Kuhn, Naturalism, and the Social Study of Science

Abstract

(September 7, 2012) I have argued that the Kuhn of *The Structure of Scientific Revolutions* promotes a naturalistic study of science. The Sociology of Scientific Knowledge is also seen by many of its proponents as a naturalistic approach to science, and as such may be seen as part of the Kuhnian legacy. Nonetheless, I argue that it has been an inadequate successor to Kuhn. In particular, its emphasis on externalist explanations of scientific change are inconsistent with Kuhn's model of scientific development. A more faithful and fruitful heir to Kuhn's legacy is the cohort of psychological and cognitive studies into scientific reasoning as based on exemplars and their like.

1 Naturalism in the work of Thomas Kuhn

One of the most interesting features of Thomas Kuhn's work in *The Structure of Scientific Revolutions* is its naturalism. From a philosophical point of view, this naturalism was highly original, sufficiently original that it was not properly understood, either by critics or by many supporters of Kuhn's work. Quine (1969) famously proposed methodological naturalism as an approach to epistemology, seven years after the publication of the first edition of *Structure*, and only just before the publication of the second edition, in which a postscript clarified some of the more important naturalistic elements of the book. It is no wonder that Kuhn said of his notion of *exemplar* (the central application of the concept *paradigm*), that this is the 'most novel and least understood aspect of [*Structure*]' (Kuhn 1970: 187).

There are two strands to Kuhn's naturalism. The first concerns his use of history. *Structure* contains a lot of history. But it is not a historical work in a straightforward way (as is his 1978 *Black-Body Theory and the Quantum Discontinuity, 1894–1912*). Rather, Kuhn uses historical examples to generate a general account of the (structure of) the historical development of science, an account that has a significant social element. This in turn he puts to philosophical use, fulfilling his original motivation for this work. Philosophers of science had used historical examples before, but not in this way. Popper uses historical examples, but the philosophical purposes are strictly limited. Popper's work is normative; the use of examples is to show that science does indeed live up to these norms—a question that he would have regarded as not strictly philosophical at all. Nor is Popper's use of historical examples systematic; in common with other philosophers of science, Popper's use is illustrative rather than marshalled as evidence. Kuhn instead makes a powerful case for a pattern in the history of science, an alternation of normal and revolutionary periods of science. This contrasts with the cumulative account, whereby new well-confirmed beliefs are added to the pre-existing stock of well-confirmed beliefs. The cumulative account is what one would expect (perhaps subject to some idealizing assumptions) if standard accounts of theory confirmation (as one may find among the positivists) were correct.

The second strand of Kuhn's naturalism concerns his willingness to deploy evidence drawn from psychology. For example he cites the experiments of the psychologists Bruner and Postman as evidence for a thesis concerning the theory-ladenness of observation (1962: 62–3). He also refers to the work of the Gestalt psychologists in articulating his thesis of world-change—how the world appears differently after a revolution (e.g. 1962: 2, 111–4). He also briefly mentions the work of Jean Piaget and of Benjamin Whorf (1962: 2). Kuhn (1970: 191–2) even mentions that he was experimenting with a computer program to model shared intuitions about similarity. While the use of Gestalt images is primarily as an analogy, Kuhn does raise the possibility that there is an underlying unity to both Gestalt psychology and to the cognitive processes involved in revolutionary scientific change, and refers to other psychological work as going in this direction, especially that of the Hanover Institute; he footnotes works by the psychologists Stratton and Carr concerning accommodation to image-inverting glasses. That the psychology of Gestalt images and the psychology of paradigm-change are linked is an entirely reasonable speculation for Kuhn. Kuhn's most significant idea in *Structure* is the idea of an exemplar. This is the core of the concept of paradigm; an exemplar is an exemplary, paradigmatic puzzle solution that is used in the training of scientists. This training helps establish *similarity relations* for the scientist. Similarity relations are dispositions to see one thing as like another; in particular these enable the scientist to see one puzzle to be similar to another puzzle and, likewise, one puzzle-solution to be similar to another solution. A scientific revolution involves the introduction of new puzzle-solutions to solve particularly significant, anomalous puzzles. These new solutions become the new exemplars. Consequently new similarity relations are set up and old ones are broken down. Since such similarity relations structure how the subject apprehends the world, scientific revolutions cause a change in the nature of the subject's apprehending. 'Apprehend', as I have used it, can mean the visual appearance of the world, and this would apply to cases similar to the Bruner and Postman experiments and those discussed by the Gestalt psychologists. 'Apprehend' may also mean the subject's understanding of the more abstract structure or pattern of ideas in a scientific theory.

