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Discussion

Kuhn, naturalism, and the positivist legacy

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Abstract

I defend against criticism the following claims concerning Thomas Kuhn: (i) there is a strong naturalist streak in *The structure of scientific revolutions*, whereby Kuhn used the results of a *a posteriori* enquiry in addressing philosophical questions; (ii) as Kuhn's career as a philosopher of science developed he tended to drop the naturalistic elements and to replace them with more traditionally philosophical, *a priori* approaches; (iii) at the same time there is a significant residue of positivist thought in Kuhn, which Kuhn did not recognise as such; (iv) the naturalistic elements referred to in (i) are the most original and fruitful elements of Kuhn's thinking; (v) the positivistic elements referred to in (iii) vitiated his thought and acted as factors in preventing Kuhn from developing the naturalistic elements and from following the path taken by much subsequent philosophy of science. Preston presents an alternative reading of Kuhn which emphasizes the Wittgensteinian elements in Kuhn. I argue that this alternative view is, descriptively, poorly supported by the textual evidence and the facts of the history of philosophy of science in the twentieth century. I provide some defence of the naturalistic approach and related themes.

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1. Introduction

I have argued for the various theses concerning Thomas Kuhn's work.¹ Descriptively, I have argued that:

- (i) there is a strong naturalist streak in *The structure of scientific revolutions*, which is to say that Kuhn was happy to use the deliverances of various

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¹ Bird (2000, 2002).

a posteriori sciences and other disciplines (notably psychology and history) in dealing with questions that are at least partially philosophical;
 (ii) as Kuhn's career as a philosopher of science developed he tended to drop the naturalistic elements and to replace them with more traditionally philosophical, *a priori* approaches;
 (iii) at the same time there is a significant residue of positivist thought in Kuhn, which Kuhn did not recognise as such.

Evaluatively I have argued that:

(iv) the naturalistic elements referred to in (i) are the most original and fruitful element of Kuhn's thinking;
 (v) the positivistic elements referred to in (iii) vitiated his thought and acted as the factors (but by no means the only factors) in preventing Kuhn from developing the naturalistic elements and from following the path taken by much subsequent philosophy of science.

I would like to thank John Preston in articulating my views so clearly and reasonably in his 'Bird, Kuhn, and positivism' (Preston, 2004). In taking issue with my view, Preston shows much more sympathy for the quasi-Wittgensteinian interpretation of Kuhn given recently by Read & Sharrock.² This is an important interpretation of Kuhn, and Preston's paper has done much to help articulate it and to show up its differences with mine. However, as I shall argue below, this alternative view is, descriptively, poorly supported by the textual evidence and the facts of the history of philosophy of science in the twentieth century. Evaluatively, I shall provide some defence of the naturalistic approach and related themes. Hence this paper is not simply a reply to critical comments but also an opportunity to further elaborate my interpretation of Kuhn against a popular alternative.

2. The meaning of theoretical terms

One feature of Kuhn's thought that I identify as involving a commitment to the positivist legacy is Kuhn's theoretical context approach to the meaning of theoretical terms. Preston asks whether this is a deeply positivistic thesis. He suggests that it might be thought to be positivistic because of its being verificationist, in the sense that it requires meaning to be 'recognisable'. Preston goes on to remark that this kind of verificationism is not objectionable, and indeed he thinks that the kind of realist semantics I discuss satisfies its requirements.

There are a variety of interesting points here that need to be disentangled. But I shall start with the most important—in what sense is the theoretical context view of meaning positivist? Questions such as 'Is thesis T a positivist (empiricist, idealist, realist etc.) thesis?' are notoriously difficult. Nonetheless, for the current discussion,

² Read & Sharrock (2002). Assessments and interpretations of Kuhn with a Wittgensteinian element also include Kindi (1995) and Barnes (1982).

the following distinction will do. One might take thesis T to be *essentially* positivist, meaning that only positivists hold T. Or one might take thesis T to be *aetiologically* positivist, in that the historical explanation of the prevalence or holding of thesis T is because it was held by positivists or that it developed historically from positivist theses. Whether the theoretical context account of meaning is essentially positivist I am not sure. However, I do believe that its prevalence among philosophers of science in the 1950s and 1960s is due to the fact that it was developed by positivists. So the thesis is aetiologically positivist.

Contrary to what Preston suggests, I do not think that the theoretical context view is positivist because of a connection with verificationism, at least not in the way Preston proposes. Aetiologically, the view was developed among positivists as a consequence of starting with the essentially positivistic thesis that theoretical meaning is founded on the meaning of observational terms and then responding to the various problems that view faced. That observational terms are semantically foundational is indeed a verificationist claim, but the issue is not so much the recognisability of meaning, but rather the idea that observation sentences are (allegedly) the only ones we can directly verify. Hence, if theoretical sentences have any meaning at all, that meaning must be cashed out in terms of observation sentences. The positivists tried various ways of developing this idea. Pure instrumentalism asserted that only observational sentences have any meaning, and that strictly theoretical claims and terms have no meaning. Reductionism held that theoretical sentences and their terms are equivalent in meaning to (complex) observational sentences and terms. But these approaches failed to be both plausible and workable in detail. The double-language model was then developed, whereby the theoretical part of scientific language was regarded as having genuine meaning (contra instrumentalism) but without that meaning being identical to the meanings of the observational parts of scientific language (contra reductionism). So the observational and theoretical parts of language are distinct (hence *double-language* model). Nonetheless, the positivist commitment to observation being the root of meaning in all scientific language was retained. Observational terms have meaning as before, by virtue of correlation with observable items. The theoretical part of language inherits meaning from the observational part via correspondence rules. However, the correspondence rules do not link individual theoretical terms to the observational language, nor do they link every theoretical proposition to a corresponding observational one. Rather they link whole theories to observational propositions. So the question, what is the meaning of some individual theoretical term? remains unanswered. The response was given that while theories as a whole get their meaning via the correspondence rules, individual theoretical terms get their meaning from the meanings of the theories *via the roles they play within the theories*. Thus to explain the meaning of a term *t* is to explain that it appears in a theory T (which is linked to observational language by correspondence rules $r_1, r_2 \dots$) and that within T the term *t* plays such-and-such a role.

