes. Then we will need to know which model to apply to solve a problem. Sequences of operations, and so the abstract structures of models, have patterns. Thus a higher-level pattern-recognitional capacity will have the function of recognising a problem as requiring a certain model in order to solve it.

Now this account is a departure for pure PDP in two respects. First, what was external to the processing system, the marks on the paper, has become internal, stored in memory. To that extent this model represents a move back toward classical cognition, which keeps the processor and data distinct. Secondly, the thought that a mental model governs the use of different tasks, resembles the modular, computational approach of a main programme deploying subroutines. Clark himself sees no reason to adopt an extremist position on either side of this debate, and in any case the concession to classical cognition is limited. More importantly, we can see how a system that employs PDP as its basic architecture can account for sequential thought. Consequently we can see that Kuhn's leading idea (and more explicitly that of Margolis), that scientific puzzle-solving is *au fond* an exercise in pattern recognition, is consistent with the obvious fact that scientific discovery involves sequential thought. Above I hypothesized that Kuhn's insight could operate at two levels: firstly at the level of individual steps in reasoning, secondly at the level of large-scale abstract patterns. The PDP solution to sequential thought corroborates both aspects of the hypothesis. As we have seen, sequential thought is taken to be a sequence of simple applications of PDP pattern recognition, governed by a mental model, whose appropriateness and whose application to the problem in hand is determined by the large-scale pattern recognition.

World-changes as psychological changes So what is the psychological reading of world-change and incommensurability promoted in my approach? We have seen that learning with exemplars gives a subject a range of cognitive capacities and habits of mind, such as inferential habits, that are not reducible to the possession of conscious or near conscious rules. A person's world can be understood as her set of entrenched pattern-recognitional capacities and habits of mind, including therefore a subject's tacit assumptions.[16] These together I have called a subject's quasi-intuitive associations. A world-change is a change in such capacities, assumptions, and associations generally. Because exchanging one large-scale pattern-recognitional capacity for another is difficult, because it is often difficult to realise that one's thinking is governed by a particular pattern-recognitional capacity, because it is difficult to retrieve one's tacit assumptions, and because it is difficult to shift entrenched habits, significant world-changes will be relatively rare. A revolution might be primarily a matter of uncovering these associations and showing them to be false. (The Galilean and Einsteinian revolutions did just this.) That said, entrenchment and difficulty of retrieval are a matter of

16. I believe that this conception of world captures part of an everyday usage of the word 'world'. What makes the 'world of the poet' different from the 'world of the professional footballer' is a difference in expectations, shared assumptions, and other psychological states characteristic of the two professions. The everyday term also has a sociological connotation which may also be appropriate to the Kuhnian 'world' concept.

degree and one might expect less significant world-changes to occur more frequently. Independently, it has been suggested that Kuhn's model of change needs to take into account localised 'mini-revolutions'.

Worlds and world-changes thus understood are psychological. Such an account brings with it a psychological conception of incommensurability, which arises from a difference in worlds—a difference in pattern-recognitional capacities and cognitive habits—between subjects.

One kind of incommensurability will arise when one subject can see a large-scale abstract pattern where another cannot. The second will consequently not be able to see why a solution to a problem that is based on seeing that pattern is a reasonable solution to that problem. Howard Margolis has analyzed in detail the components of the abstract pattern that was Ptolemaic astronomy and the habits of mind this gave rise to, and why this made it difficult for many to see Copernican astronomy as plausible alternative pattern.[17]

In such a case a theoretical claim may be entrenched as part of a larger pattern, and so can be entrenched while being conscious and explicit. In other cases, incommensurability arises from a failure to share tacit assumptions and inferential habits. For an important feature of one's ability to communicate with others in one's community is the sharing of unspoken assumptions and background knowledge. Someone who does not share these assumptions may find it very difficult to comprehend what another is saying. In such cases there is no problem in understanding the literal meaning of the words used. The individual sentences make sense. What is not clear to one subject is why it was reasonable for another to utter them. What makes them reasonable are the unstated, tacit assumptions made by the speaker. Sometimes such assumptions can be revealed reasonably quickly. In other cases they may be difficult to uncover. An obvious case will be that of ancient texts where one cannot reveal the assumptions by interrogating the author. In other cases the difficulty may lie in the fact that those employing the tacit assumptions are

17. Margolis 1987. Some astronomers recognized the instrumental utility of Copernicus' heliocentric model without being able to see it as a workable physical theory.

not aware that they do so. That tacit assumptions are made in communication has been widely recognized and discussed by linguists.[18] Often their focus has been on the transition between the words uttered and their literal meaning, as in cases of potential ambiguity that are nonetheless easily understood univocally thanks to appropriate assumptions. However, linguists also note that what is meant, in a broad sense—what the speaker intends to communicate—can go beyond the literal meaning of what is said, and that this transition too is mediated by tacit assumptions.