The two naturalistic strands (historical-sociological and the cognitive-psychological) are not independent. For exemplars, by setting similarity relations for puzzles and their solutions, are the motor of normal science. Kuhn (1962: 189–90) tells us, 'The role of acquired similarity relations also shows clearly in the history of science. Scientists solve puzzles by modeling them on previous puzzle-solutions, often with only minimal recourse to symbolic generalizations. Galileo found that a ball rolling down an incline acquires just enough velocity to return it to the same vertical height on a second incline of any slope, and he learned to see that experimental situation as like the pendulum with a point-mass for a bob.' And it is the failure of anomalous puzzles to find a solution by reference to these exemplars that gives rise to crisis and so to revolution. Consequently the cognitive role of exemplars explains the historical cycle of normal science and revolution. This is important, because it adds to the philosophical significance of Kuhn's historical work. This point is best appreciated by comparison to the role of history in Popper's work. Above, I suggested that that historical examples are subsidiary to Popper's principal normative task. Popper's examples are intended to show that as a matter of fact scientists do obey the norms he sets out. But it would not immediately invalidate the normative claims to discover that they do not always obey the norms—one would just have to conclude that scientists are not always as rational as one might have hoped. A similar response might have been made on behalf of the positivists in response to Kuhn's historical studies if all Kuhn had shown was that sometimes science undergoes non-cumulative revolutions. That is *prima facie* consistent with the claim that had scientists been perfectly rational, such episodes would not have occurred. The fact the Kuhn offers a well-evidenced *pattern* puts some pressure on such a reply: how could individual cases of irrationality explain a pattern in science? More importantly, Kuhn's explanation in terms of exemplars provides an alternative explanation: crises and revolutions are not the outcome of localised irrationality, but rather of deep facts about human (individual and social) cognition. Kuhn is offering not an alternative to the rationality of science but an alternative account of what rationality consists in; it is not a matter of following some *a priori* rule of method but is rather a matter of using acquired similarity relations to match puzzles and solutions. I shall return to Kuhn's conception of rationality below.

As it was, the significance of Kuhn's ideas was not properly appreciated at the time. The was partly because Kuhn's broad use of the term 'paradigm' obscured the core idea of the paradigm-as-exemplar, requiring him to make this idea more explicit in the Postscript to the second edition of *Structure*. It was also because Kuhn's naturalism, in both respects, was novel in philosophy. As mentioned, Kuhn's naturalism pre-dated Quine's 'Epistemology naturalized' by the best part of a decade. And more importantly, Quine was only proposing naturalism, not exhibiting it—it was Kuhn's naturalistic *practice* (without a wrapping of philosophical explanation) that nonplussed philosophers. A third component was social. The fact that science is a social enterprise links the cognitive function of exemplars in individuals to the large-scale pattern in the history of science. So individual scientists, during normal science at least, acquire their similarity relations (their ability to recognize similarities between puzzles and between puzzle solutions etc.) by enculturation during the process of education and training. This key social element in Kuhn's story contrasted starkly with the individualist paradigm in epistemology and so seemed inconsistent with the possibility of justified belief in science. His view being misunderstood, Kuhn was taken to endorse an irrationalist conception of science: according to this accusation, he held that the development of science is the manifestation of 'mob psychology' (Lakatos 1970: 178).

While this meant that Kuhn's work was under-appreciated by philosophers of science, it did also lead to its being held to provide an opportunity for other students of science. Not only does the Kuhnian picture gives a central role to social practices (in enculturating paradigms), it permits a wider integration of science as a field of activity into the traditional concerns of historiography and sociology. For if the outcome of a scientific dispute is not determined by rules of rationality, then there is room for those outcomes being determined by social and political forces, just as they are outside science. Historians and social theorists had earlier tended to think that even if there is some room for explaining the social setting of science and how this influences the general development of science, there was only limited opportunity for distinctively historical and social explanations of the content of the particular theories that prevailed. So while Robert K. Merton (1938) linked the scientific revolution in seventeenth century England to protestant pietism, that explanation did not seek to explain the detailed products of that science, such as Harvey's hypothesis of the circulation of the blood, Newton's gravitational theory, or Boyle's law. Kuhn's approach seemed to open up the possibility that these too would submit to historical and social explanation—Kuhn's work lies at the origin of the contemporary study of science, inaugurating what Collins and Evans (2002) call the 'second wave' in science studies. The most significant and philosophically most sophisticated school within this second wave is the Edinburgh School's Strong Programme in the Sociology of Scientific Knowledge. The latter was formulated as such in explicit contrast to the *weak* programme, which desists from applying sociological analysis to the content of scientific belief except in pathological cases.

Kuhn (1992: 8–9) himself, in the strongest terms, repudiated the Strong Programme,

> ... has been widely understood as claiming that power and interest are all there are. Nature itself, whatever that maybe, has seemed to have no part in the development of beliefs about it. Talk of evidence, of the rationality of claims drawn from it, and of the truth or probability of those claims has been seen as simply the rhetoric behind which the victorious party cloaks its power. What passes for scientific knowledge becomes, then, simply the belief of the winners.
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> I am among those who have found the claims of the strong program absurd: an example of deconstruction gone made. And the more qualified sociological and historical formulations that currently strive to replace it are, in my view, scarcely more satisfactory.