If one has adopted a theoretical context view of theoretical meaning by following the path outlined above, or because one takes that view on board from others who have followed that path, then one's adoption of that view is aetiologically

positivist. The latter is what I claim is true of Kuhn. The truth of my claim cannot be established directly, because Kuhn does not even articulate a theoretical context view of meaning in *The structure of scientific revolutions* let alone justify it or say what his source for it is.³ Now it might be possible to acquire a theoretical context view of meaning from more than one intellectual source. For example, it might be an application of Wittgenstein's local holism about meaning, developed in the 1930s, to the particular case of theoretical terms. But when we recall Kuhn's relatively thin philosophical training, it is far more plausible to infer that he adopted this view because it was the then predominant view of theoretical meaning in the philosophy of science. For example the view is explicit in Ernest Nagel's hugely influential 1961 book *The structure of science*.⁴ Kuhn mentions Nagel as one of four friends who were most significant in helping him rewrite his draft of *The structure of scientific revolutions*. (I surmise that the similarity of the titles of Nagel's and Kuhn's books is not mere coincidence.)⁵ Nagel's theoretical context view of the meaning of theoretical terms is part of a double language model that has the aetiology I have described above. For this reason it is appropriate to ascribe to Kuhn an aetiological positivism in his use of the theoretical context view.

While I do not associate the theoretical context view with positivism because of any connection with the recognisability of meaning, Preston's remarks on that matter are nonetheless interesting, since he takes the recognisability of meaning to be a desideratum that is not met by the referential semantics of Kripke and Putnam, but which is met by the theoretical context view.

Preston says that the Kripke–Putnam view involves *semantic scepticism*, because it is possible in that view not to know what the meaning of a term is, in this sense: according to that view the meaning of a member of a relevant species of expression is its referent and a speaker may not be able to identify that referent or distinguish it from items that are not the expression's referent. First a terminological point. 'Scepticism' seems the wrong word to use here. Scepticism about the external world is the view that one *cannot* know anything about the external world, not that the external world is such that one *might* not know certain things about it (the latter is just common sense). Correspondingly semantic scepticism should be the view that we *cannot* know the meanings of our words, and the Kripke–Putnam view is by no means committed to that. It might be better to call the view semantic

³ Instead there are a few remarks that imply such a view of meaning, the best known of which is this, concerning 'Einsteinian space, time, and mass': 'the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved; Einsteinian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same)' (Kuhn, 1970a, p. 102).

⁴ Nagel (1961), Ch. 5.

⁵ Nagel was also on the advisory committee of the International Encyclopedia of Unified Science that published *The structure of scientific revolutions*, along with a host of other positivist philosophers of science.

fallibilism, since the most that Preston can reasonably accuse it of is a commitment to the possibility of ignorance concerning meaning.

But is semantic fallibilism both true of Kripke–Putnam and objectionable? Let us take the two central cases: proper names and natural kind terms. Leaving aside the fact that it is odd to talk of the meaning of a proper name at all, is it true that a referential view of proper names is committed to the claim that we might not know the meaning (referent) of a proper name we competently use? One could claim that any competent English speaker who knows that ‘Ronald Reagan’ is a proper name knows precisely to whom that name refers—it refers to Ronald Reagan. In general a competent speaker knows, for any name ‘*t*’, that if ‘*t*’ refers at all, ‘*t*’ refers to *t*. Preston might respond that this is not what is required; what is required to know the referent of ‘Ronald Reagan’ is to know Ronald Reagan himself. But if that is what is required by the requirement that meaning be recognisable, then that requirement involves an unreasonable Russellian principle of acquaintance, and there is no reason to reject semantic fallibilism. Similarly it could be argued that any competent English speaker knows that what ‘water’ refers to is water. However, Preston thinks that according to the Kripke–Putnam view, a speaker doesn’t really know (or ‘fully’ know) the meaning of ‘water’ unless he knows that water is H₂O. It is unclear why Preston is entitled to attribute this view to Kripke and Putnam. What seems to be doing the work is the concern that without knowing that water is H₂O, we cannot know the correct extension of ‘water’. Taken one way that is false. On the Putnam view the extension of ‘water’ includes all and everything sharing the microstructure of some sample of water (e.g. a raindrop). Someone could know that without knowing what that microstructure is. Taken another way it is true, but unobjectionable. The sense in which it is true that without knowing that water is H₂O one does not know the exact extension of ‘water’ is this: such a person does not know for every item in the world whether it is water or not. But that remains true even for those who do know that water is H₂O. For there are many items in the universe of which we do not know and could not know whether or not they are water.

So, depending on how the recognisability of meaning requirement is spelt out, that requirement is either met by the Kripke–Putnam view or it is not a reasonable requirement after all.

3. Scepticism, knowledge, and truth

From the second edition of *The structure of scientific revolutions* Kuhn was overtly a sceptic. In the first edition, the official position seems to be neutral as regards truth and knowledge (nothing he says rules out or in the possibility of knowledge and true belief). The core idea that remained throughout his work is certainly that an adequate account of the development of science does not need to employ such concepts—on this Preston and I agree. But we disagree when I say that from 1969 and possibly before, Kuhn was a sceptic.