On this view, full understanding requires more than a knowledge of the literal meaning of what another is saying. An argument may make no sense even if one understands every proposition in the argument. One way in which this may occur is as follows. As mentioned, what a speaker intends to communicate often goes beyond what is literally said, where the extension of communicative intention is mediated by tacit assumptions. It follows that a reader of a text who lacks the relevant tacit assumptions is in danger of not understanding all that the writer intended to communicate. Furthermore, when a sentence is used as a premise in an argument, it may be that the excess of what is intended over what is literally said is a crucial part of what permits the conclusion to follow. Consequently the reader who fails to understand all that is intended may see the argument as a non-sequitur.

But it need not be that the argument works, as the writer sees it, in virtue of what he intends to communicate. It may work just in virtue of background assumptions shared by the writer and his intended audience. Above I mentioned a mathematical argument with the steps: (i) $d^2\theta/dt^2 = -k^2 \sin\theta$; (ii) $d^2\theta/dt^2 \approx -k^2\theta$ for small θ; (iii) $\theta \approx a \sin kt + c$ for small θ. The writer of the propositions (i) and (ii) does not intend to communicate unstated assumptions. Rather he merely unconsciously used them in progressing from (i) to (ii) and from (ii) to (iii). A reader who does not have access to those assumptions will not be able to follow the argument. The argument may even appear to be an irrationality, if the tacit assumptions employed contradict what the reader believes. In some cases it may be easy to recover the

18. See for example, Carston 2002; and Sperber and Wilson 1995.

unstated assumption (perhaps in the transition from (i) to (ii)); but in others it will be much less easy and will require a great deal of careful scholarship. Discovering such assumptions will enable the reader to see why an argument that seemed initially to be a non-sequitur can be rational. Something like this, I surmise, was what enabled Kuhn eventually to appreciate the fact that Aristotle was indeed a great scientist, not an irrational one.

So there are two ways in which tacit assumptions can cause incommensurability. In the first, a tacit assumption plays a part in what a speaker intends to communicate; a failure to share this assumption will lead to a failure of comprehension. In particular this may affect a hearer's ability to understand the rationality of an argument when that argument depends on what is intended to be communicated rather than on what is literally said. Secondly, the tacit assumption may enthymatically licence an inference; consequently the rationality of that inference will not be appreciated by one who does not share or know of that tacit assumption. But as we saw, tacit assumptions, which we may regard as embedded in inferential habits of mind and micro-level pattern recognitional capacities, are not the only source of incommensurability. Differences in large-scale pattern-recognitional capacities may also account for the difficult one subject has in seeing the value of what is for another a blindingly clear solution to a scientific problem.[19]

IV

Conclusion. I have already mentioned that Kuhn regarded the functioning of paradigms as exemplars as the most novel and least understood aspect of *The Structure of Scientific Revolutions*. I concur in both respects. That paradigms operate by inculcating, in a socially enforced manner, capacities for pattern recognition is, I believe, the most fruitful insight of all of Kuhn's work. This

19. There may be other kinds of association. I have elsewhere suggested that one affect of the Darwinian revolution was to change our view of ourselves and our relations to other animals. Whereas beforehand it was natural to think of humans as very different from other animals including primates, it is now natural to think of the great apes as our 'cousins'. This in turn has had an effect on our treatment of animals. The point is that we do not explicitly think 'Darwin has shown that humans and apes have a common ancestor. Therefore we should treat them as similar to us.' Rather, the effect is unconscious, colouring our view of the world (Bird 2000; 134–5).

paper has sought to sketch some of the ways in which that insight might be developed.[20]

That insight was not appreciated when *The Structure of Scientific Revolutions* was published, for several reasons. First, the naturalism of this approach was unfamiliar and antithetical to the prevailing paradigm whereby theory choice and confirmation are to be explicable in terms of *a priori* rules. Secondly, Kuhn had no theoretical backing for this view, and very little in the way of experimental evidence. He does refer to the results of psychologists, but those results, concerned primarily with perceptual effects, could at best be suggestive of the way the mind works in science and provided no direct support. Kuhn had nothing to suggest in the way of a neural architecture or other mechanism that might implement his proposal, although he does mention working with computer models for similarity judgments. Thirdly, the predominant thinking at the time concerning the underlying structure of reasoning was provided by classical AI, according to which thinking is a matter of following unconscious rules rather than recognising patterns.

In all respects matters look rather different today. Naturalism in philosophy and in philosophy of science in particular is familiar; now there is both empirical support for the view that scientists do employ analogical thinking and pattern recognition and a theoretical account, from PDP, about how this might be implemented; also, as a consequence, classical AI is not the only game in town. For these reasons the time is ripe for a retrospective reassessment of the earlier, naturalistic Kuhn, and for the future development of his ideas.

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20. It is only a sketch of possibilities, and some important areas have scarcely been touched. For example recognizing the similarity of a proposed puzzle-solution to an exemplar is supposed to be part of what explains theory-preference. But clearly empirical success is important also. So how do these factors relate?

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