While it may rightly be said that Kuhn is responding to a caricature of the Strong Programme, these remarks nevertheless demonstrate that there is a significant gulf between Kuhn and those who regard themselves are working in a Kuhnian tradition. Donald MacKenzie (1981: 2–3) contrasts two views of the socio-historical investigation of science, corresponding to the weak programme (or first wave of science studies) and to the strong programme (the second wave). The former discusses 'factors [that] could hinder or promote work in the field and perhaps condition the quality of the work done', but which, 'are, however, not taken ... as explaining the content of the theoretical advances.' The is exemplified by Joseph Ben-David's work (1971) on the growth of statistics in the United States and Britain and the social context that allowed particularly rapid theoretical advance in Britain. MacKenzie regards his own work, which links the content of these advances to the promotion of eugenics in the interests of Britain's professional middle classes, as exemplifying the second view, according to which social factors play a role in the process of producing new ideas, and, furthermore (in a stronger version of the second view), in determining their acceptance or rejection. *The Structure of Scientific Revolutions*, says MacKenzie, 'can be read as a statement of this second view in its strong version.'

At this point it will be useful to make two distinctions that are both vague and contested. For our purposes, the more important is the distinction between explanatory factors that are internal to science and those that are external to science. The rough idea of this difference is clear: if a scientist promotes adoption of an idea because she believes it solves an agreed puzzle, then that explanation is internal to science; but the explanation is external if, for example, the idea is promoted because the scientist, who holds a patent, has a financial stake in its being believed. The key distinction may be taken to be something like this: a factor that influences theory construction and choice is internal to science if it is among those factors that the scientific community takes to be a rational in the sense of being conducive to the epistemic aims of science. Otherwise a factor is external. When Copernicus retained the circular motion central to Ptolemy's cosmology, he did so because he reasonably believed this to be a feature that would contribute to accurate puzzle-solving in cosmology, and so is an internal feature. Whereas, according to Farley and Geison (1974), what they call 'extrinsic' factors such as conservative politics and religion played a part in forming Pasteur's views and enabling their triumph over those of Pouchet; the amenability of a scientific idea to a certain religious or political perspective does not lead to improved puzzle-solving, nor would it reasonably have appeared so to the scientific community of the time, and so such factors (if they did play this role) are external.

Whether or not one regards this distinction as important or useful (or indeed one that can be properly made at all), Kuhn himself employed it. As MacKenzie correctly notes, 'In Kuhn's work the society that influences scientific judgement is typically taken to be the community of practising scientists in a particular specialty.' For Kuhn those factors are therefore internal. MacKenzie (1981: 4) himself, regards the factors he discusses as including external factors, and he sees this as an extension of Kuhn's approach:

> But there seem to be no overwhelming grounds why only social factors internal to the community of scientists must necessarily be at work. If the basic point of the social nature of scientific judgement is taken, then it seems reasonable to search society at large, as well as the scientific community, for determinants of it.

MacKenzie clearly sees his externalist approach as being a natural extension of the lessons learned from Kuhn. As I shall explain, this is to gravely misunderstand Kuhn. As we have seen, Kuhn distances himself from the Strong Programme, and even if the strength of his rhetoric is not justified by a more nuanced understand of the Strong Programme's aims, it nonetheless remains the case that Kuhn has excellent reasons for maintaining a robustly internalist approach, one that is very much close to Ben-David than to MacKenzie.

The second distinction is between micro-social and macro-social explanations. Macro-social explanations refer to large-scale social structures and changes, such as political, religious, or economic forces, that play a role in many aspects of society. Micro-social explanations refer to explanations that focus on interactions between agents in fairly restricted groups. A macro-social investigation of industrial unrest would be concerned with the economic climate, social attitudes to strikes, relationship with other political agendas, legislative context and the like. A micro-sociological study would focus on the various kinds of relationship of power and interest (and perhaps collegiality or suspicion) at work between union officials, industrial bosses, the workers, and party leaders. The two kinds of study are not mutually exclusive and may overlap, but the kinds of focus are clearly different. The first wave of science studies concerned itself with macro-sociological explanations. Merton's work is a notable example, as is Hessen's 'The Social and Economic Roots of Newton's *Principia*' (1971). Micro-social studies are more recent, exemplified by the work of Collins (2004) on the interactions of networks of individual scientists working on gravity waves.