Preston has two arguments against me. First that the sceptic essentially needs the concepts of knowledge and truth (as philosophers understand them) to characterise his position, whereas Kuhn rejects the use of such concepts (as philosophers of science have understood them). Secondly, Kuhn does on occasion talk of scientific knowledge (presumably in a non-philosophical sense?).

In the ‘Postscript 1969’ to the second edition of *The structure of scientific revolutions* Kuhn has an argument, which is his basis for the rejection of the concepts of truth and knowledge. The argument and its context is this:

Compared with the notion of progress most prevalent among both philosophers of science and laymen, however, this [Kuhn’s] position lacks an essential element. A scientific theory is usually felt to be better than its predecessors not only in the sense that it is a better instrument for discovering and solving puzzles but also because it is somehow a better representation of what nature is really like. One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. Apparently generalizations like that refer not to the puzzle-solutions and the concrete predictions derived from a theory but rather to its ontology, to the match, that is, between the entities with which the theory populates nature and what is “really there.”

Perhaps there is some other way of salvaging the notion of ‘truth’ for application to whole theories, but this one will not do. There is, I think, no theory-independent way to reconstruct phrases like ‘really there’; the notion of a match between the ontology of a theory and its “real” counterpart in nature now seems to me illusive in principle.⁶

Paul Hoyningen-Huene interprets Kuhn thus:

The ... argument is epistemological; it proceeds from the assumption that it’s essentially meaningless to talk of what there really is, beyond (or outside) of all theory. If this insight is correct, it’s impossible to see how talk of a “match” between theory and absolute or theory-free, purely object-sided reality could have any discernible meaning. How could the (qualitative) assertion of a match, or the (comparative) assertion of a better match, be assessed? The two pieces asserted to match each other more or less would have to be accessible independently of one another, where one of the pieces is absolute reality. But if we had access to absolute reality—and here we can only return to our initial premise—what interest would we have in theories about it?⁷

Not only, as Hoyningen-Huene says, is this an epistemological argument, it is also clearly a sceptical one. Kuhn may well reject the notions of truth and knowledge because of this argument, but that does not save him from the charge of being a sceptic. Preston’s response is that these are arguments concerning the concepts of

⁶ Kuhn (1970a), p. 206.

⁷ Hoyningen-Huene (1993), pp. 263–264.

truth and knowledge *as philosophers have understood them*. I don't think so. Note that Kuhn's argument is directed not against a view held only by philosophers of science—it is a view he attributes also to *laymen*. So Kuhn's objection, *pace* Preston, cannot be to a specifically philosophical usage of 'truth' and 'knowledge'. And in any case the argument is directed against the idea of our being able to match theory and what is "really there"—but the expression, which Kuhn always encloses in scare quotes, "really there" is hardly a philosophical term of art.⁸

Nonetheless, let us say for sake of argument that Preston is right on this last point. Then we agree at least on this then, that Kuhn was a sceptic*, where someone is a sceptic* when they reject the possibility of knowledge*, where knowledge* is knowledge-as-understood-by-philosophers.

Then the question is whether knowledge* is so very different from knowledge—where the latter is what is meant by the ordinary term of English, 'knowledge'. Correspondingly, we may ask, in addressing Preston's first objection whether knowledge[†] (which is what Kuhn means by his use of 'knowledge') is the same as knowledge.

In the context of Kuhn's argument, knowledge* and truth* might be characterised (partially) thus:

- (I) a proposition is true* precisely when the world is as the proposition says it is;
- (II) S knows* *p* entails *p* is true*.

I challenge Preston to tell us what is so objectionably philosophical about the above and how the concepts so characterised differ from the concepts of knowledge and truth as ordinarily used.

Correspondingly, it would be good to know what, according to Preston, Kuhn's alternative conception of knowledge, 'knowledge[†]', is, where precisely it differs from knowledge*, and why it matches the ordinary concept of knowledge better than knowledge*. So, for example, the following questions are pertinent, does knowledge[†] entail truth in any sense (e.g. some 'truth[†]')? If not, does that not conflict with the common conception that only what is true can be known? And if knowledge[†] does entail truth[†], what then is truth[†]? Does the truth[†] depend on the way things are (independently, typically, of what we believe)? If so, how then does truth[†] differ materially from truth* (and knowledge[†] from knowledge*)? If truth[†] doesn't depend on the way things are, then we have to ask why truth[†] is a good account of the ordinary conception of truth which *prima facie* does take truth to depend on the way things are. It is indeed the case that philosophers have developed sophisticated correspondence theories of truth (most notably Russell and Austin). Those theories share the presumption that truth is a matter of a *structural* correspondence between the world and proposition. It should be noted that the simple view that truth depends on the way the world is (rather than vice-versa)

⁸ I examine Kuhn's argument in some detail in Bird (2000), pp. 225–232.

does *not* of itself amount to a correspondence theory of truth, since it requires nothing in the way of structural correspondence. Although there have been philosophical theories that have denied this simple idea (and those that have supported it), in itself the idea that truth depends on the way the world is can hardly be described as ‘philosophical’.

It would be difficult to reconstruct a positive Kuhnian account of knowledge and truth, because he says little on the matter. Preston cites two examples of Kuhn’s use of the term.

- (Ia) ... science—our surest example of sound knowledge ...⁹
- (Ib) ... the bulk of scientific knowledge clearly increases with time ...¹⁰
- (II) [S]cientists behave in the following ways; those modes of behaviour have (here theory enters) the following essential functions; in the absence of an alternate mode *that would serve similar functions*, scientists should behave essentially as they do if their concern is to improve scientific knowledge.¹¹

This does not prove that Kuhn rejected only a philosophical conception of knowledge (knowledge*) nor does it refute my claim that from the second edition of *The structure of scientific revolutions* (1970) Kuhn was a sceptic. First note that Preston’s quotations are from Kuhn’s papers in Lakatos & Musgrave’s *Criticism and the growth of knowledge*.¹² Although published in 1970, the volume is the proceedings of a colloquium that took place in 1965. Quotations (Ia) and (Ib) are from Kuhn’s ‘Logic of discovery or psychology of research?’, which according to Lakatos and Musgrave was ‘essentially in the form in which it was first read’ (viz. in 1965).¹³ Quotation (II), however, comes from Kuhn’s ‘Reflections on my critics’ which was not read at the colloquium, but was completed in 1969, which was the year in which Kuhn prepared the ‘Postscript’ to the second edition of *The structure of scientific revolutions*, and so strictly only (II) bears on my claim.

Quotation (II) does *not* imply that Kuhn thinks that knowledge is achievable (however ‘knowledge’ is understood). For what it says is that scientists should behave as they do *if their concern is to improve scientific knowledge*, and so ‘scientific knowledge’ is in an intensional context (it is part of the content of the putative concern of the scientists). A commentator can say what S would do if S’s concern were to increase scientific knowledge even if the commentator thinks that scientific knowledge is impossible. For the commentator’s claim to make sense, it is required only that S think that scientific knowledge is possible, not that the commentator (in this case Kuhn) does. This is just as I can say that Arthur should behave as he does, if his concern is to impress Father Christmas, even if I don’t believe in Father Christmas.

⁹ Kuhn (1970b), p. 20.

¹⁰ Ibid.

¹¹ Kuhn (1970c), p. 237.

¹² Lakatos & Musgrave (1970).

¹³ Ibid., p. vi.

If we put the latter aside, quotation (II) would show that Kuhn was willing to use the term ‘knowledge’ while also engaging in what I take to be sceptical arguments. Does this demonstrate that Kuhn only rejected a philosophical conception of knowledge, not the ordinary one? Scarcely, until one has shown (and as I have challenged Preston to show) that the target of Kuhn’s arguments in the ‘Postscript 1969’ is a philosophical notion of truth, not the ordinary one. A hypothesis that is equally and probably more plausible is that Kuhn, knowingly or otherwise, rejected for sceptical reasons a perfectly ordinary notion of truth and *himself* uses ‘knowledge’ in a peculiar sense, roughly equivalent to ‘what is taken to be knowledge’. That usage of ‘knowledge’ conflicts with our ordinary usage, but has for some time been an entirely standard usage among sociologists of scientific knowledge.

Let us return to quotation (Ia). Although it was written before the time from which, I claim, Kuhn was overtly sceptical, it does throw some light on his thinking. For the context is this:

(Ia*) I must first ask what it is that still requires explanation. Not that scientists discover the truth about nature, nor that they approach ever closer to the truth. Unless, as one of my critics suggests, we simply define the approach to truth as the result of what scientists do, we cannot recognize progress towards that goal. Rather we must explain why science—our surest example of sound knowledge—progresses as it does, and we must first find out how, in fact, it does progress.¹⁴

This paragraph tells us that we cannot recognise truth or the approach towards truth. There is no suggestion that this claim is about truth in some philosophical sense. On the contrary, Kuhn seems to resist any redefinition of truth. The claim that we cannot recognise the truth would seem as straightforwardly sceptical as one could ask for. But then Kuhn goes on, as Preston quotes, to talk as if scientific knowledge is attainable. But how can one have knowledge if truth is unrecognisable? Doesn’t knowledge entail truth? Evidently Kuhn doesn’t think so. It seems to me that the best explanation of what is going on is this. Kuhn is a sceptic in that he thinks that truth cannot be recognised, we cannot ever be in a position to tell that our theories match nature. It seems that he may have held such a view as early as 1965, and possibly before, but he provides an argument for it only later in the second edition of *The structure of scientific revolutions*. Normally one would conclude from the scepticism just identified that Kuhn thinks that theoretical knowledge is not possible, since knowledge just is a certain kind of recognition of the truth. But Kuhn is, on rare occasions, willing to talk as if there is scientific knowledge. So clearly Kuhn’s notion of knowledge is not one that involves (recognition of) truth. What I take Kuhn to mean by ‘scientific knowledge’ is that body of scientific belief that is well-established, widely held in the relevant scientific communities, not regarded as tentative or falsified. That is, in common with other writers (especially those of a relativist bent), ‘scientific knowledge’ is what scientists

¹⁴ Kuhn (1970b), p. 20.

take to be scientific knowledge. It is noteworthy that Kuhn uses the term knowledge as a mass term, typically in the phrase ‘scientific knowledge’ and does not discuss whether individual scientists or groups of scientists know some particular fact or have theoretical knowledge of some specific kind. Furthermore, his use of ‘scientific knowledge’ is not in epistemologically sensitive contexts but in the discussion of the explanation of the development of scientific ideas, that is to say in a context where the distinction between what is really knowledge and what is taken to be knowledge is not particularly germane.

Preston’s parting shot on these issues is that the accusations of scepticism are at odds with my characterising Kuhn’s thinking here as positivist, since the positivists ‘are the last people to think that science cannot deliver knowledge, since they think science precisely is *positive knowledge*’. I don’t think that Kuhn’s scepticism is essentially positivist—after all the view and the argument are shared by many non-positivists. However, there is no tension between the kind of scepticism I attribute to Kuhn and positivism. On the contrary this very same sceptical argument is used by the positivists, for example Neurath, to motivate a coherence theory of truth.¹⁵ That is not surprising, since the argument goes back at least to Kant, and as is being increasingly recognised there is a strong Kantian stream in positivism alongside the more overt empiricist stream. The latter, of course, also has a strong sceptical element, most obviously with respect to knowledge of unobservable and inductive knowledge. This empiricist scepticism makes itself felt firstly in the attempt to reduce theoretical talk to observational talk or otherwise reconstrue theoretical talk so that it is no longer about unobservable entities. This I have discussed above. Secondly the sceptical concerns with (ampliative) induction are shown, for example, in Carnap’s (failed) attempt to devise a non-ampliative logic of induction based on the idea of logical probability. From the perspective of contemporary philosophy of science, dominated by scientific realism, positivism’s anti-realism is distinctively sceptical. To be sure, the positivists were not simply sceptical about scientific knowledge—quite the opposite, as Preston says. They had a pro-science attitude and wanted to promote, in Schlick’s phrase a ‘scientific world-conception’. But much of the detail of positivism can be understood as the result of the attempt to square this pro-science attitude with the sceptical philosophy they inherited from empiricism and from Kant. Hence the need to show that scientific theories are not about unobservable theoretical entities and that science does not use abductive or ‘Humean’ induction.