The relationship between externalism/internalism and macro/micro is complex. Whether a macro-sociological study is externalist or not will depend on what it aims to explain. If it aims to explain the content of the beliefs that come to be accepted as knowledge, then it will be externalist. Merton, and indeed Hessen for that matter, do not aim to tell us why *these* theories were adopted in the scientific revolution and not those. MacKenzie, on the other hand, does aim at showing how the content of statistical theory was influenced by eugenics and the interests of the professional class, and so is externalist. When it comes to micro-social explanations, the distinction between internalist and externalist explanation is less straightforward (and in many cases less pertinent). Typical of such explanations is a reference to the interests of an actor. If such explanations are epistemic, then the explanation is straightforwardly internalist. But even interests that are not epistemic, such as professional ambition, might be consistent with having other motivations that are. Thus the micro-social explanations of Collins's work can be read in a mostly internalist way.

So we have two strands to Kuhn's naturalism: a historical-sociological one, which led to a major trend in science studies, a trend which Kuhn himself rejected. And a cognitive-psychological naturalism that was rejected by Kuhn's philosophical contemporaries. While the latter has been under-appreciated, the former has been over-appreciated. Neither has been properly understood. I shall concentrate largely on the science studies movement as putative heirs to Kuhn's socio-historical naturalism before returning to discuss recent rehabilitation of Kuhn's cognitive-psychological naturalism. In this article I argue that insofar as they take a predominantly externalist approach, students of science studies are not Kuhn's true heirs. Kuhn had very good theoretical reasons for rejecting externalism. I shall explain why Kuhn did, unwittingly, play a major role in creating the environment in which science studies could flourish. After suggesting that Durkheim's functionalism would provide a suitable basis for future sociology of science, I shall conclude by suggesting that the naturalistic study of science has hitherto been best represented by recent work on the psychology of scientific research, in particular work on model-based and analogical reasoning.

2 Kuhn and externalism in the study of science

In a phrase that has elicited scorn from Steve Fuller (2002), I have described *The Structure of Scientific Revolutions* as 'theoretical history of science' (2000: 29), by which I mean it does two things that have an analogue in natural science:

> (i) a *descriptive* element—*Structure* identifies a general pattern in the development of science: science is a puzzle-solving enterprise which shows a cyclical pattern of normal science, crisis, revolution, normal science . . . ;
> (ii) an *explanatory* element—it proposes an explanation of the pattern identified in (i): puzzle-solving is driven by adherence to a *paradigm* (an exemplary puzzle-solution).

It is important to appreciate that if Kuhn's theory is to be correct, the drivers of scientific change must be largely internal to science itself. Just taking the descriptive element on its own, it would be odd if the development of science showed this regular, cyclical pattern yet the causes of the pattern were to be found outside science. One might reconcile patterns in science with external forces, if one could show that those external forces themselves show a similar kind of regularity. But that is implausible. The oddity in my phrase 'theoretical history' is precisely that historians for the most part do not believe that there are patterns in history; and even those that have been proposed, such as Marx's, do not fit with Kuhn's patterns. Kuhn opposed the extension of his ideas beyond science—so a true Kuhnian could not claim that the pattern found in science is an instance of some more general pattern found in the development of, for example, societies at large. If there is a Kuhnian pattern in science, then the best explanation of it will be one that refers to factors internal to the activity of science.

And indeed, as we know, that is precisely what Kuhn offers us in the explanatory part of his theory. During normal science scientific advance is driven by a paradigm or set of paradigms, which are exemplary scientific puzzle solutions. Kuhn spells out in detail how these fulfil a myriad of key functions in normal science: fixing concepts, training scientists to see puzzle solutions, providing a standard for the assessment of proposed puzzle solutions, and so forth. Since, Kuhn emphasizes, the bulk of scientific activity is normal science, it follows that at least most scientific change is governed by factors internal to science.

Consequently, Kuhn has good reasons stemming from his central commitments of his theory for maintaining a predominantly internalist picture of science: '. . . the ambient intellectual milieu reacts on the theoretical structure of a science only to the extent that it can be made relevant to the concrete technical problems with which the practitioners of the field engage' (1971: 137–8). Kuhn does make room for two ways in which external forces do play a part in science. First, such factors may be significant at the inception and early development of a discipline (Kuhn 1968: 118). Secondly, they may play a role in determining the timing of a scientific development (Kuhn 1962: 69; Kuhn 1968: 118). Note that neither of these contradicts the position articulated above, of which the central claim is that the content of scientific change is determined by factors internal to a tradition. Tautologously, that tradition cannot be brought into existence by factors internal to it. And it is no rejection of the claim to accept that the pace of change is constrained by external factors such as the resources society is able to put into science. These exceptions thus prove the Kuhnian rule, that scientific change is driven by the internal requirement of problem-solving. Returning to MacKenzie's contrast between Ben-David's view of social forces and his own, we see that Kuhn is much closer to Ben-David than MacKenzie. Both Kuhn and Ben-David restrict the principal effects of social causes on the development of a field to its acceleration or retardation and reject effects on change in its content. Hence it is at best misleading to portray the extension of science studies to include social causes of change in content as a further step in a direction Kuhn had already pointed us in. Whatever may be true of students of science in the 'first wave', adherence to internalism is not a 'failure of nerve' (Bloor 1991: 4) in Kuhn's case, but a theoretically well-motivated commitment.