4. Positivism and naturalism

With regard to accusations of positivism, Preston thinks that the boot is sometimes on the other foot, in that in some respects contemporary naturalist philosophers are more positivistic than Kuhn, and in a way that Preston thinks is particularly problematic.

¹⁵ See Hempel (1935), pp. 49–59, and Bird (2000), pp. 232–235.

The dialectic of Preston's accusation is unclear. He starts by telling us that he will show that naturalists are positivists or positivistic in some important respect. But when it comes to the crunch of the argument, he says this is a 'respect in which positivists *are* naturalists'. The two propositions are of course quite different: one has the form Fs are Gs and the other has the form Gs are Fs. The latter can be true while the former is false. It may be that in some respect positivists are naturalistic without it following at all that naturalists are in any way positivistic.

What is the respect in question? The crux of it is that positivists are, allegedly, 'methodological naturalists', who hold, according to Preston, the view that there is not or should not be any difference between the methodology of the social and natural sciences. The view that there is a single method common to all the sciences, natural or social, would less controversially be called methodological *monism*. Is methodological monism naturalistic? I can see no reason to suppose that it is. On the contrary, my own view is that naturalists should be methodological pluralists.¹⁶ If anything, this is another area where logical positivists and naturalists differ sharply and do not, *pace* Preston, 'make common cause'. The positivists were indeed committed to the unity of science and a single scientific method. In principle, the nature and adequacy of that method would be knowable *a priori*, and it would be an extension of formal propositional and predicate calculus, as for example Carnap's above-mentioned logic of induction. Admittedly, when such programmes ran into difficulty, as Carnap's did, the response was to appeal to convention. And the possibility of a conventional choice implies a plurality of possible methods. But this is not methodological pluralism in the sense of this discussion, since there was no sense in which different choices would be appropriate for different parts of science (for example physics and sociology). What was not contemplated was the idea that a scientific method could itself be the product of a scientific discovery. The latter, which is an essentially naturalistic claim, leads fairly swiftly to methodological pluralism. For it is clear that the subject matters of the various sciences are very different and so one should have no expectation that a method developed in one field should have application in every field. Of course a method might have application in new areas, but that is again a matter for discovery and science itself must determine the limits of applicability. So, for example, Sir Joseph Larmor's discovery of the relationship between the frequency of absorption and re-emission of electromagnetic waves by nuclei in a magnetic field, and the strength of that field, was pure science. In the 1940s this discovery was put to use as a method for analysing small samples of chemical compounds, while only in the 1970s was the technique first employed in medical science, in magnetic resonance imaging; more recently the technique has been used as a research method in psychology. It seems unlikely that MRI will become a method in the hands of sociologists, but that cannot be an *a priori* judgment. Admittedly, it is more obvious that the methods of sociology, such as mass questionnaire surveys, will never be used in physics, but even

¹⁶ Bird (1998), pp. 224–226, 258–260.

in sociology the applicability of the method, and more importantly its limitations, details of questionnaire design and so forth are matters for sociological and psychological investigation. The naturalist might be impressed by the fact that NMR links physics, chemistry, medicine, and psychology, and on this basis promote the thesis that science is unified by a web of methods. This weak and *a posteriori* unity of science hypothesis is motivated by a pluralistic and *a posteriori* approach to methodology that is in marked contrast to the positivistic theme identified by Preston.

Preston also describes at length how on the one hand positivists all felt that science exhausted the domain of positive knowledge, while on the other hand this view was rejected by Wittgenstein and Kuhn. Preston is clearly right about this, but its bearing on the current debate about Kuhn's relation to positivism and naturalism is minimal. For the naturalist is not obliged to believe that all knowledge is scientific knowledge. On the contrary, she might well believe, because of her methodological pluralism, that there are ways of acquiring knowledge that are not scientific. For example she might believe (with good evidence) that nature has endowed humans with certain powers of intuition, which can be understood as unconscious information processing, that lead to certain feelings and thence to beliefs that might be knowledge (for example the processes cause a feeling of unease that causes a belief, indeed knowledge, that there is danger). Such knowledge could not be called scientific. Nor is the naturalist obliged to restrict non-scientific knowledge to such esoteric cases. The majority of what we know collectively and individually has been gained by methods that it would be incorrect to call scientific (for example by perception and common sense reasoning of various sorts). However, the philosophical naturalist does think that the methods of science—or at least the deliverances of science—may be used in philosophy itself. Some traditionally philosophical questions may not be answerable by the *a priori* methods of pure first philosophy but may need *a posteriori* scientific input also. On this point the naturalist is again at odds with the positivist, whereas the positivist and Wittgenstein (early and late) are in agreement. While the naturalist thinks that philosophical knowledge is in principle possible and may overlap with scientific knowledge, the positivists and Wittgenstein denied the contiguity of science and philosophy (as Preston rightly shows for the early Wittgenstein), and indeed arguably denied the possibility of philosophical knowledge at all. Here is how Baker and Hacker sum up Wittgenstein's view:

According to one conception, philosophy is extra-ordinary in that it adjudicates the bounds of sense. It clarifies which questions are intelligible and which investigations are in principle relevant or irrelevant for answering them. This view Wittgenstein held and argued for throughout his career. It is in this sense that he thought of philosophy as the activity of clarifying thoughts (*Tractatus logico-philosophicus* 4.112) or as striving for a deeper understanding of language (*Philosophical remarks* 7). The sciences are totally different in nature and

pursued for very different purposes. Consequently, science is irrelevant for philosophy.¹⁷

Similarly the positivists took much of traditional philosophy, particularly metaphysics, to fail to adjudicate the bounds of sense and hence to transgress them. Some positivists sought a role for ‘good philosophy’ as a logic of science, but even they saw that a logic of science is no part of science. Schlick, on the other hand, took a more Wittgensteinian view of philosophy, as an activity and hence no part of knowledge.

And so although both positivists and naturalists both take a ‘pro-science’ attitude, their similarities end about there. As regards the nature of philosophy and its relationship to science, they diverge radically.

5. Psychology and computationalism

Scientists make judgments not by following rules but rather on the basis of a learned sense of similarity to paradigmatically excellent puzzle solutions. This is the central insight of *The structure of scientific revolutions*, and it explains the nature of a scientific revolution, for a revolution is a change of paradigm exemplars. Consequently a scientist who changes his view during a revolution is one whose sense of what constitutes an excellent piece of science changes. This can be a profound change for the scientist, in part because it involves a change in an unconscious sense rather than solely a change in conscious belief. Kuhn likened the change to a Gestalt-switch, and indeed speculated that the psychological mechanisms involved are the same.

Kuhn’s remarks on revolutions being like Gestalt-switches and involving a change in the scientist’s world provoked a negative reaction among his philosophical critics. One reason was that Kuhn’s approach to understanding scientific change was naturalistic, while his critics still expected the answer to be in terms of some *a priori* assessable set of rules of method or confirmation. Another reason for the negative reaction, I speculated, was that Kuhn’s explanation looked like so much mystery-mongering in the absence of any mechanism to underpin the processes that Kuhn hypothesises. I went on to say that Kuhn’s problem was that he was before his time, for mechanisms that would explain the sort of learning processes that Kuhn describes were later the subject of considerable research in cognitive science and psychology, in the forms of ‘connectionist’ or ‘neural net’ models. My central point is not that these models are the correct ones, but rather that before such models, Kuhn’s remarks looked like an appeal to the mysterious, but in the light of those models the same remarks look insightful. It might be that connectionism is badly wrong and that something else provides the mechanism for Kuhn’s processes.¹⁸

¹⁷ Baker & Hacker (1980), p. 259.

¹⁸ I am grateful to Guy Longworth for emphasising this point.

That said, I did endorse connectionism as a promising way forward, and I do believe that further research will in due course reveal that Kuhn was largely right, in the important respects, about the nature of scientific learning and judgment-making and that the mechanism underlying those processes will have a significant connectionist element. And a similar mechanism will explain what is going on in Gestalt-switches and the playing-card experiments performed by Bruner and Postman, to which Kuhn refers.¹⁹

Preston associates connectionism with positivism, in which case, once again, the boot is on the other foot when it comes to accusations of hidden positivism. As Preston says, it is not the links with positivism that ultimately are of importance but the truth of the issues involved. Although I think that the more positivist a view is, the more likely it is to be mistaken, in this case I think the link between connectionism and positivism, even if it exists, is so tenuous as to be of no disadvantage to connectionism.

It is debatable whether the link really exists at all. The alleged link is that connectionism subscribes to a computational model of the mind and this is the product of behaviourism, itself a product of positivism. I will readily admit that I am no expert on these matters and expect to be put right by those who are. But it seems to be extremely misleading to suggest that connectionism is computational in the sense that a computational approach to the mind is a product of behaviourism. How is computationalism a product of behaviourism? As I understand it, the answer is that behaviourism's response to the mind–body problem in general and to the unobservability of 'inner' mental states in particular, is to reduce the mental to behaviour, or at least to dispositions to behave. Behaviourism suffered from several problems, but one of the most important was the fact that the behaviour appropriate to one mental state would depend on what other mental states one is in (what actions are prompted by a desire will depend upon what one believes). So it looks as if the mental cannot be eliminated after all. Functionalism can be seen as an answer to this problem that retains the spirit of behaviourism. For the functionalist says that to be in a mental state is to be in some *physical* state that will, given a certain stimulus, yield a corresponding output, depending on what other mental states the person is in. Since those other mental states will also be a matter of being in some physical state, the mental fully supervenes on the physical, while at the same time we have succeeded in providing a conceptual basis for the mental in behaviour (even if no elimination of mentalistic talk is available).

An important stimulus to the functionalist programme is the computational analogy. Alan Turing gave the first formal account of a computer programme, which looked analogous to the functionalist account of the mind. A computer programme may be specified as a set of states, where each state is specified in terms of its response, when it is the active state, to a stimulus (what the computer reads from its computer tape or memory), the response being a matter of an output (changing the tape/memory or scanning another part of the tape/memory, and

¹⁹ Kuhn (1970a), p. 63.

changing/keeping the active state of the computer). This characterisation of a Turing machine is abstract; for a real, physical computer to instantiate a Turing machine it is required simply that the computer have certain physical states whose causal roles allow them to fulfil the functions in the specification of the Turing machine. Similarly, it might be thought, to have a certain mental state is to have some physical makeup (which could be brain matter or something else), whose causal role allows it to fulfil the functional characterisation of that mental state.