A natural response may be to point to revolutionary science as being the locus of the play of external social forces within a Kuhnian framework. Yet I believe this to show a misunderstanding of Kuhnian revolutions, and that as regards the principal driver of scientific progress, there is considerable continuity between normal and revolutionary science; for in both the central concern is with the need to solve scientific puzzles. What differs is the manner in which this driver is manifest. During normal science, the need to solve problems is satisfied by the paradigm. During extraordinary science the need remains, but now must be satisfied by finding a replacement paradigm. What determines the outcome will still be, principally, the power of a proposed paradigm to solve puzzles. That may not determine the outcome uniquely—Kuhn stresses that there is room for rational disagreement about the relative problem-solving power of a proposed new paradigm compared to the old one or a competitor. Nonetheless, the fact that the dispute is about scientific puzzle-solving power puts significant constraints on the choices available. The participants in the debate must be able rationally to believe that their favoured solution will deliver more and better puzzle solutions than its competitors. In particular supporters of a new paradigm must, in most cases, be able to show that it solves a sizeable proportion of the most significant anomalies that beset the old paradigm, while also preserving the bulk of the puzzle-solving power of its predecessor. Since finding an innovative solution that achieves this is not easy, most episodes in revolutionary science will provide very few choices, typically just the old paradigm and a single revolutionary proposal. Many critics, from Toulmin (1970) onwards, myself included, have argued that Kuhn overemphasizes the difference between normal and revolutionary science. But we should not ourselves read into Kuhn an account of the difference that makes the two utterly unlike one another; Nickles (2012), in this volume, suggests that the best Kuhnian account of the use and development of exemplars should allow for innovation with regards to exemplars in normal science as well as the retention of exemplars through extraordinary science. There is a caricature of Kuhn's scientific revolutions as root-and-branch revisions, complete breaks with the past, whose outcomes are determined in a manner quite unlike the determination of episodes in normal science. Yet Kuhn himself argued for significant similarities between the two kinds of episode. Perhaps the least read chapter in *The Structure of Scientific Revolutions* is the one entitled 'Continuity Through Revolutions' in which Kuhn describes the constraints imposed on the new paradigm by the long-standing success in puzzle-solving of its predecessor. Such constraints, which include satisfaction of Kuhn's five scientific values (accuracy, consistency, breadth of scope, simplicity, fruitfulness), mean that there is significant continuity in revolutionary science. Consequently, given the infinite range of beliefs a scientist could have about a given subject matter, all but a small handful are excluded by factors internal to science, even during extraordinary science.

The moral of this section is that consideration of Kuhn's mode of explanation for both normal and revolutionary science is incompatible with significantly externalist elements in an explanation of a scientific development. Given that Kuhn states his internalism and that his theory gives him reason to do so, why has Kuhn been seen as a promoter of externalism? In particular why has the (internalism-supporting) continuity afforded by the unchanging puzzle-solving imperative in Kuhn's picture not been sufficiently appreciated? I suggest that there are two reasons. The first is the overemphasis given to one unrepresentative passage in *The Structure of Scientific Revolutions*. The second centres on the alleged irrationalism of Kuhn's view, which if it were correct, would fit well with a significant role for social influence on the content of science.

One passage in *Structure* has been a particular source for those who wish to take an externalist lesson from Kuhn.

> Individual scientists embrace a new paradigm for all sorts of reasons and usually for several at once. Some of these reasons—for example, the sun worship that helped make Kepler a Copernican—lie outside the apparent sphere of science altogether. Others may depend upon idiosyncrasies of autobiography and personality. Even the nationality or the prior reputation of the innovator and his teachers can sometimes play a significant role.

On inspection, this passage (Kuhn 1962: 152–3) is not so thoroughly externalist as may be supposed. On the one hand Kepler's sun worship is certainly externalist, and so is nationality. But the other factors are not. Note that Kuhn *contrasts* these other factors with the clearly externalist sun worship. So Kuhn (1977a: 325) tells us that 'some scientists place more premium than others on originality and are correspondingly more willing to take risks', for example. Frank Sulloway (1996) has shown that birth order plays a significant part in one's readiness to accept unorthodox opinions. Such a readiness, a rebellious temperament, is part of one's personality. Should this count as externalist? Arguably yes, since is a cause of behaviour that originates outside science. Yet, at the same time what is affected is a differential response to the relationship between evidence and theory. Below I shall argue that Kuhn has a conception of scientific rationality that accommodates disagreement about the import of evidence. Indeed, Kuhn himself suggests elsewhere that such individual differences are healthy for scientific progress, since a greater variety of ideas will be pursued, and that the mean effect of such differences coincides with what a more traditional view would regard as rationally recommended. Also under the heading of biography, are factors that (Kuhn 1977b) 'result from the individual's experience as a scientist. In what part of the field was he at work when confronted by the need to choose [between theories]? How long had he worked there; how successful had he been; and how much of his work depended on concepts and techniques challenged by the new theory?' While these are matters of individual biography, they relate, as Kuhn says, to the individual's experience *as a scientist*. Kuhn also mentions reputation, a social category. As I noted above social explanations are not necessarily externalist. If a scientist's opinion carries weight because she has an impressive scientific record, then that factor is unproblematically internalist; it remains the case that the community's driving interest is effective puzzle-solving.

More importantly, not only is this short passage only partly externalist it is followed by a much longer and more detailed discussion of clearly internalist factors, which commences, 'Probably the single most prevalent claim advanced by the proponents of a new paradigm is that they can solve the problems that have led the old one to a crisis. When it can legitimately be made, this claim is often the most effective one possible' (Kuhn 1962: 153). And it is most effective when 'the new paradigm displays a quantitative precision strikingly better than its older competitor' (1962: 153–4). Kuhn then goes on to point out that solving the old paradigm's anomalies is not always necessary, for sometimes the new candidate paradigm might not provide solutions to the crisis-evoking problems. In that case novel predictions, predictions of phenomena that would be entirely unexpected under the old paradigm can be persuasive (such as the prediction of the phases of Venus by Copernicus's theory). Kuhn then mentions the role of aesthetic considerations. He also discusses at length the characteristic of revolutions we have subsequently called 'Kuhn-loss' and the importance of a new paradigm being a fruitful basis for new problem-solving research. In assessing whether Kuhn gave direct encouragement to externalist study of science, we must set the short quoted passage against the six pages that follow.

Those who promote SSK often draw upon the Duhem–Quine thesis to support their view that social and political factors must play a role in determining which scientific theories are adopted.[1] The idea is that rationality plus the evidence is never enough to determine a single scientific conclusion—there are always equally acceptable competing hypotheses available. Consequently factors in addition to the evidence must always play a part in explaining the choices that are actually made, and one natural supposition is that these factors are social and political. At first sight, Kuhn's picture seems to fits neatly into this pattern. Does he not avow a sociological conception of 'paradigm' which he renames the 'disciplinary matrix', emphasizing the socialization of new recruits to the scientific profession?

While that is true, it does not support the thesis that external elements play a determining role. During normal science successful puzzle solutions are usually determined by the evidence plus the background scientific commitments of the scientist. Take for example the normal science task of determining with greater accuracy the value of some important constant. Frequently, so long as the task is carried out competently, there will be no room for alternatives. The fact that the background commitments employed in this process may be acquired by a process of socialization into a disciplinary matrix does not mean that they are extra-scientific. As I have just argued, even in Kuhn's account of scientific revolutions the factors that influence decisions are predominantly internal to science. It may be true that such factors do not fix a *unique* rational preference—Kuhn tells us that in such periods rational men may disagree.[2] But this is not to concede that there is need to appeal to external factors. Consider two men watching a sporting contest, who disagree at half-time about who will win: Dave maintains that Bath will win because they have scored more points in the first half while Mick maintains that Bristol's greater ball possession and increasing dominance will lead to their victory. They may agree on the evidence but weigh its significance differently. It need not be that their difference in opinion is motivated by considerations external to the game in hand. A notion of rationality is needed that does not mandate (at most) a single rationally acceptable outcome among competing theories. Tradi-

[1]Barnes (1981: 493, 1990: 63–4) is particular example. He also attempts to pin this on Kuhn also, referring to 'Kuhn's famous dramatisation of the insufficiency of reason and experience in scientific evaluation'.

[2]I believe that Laudan (1990) hugely over emphasizes Kuhn's commitment to underdetermination. In any case, to the extent that there is *some* underdetermination during revolutionary science, that is a consequence of the account, not an assumption, whereas for Barnes, underdetermination is an assumption taken from philosophers of science.

tional accounts of scientific method do have the consequence that there is at most a single rational response to the evidence, whereas Kuhn *rejects* such a view.