While the analogy between Turing machine state and mental state is inexact in certain ways, it seemed strong enough to prompt several lines of thought. Firstly, it is conceivable that the abstract functional states that are mental states could be reduced to states of Turing machine, in the way that a computer programme written in a high level language is reducible to one specifiable solely in terms of the states of a Turing machine or something equivalent to a Turing machine. If so the mind and brain would not simply be *analogous* to a Turing machine and a computer but in fact would *be* a Turing machine and a computer. Secondly, a high level computer programme is built out of simpler functions (depicted by a flow chart that has components which themselves are flow charts, but simpler ones). So one might imagine that higher level cognition in humans is a matter of engaging in slightly lower level cognitive processes in a structured way. A researcher in Artificial Intelligence might hope to model the higher level cognition in terms of a structure of rules, each of which represents some lower level cognitive process. The creator of an ‘expert system’ will seek to make explicit the rules that, allegedly, a human expert follows, albeit unconsciously. Thus one might seek to model the judgment of a scientist in deciding between competing theories, in an expert system that contains explicitly the rules of scientific method that the scientist follows unconsciously. Thirdly, the computational approach described regards cognition as essentially involving the manipulation of symbols: cognition is governed in part by syntactical rules for combination and transformation of symbols. Furthermore, when instantiated in a particular creature or machine, these symbols could be connected to external items and hence have a semantics. This symbolic approach has been one of the governing ideas of the computational paradigm. It is worth remarking that that even if computationalism does have historical links to behaviourism, the symbolic approach shows how far it has left its roots behind, for behaviourists would reject the very idea of inner representation.

The significance of the forgoing description of the computational approach to the mind is that connectionism stands in stark contrast to *all* of it. The following points demonstrate this:

- Connectionist systems (abstractly characterised) are not always Turing machines or modellable by Turing machines. A connectionist system contains a large number of nodes that interact with connected nodes. The system is trained to perform certain cognitive tasks by reinforcing existing connections when the system gets a correct answer and by changing the connections when it gets a wrong answer. In time the system ends up in a state that performs the task with a greater success rate than it did with the initial

random assignment of connections and connection strengths. The range of strengths of connections and the functions that determine how a node responds to a stimulus need not be digital. That is the permitted range of values might be a segment of the real numbers, and the functions might be functions of real numbers. In which case the system cannot be a Turing machine nor can it be modelled by one. For Turing machines are essentially digital and can handle at most the rational numbers, not the real numbers.

- The fact that connectionist systems are typically implemented on or modelled by computers does not imply that the connectionist view of the mind and brain is a computational one. Perhaps one could model a connectionist system with string and sealing wax. But computers do a much better job. Of course a computer could never *exactly* model an analogue connectionist system of the kind described above. But it might be able to give a very good approximation (the better depending on the size of the memory of the computer), just as a computer model can capture a good approximation of other analogue systems (for example the flow of a fluid, where the variables may take any real value, including irrational values, but which will be approximated by the computer using only finitely long decimals). Say one thought that one had devised a connectionist system that exactly captured the neural structure of some particular human at some instant, and that this system was digital and so could be modelled exactly on a sufficiently powerful computer. Would that commit one to a computational view of mind and brain? No it would not, for all the computer is doing is *modelling* the brain, and that doesn't make the brain a computer any more than the fact that a computer can model fluid flow in a pipe makes a water and pipe combination a computer.
- Connectionism was devised to stand in explicit *contrast* to the computational expert-system approach to modelling and understanding human cognition. A connectionist system may implement a higher-level cognitive process without there being parts of the system that implement simpler cognitive functions. Hence the connectionist system does not permit the higher-level cognitive function to be broken down into rules that operate at a lower level. This is why a connectionist approach is appropriate to Kuhn's needs, because Kuhn was concerned to reject the idea that scientists' judgments are a matter of following rules, whether conscious or otherwise. Rather, Kuhn regarded scientific judgment as a matter of seeing similarities between a proposed puzzle-solution and a paradigmatic puzzle-solution. The latter is much closer to the processes of spotting family resemblance or recognising patterns, which are precisely the cognitive processes which are most effectively implemented by connectionist systems.
- As mentioned, the computational approach is essentially symbolic, whereas the connectionist approach isn't. It is conceivable that a particular connectionist system might be understood as manipulating symbols, but that isn't a feature of connectionist systems in general. (Similarly a connectionist system might turn out to be modular, but that isn't essential either.) Insofar as a

connectionist system doesn't employ symbols, it might appear closer to behaviourism than the computational approach. But that is accidental, since a connectionist system *could* employ symbols. More importantly, the motivations for the two views are entirely distinct. Behaviourism is motivated by the philosophical rejection of hidden inner states, whereas connectionism is motivated primarily by its proven efficacy in solving problems (such as problems of pattern recognition) that standard computationalism has failed to solve.

Stuart Shanker captures Alan Turing's view thus:

His basic idea is that thinking is an effective procedure (because the brain is a digital computer): i.e., the mind proceeds, via an unbroken chain of mechanical steps, for α to ω , even though the subject himself may only be aware of α , δ , ξ , and ω . By mapping the subject's thought-process onto a program designed to solve the same problem, we can thus fill in the intervening—*subconscious*—steps.²⁰

We can now see that the connectionist approach stands in a deep contrast to Turing's. Brains may well not be digital computers because they need not be digital. More importantly the performance of a cognitive task (for example pattern recognition) need not be seen as the running of a program whose steps correspond to unconscious steps in a thinker's thought process, because the functioning of a connectionist network is not broken down into steps.

It is true that Preston is not the only one who has categorised connectionism under the computational umbrella.²¹ But I cannot see any good reason for doing so, unless one means by 'computational' something akin to 'modelled using a computer'. But, as discussed, the fact that a connectionist system, like fluid in a pipe, can be modelled on a computer tells us very little indeed. And it would be ironic if Preston thought this was an objection to my proposing this as a way forward for Kuhn, since Kuhn himself engaged in experiments with a computer model of concept formation.²² Indeed, in his later years, according to Nancy Nersessian, Kuhn took an interest in computer modelling of learning via specifically neural-net (connectionist) systems.²³

Preston may have another concern that I have not addressed above, suggested by the references to the opinions of Bruner and Shanker that the original cognitive revolution in psychology has been usurped or betrayed by the AI tradition in that it 'turned away from the making of meaning'. Here connectionism is indeed in the same camp as the computational approach to cognitive psychology, in that it takes a descriptive, empirical psychology that is open to sub-personal explanation to be

²⁰ Shanker (1998), p. 37.

²¹ The relationship between cognition and computation, including the approaches discussed here, are explored in detail in van Gelder (1995).

²² Kuhn (1970a), p. 192; Kuhn (1977), p. 310 (originally Kuhn (1974)).

²³ Nersessian (2003), p. 185. Nersessian cites taped interviews with Kuhn conducted by Hanne Andersen in March 1994 and October 1995.

possible. Let us call the latter ‘descriptive–explanatory psychology’. Amongst a number of philosophers (for example Brandom, McDowell, Shanker), descriptive–explanatory psychology is regarded with suspicion because it contrasts with the normative nature of the mental. Nothing in my appeal to connectionism nor in connectionism generally rules out a normative account of the mental. All I asserted was that connectionism could explain how certain cognitive powers could exist. Hence those who are suspicious of descriptive–explanatory psychology must hold some thesis such as the following:

- (N) A normative understanding of the mental is incompatible with the possibility of descriptive–explanatory psychology.

I don’t see why one should accept (N). More importantly, those who do accept (N) and infer that there is something wrong with descriptive explanatory psychology have an uphill struggle to explain what exactly it is that such psychologists are doing if it is not providing a description and explanation of (some of) our cognitive and other mental capacities. For example, studies show that children as they grow older become better at discriminating sounds that occur in their native language, but become *poorer* at distinguishing sounds that occur (only) in foreign languages.²⁴ Similarly, adult humans are very good at discriminating among human faces but not among the faces of non-human mammals (including primates), whereas small babies *are* able to make discriminations between the faces of primates.²⁵ There is no reason to suppose that there is anything mistaken (philosophically or otherwise) in this description of a phenomenon in cognitive development. Furthermore, there seems nothing wrong with the hope that some explanation of these phenomena might be found in an examination of the development of cortical structures or through theoretical models of the mechanisms that underlie this development. Indeed much progress has been made in this direction in solving this as well as a myriad of similar problems.²⁶

It is clear that research such as that described is unimpeachable from a philosophical point of view. And if that is the case there should be nothing wrong, philosophically, in the hypothesis that the process of recognising differences and similarities between solutions to scientific puzzles might be similar in important respects to the process of distinguishing and recognising faces and sounds, and that the neural mechanisms underlying the development of such abilities might also be similar.²⁷ To my mind, even if undeveloped this hypothesis is one of the most striking and important claims in *The structure of scientific revolutions*. Furthermore, it is clear from elsewhere in Kuhn’s earlier writing that he rejected thesis (N). One of the aims of his ‘Logic of discovery or psychology of research?’ is precisely to reject

²⁴ See Cheour et al. (1998), and Kuhl et al. (1992).

²⁵ Pascalis et al. (2002).

²⁶ De Haan et al. (2002).

²⁷ Which is not to say that there is nothing whatsoever wrong with such an explanation *psychologically* speaking. Although I think this is an important insight, I outline some of the difficulties, as well as possible solutions, at Bird (2000), pp. 83–95.

such a dichotomy, a point which he makes forcefully in ‘Reflections on my critics’ (in the same paragraph as passage (II) above, quoted by Preston): ‘Are Kuhn’s remarks about scientific development, he [Feyerabend] asks, to be read as descriptions or prescriptions? The answer, of course, is that they should be read in both ways at once’.²⁸

6. Conclusion

Kuhn’s thought, in origin and development as well as content, is complex. I have portrayed his early work as having two predominating but contrasting elements: first, a strongly naturalistic component, which was his main weapon against those aspects of positivism that he rejected; secondly, a residual but significant commitment to certain other positivistic theses. I also maintain that the naturalistic element was later dropped (with the exception of the retained use of the history of science) and replaced by a more *a priori* and philosophical approach. This contrasts with, for example, Read & Sharrock’s view that Kuhn’s thought is more unified, less naturalistic and less positivistic, and more Wittgensteinian that I have proposed. Preston’s remarks make his stance seem reasonably close to Read & Sharrock. A detailed examination of Kuhn’s writing, plus careful consideration of his context, in particular of the legacy of positivism, shows such an interpretation to be less well supported by the evidence. Furthermore, a careful understanding of the philosophical issues involved shows that philosophical naturalism, such as I attribute to Kuhn, is deeply antithetical to positivism and is in no significant respect akin to it. Thus to attribute to Kuhn’s early work a combination of naturalism and a commitment to a residual and partial set of positivist theses (primarily those concerning the theoretical meaning), is to attribute to Kuhn a considerable tension in his work, which may be likened to the tension that Kuhn himself finds in the work of Copernicus, whose heliocentrism was a radical innovation that sat uneasily with his residual commitment to much of Aristotelian physics. Unfortunately, in Kuhn’s case, the tension was later resolved by dropping the innovative naturalistic element and retaining the incommensurability thesis which originated in his positivistic understanding of theoretical meaning. For this reason I have characterised Kuhn’s Road Since Structure as Kuhn’s Wrong Turning.

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²⁸ Kuhn (1970c), p. 237.

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