The underdetermination arguments of Barnes et al. employ a conception of rationality inherited from logical empiricism.[3] The latter tends to see rational scientific inference as an extension of deductive logic (Carnap's inductive logic is the prime example). If we employ such a conception of rationality it is will indeed appear, as Barnes (1990: 63) tells us, that 'reason and experience do not sufficiently determine the knowledge which an individual scientist should adopt.' However, the extent to which reason and experience fail to determine scientific choices rather depends on what counts as 'reason' (and for that matter, 'experience' also). Since a richer conception of reason will make a choice more determinate whereas a thin conception of reason, e.g. limited to deductive logic, will leave a great deal more undetermined, Barnes's claim would appear to depend on his accepting the limited account of reason just mentioned. That commitment, it may be noted, would thereby seem to be in tension with the rationality version of the symmetry principle, as presented by Barnes and Bloor (1982). For the latter should preclude the analyst from adopting any specific account of what is rational or not. A more Kuhnian conception of rationality is not only less susceptible to the underdetermination argument, it will also be, I think, consistent the relativism implied by the symmetry principle. For as our acquired similarity relations change, so will what it is rational to believe, even if the evidence *per se* does not change. Furthermore, unlike the logical empiricist account, it will allow that incompatible propositions might both be believed on the basis of the same evidence. Although one might wish to explain differences in opinion, if differing conclusions may both be rational responses to the evidence, then acceptable explanations of that difference need not include external social explanations.

This leads to the second reason why externalism has been attributed to Kuhn: his alleged irrationalism. The standard logical empiricist account says that scientific change is answerable to (if not exactly driven by) unchanging rational rules of logic and confirmation; these rules, which may be applied algorithmically, determine a unique rational answer. Kuhn (1962: 199) remarks, 'Debates over theory-choice cannot be cast in a form that fully resembles logical or mathematical proof.' While Kuhn thinks this point is long familiar in philosophy of science, his own articulation of the process of theory choice was not familiar: 'The practice of normal science depends on the ability , acquired from exemplars, to group objects and situations into similarity sets which are primitive . . . ' (1962: 200). The divergence between the logical empiricist account of scientific thought and

[3]Kuhn (1992: 9) makes a related, but slightly stronger point he attributes to Marcello Pera, that 'The authors of microsociological studies are . . . taking the traditional view of scientific knowledge too much for granted.'

Kuhn's meant that the latter was immediately characterized by his detractors as an irrationalist view, as Kuhn (1962: 199) himself notes. But once one has decided that Kuhn is portraying scientists as irrational it doesn't much matter where in his picture the influences on their choices come from. As Roy Porter (1990: 38) says,

> Because Kuhn argued that paradigms were strictly speaking incommensurable, i.e. one could not be judged better than its predecessor according to an accessible objective truth criterion, his opponents accused him of encouraging a subjectivist and relativist view of the status of science. If that were so, factors which were (in the wider sense) ultimately social would count, after all, in theory choice. Kuhn later backtracked on various of the inferences derived from his ideas, but his hyper-influential book gave impetus and respectability to many new currents from the mid-1960s.

The difference between internal and external causes of irrational belief hardly mattered and was overlooked by his critics as insignificant when compared to the important distinction between rational and irrational belief. I suspect that many in the science studies world overlooked the distinction for the same reason, although taking a different view of whether the charge amounted to a criticism of Kuhn.[4]

Kuhn spends much of the Postscript resisting the charges of subjectivism and irrationalism. This contrasts with his response to the charge of relativism, which, with appropriate explanations, he is willing to accept (1970: 207). We should conclude therefore that Kuhn is not rejecting rationalism about science.[5] He rejects a particular, logical empiricist account of rationality, one that sees it as an extension of deductive logic and which denies the possibility of rational disagreement. And in place of the latter, Kuhn is in effect offering a different conception of what rational science is: one that is anchored in certain shared similarity relations.

There are two features to the shared similarity relations that are important for understanding Kuhn's revisionary approach to rationality. The first is social: these are *shared* relations. For example, Kuhn tells us that theory-choice is dependent upon the values scientists hold (such as consistency, fruitfulness etc.) and which are acquired via training with paradigms. He (1962: 185–6) writes,

[4] This is how Roy Porter (1990: 37), for example, sees things. As regards the social determination of the content of science, he writes, 'Of crucial importance in this respect—not necessarily for what it overtly stated but for what were taken to be its implications—was Thomas S. Kuhn's *The Structure of Scientific Revolutions*.'

[5] This is an important point—Kuhn frequently appeals to what it is rational for scientists to believe or do, but nowhere says that science is not a rational enterprise—but a point that is ignored by sociologists of science who appeal to Kuhn, for example Barnes (1990: 64). Barnes does acknowledge that it is a misconstrual of Kuhn to see him as attacking science; but it is clear that to see him as rejecting the rationality of science is also a misconstrual.

> To many readers of the preceding chapters, this characteristic of the operation of shared values has seemed a major weakness of my position. Because I insist that what scientists share is not sufficient to command uniform assent about such matters as the choice between competing theories or the distinction between an ordinary anomaly, and a crisis-provoking one, I am occasionally accused of glorifying subjectivity and even irrationality. But that reaction ignores two characteristics displayed by value judgments in any field. First, shared values can be important determinants of group behavior even though the members of the group do not all apply them in the same way. . . . Second, individual variability in the application of shared values may serve functions essential to science.

Kuhn's second point in this passage is an important reflection on group rationality and is connected to the evolutionary account of change he briefly introduces in the Postscript to the second edition of *Structure*. Some processes, such as evolution, depend on variation, especially at times of stress. Hence it is helpful to progress for the standards applied not to fix a unique outcome but to permit a variety of possible views. Thus science must permit it to be rational for scientists, in some cases at least, to disagree without either being irrational. We must see individual rationality as bound up with the rationality of the group enterprise. And since the intuitions and similarity relations employed by scientists are shared, not merely individual, Kuhn can reject the charge of subjectivism.

The second feature of Kuhnian rationality is a matter of individual psychology: the central role of acquired intuitions about similarity, the ability of a scientist to recognise the similarity between a scientific puzzle and a paradigm and likewise the similarity between their solutions. Kuhn does not attempt to articulate a detailed account of rationality on this basis, but the following, is, I suggest, how such an account might begin. A naturalized conception of rationality locates it not in dispositions to follow logical or other *a priori* but in capacities that fill a cognitive role. At a first approximation, a subject is acting rationally when she follows a cognitive procedure that meets the subject's cognitive goals. On this view, identifying people by recognizing their faces is an exercise of rationality (albeit a minimal one). Scientific rationality is a matter of acting on a similarity relation that contributes to the pursuit of science's cognitive goals, the solving scientific puzzles. (So not every acquired similarity relation contributes to being rational.) As such, the rationality of a scientist is much more akin to the ability of an art connoisseur to recognise the work of an old master by its brushwork that it is to that of a logician who is able to verify a proof using the rules of logic.[6] Similarity

[6] Two further points may be made here. The ability of the mathematician to *construct* the proof may depend on similarity relations acquired by training and experience. And even the logician's

is not quantifiable but is learned by exposure to exemplars. Furthermore, the relevant features that go to make up a similarity relation are multiple and imperfectly commensurable. So there is no reason to suppose that (at most) a single conclusion is rationally mandated or that scientists in possession of the same evidence cannot rationally disagree (Kuhn 1962: 199).

We should note that because scientific rationality consists in exercising capacities that promote science's cognitive goals (maximizing the puzzle-solving power of science) there is scope for a clear demarcation between rational and irrational scientific behaviour within Kuhn's picture. An irrational scientific choice occurs when one other things being equal prefers a hypothesis that fails to solve puzzles over one that does. And such choices will typically occur when the choices are influenced by factors external to science.[7] Thus Kuhn is able to make the same judgment of, for example, Lysenkoism that the logical empiricists would have, viz. that it is degenerate, irrational science.

To summarize this section: Kuhn's theory requires him to take a predominantly internalist approach to explaining science scientific change. This is true of revolutionary change as well as developments in normal science, Kuhn's few remarks about Kepler and sun worship and the like notwithstanding. This fact has been ignored by both Kuhn's critics and by proponents of externalist approaches in science studies,. This is in part because both groups adopt a restricted, logical empiricist conception of rationality. Kuhn's account does indeed conflict with that conception, but not because he regards scientists as irrational but because he is offering a different, more naturalistic conception of what rationality in science is.

3 The Strong Programme and naturalism

Now I want to turn to the characteristic that Kuhn and at least one important strand in science studies do share. Kuhn's approach and that avowed by the Strong Programme are both naturalistic. By a 'naturalistic' approach to some subject matter, I mean an approach that is willing to employ the methods and results of the natural sciences. If our subject matter is the nature of scientific thought and development, the approach of the logical empiricists was decidedly non-naturalistic. It was al-

ability to see that a step in the proof instantiates a rule might be seen as another example of pattern recognition. So perhaps we should not contrast rule-following and acquired similarity relations but instead regard rule-following as just a special case of applying similarity relations. See Bird 2005.

[7] Often things will not be equal in extraordinary science, for example because of Kuhn-loss. Consequently, it will be possible, as emphasized above, rationally to continue to resist a revolution, as Kuhn remarks is true of Priestley. Nonetheless, the room for being a rational hold-out will diminish over time as the promise of a new paradigm is converted into proven success.

most entirely *a priori*. According to that view, the principal task for a philosopher of science is to find an *a priori* account of scientific rationality, be it an account of confirmation or of the logic of induction or, at least, of refutation. This, plus the minimal assumption of the rationality of most scientists, provides an account of what scientific thought in fact is. Likewise we have a schema for explaining, in the general case, why a scientist believes what he does believe—we refer to the evidence that scientists had available, and the account of scientific rationality does the rest. Only in the peculiar cases, where a scientist is behaving irrationally do we need to appeal to any additional factor and a form of explanation drawn from outside philosophy (e.g. a psychological or social explanation).

What was revolutionary about Kuhn was his willingness to apply social and most particularly psychological explanations in the general case (which gave further impetus to Kuhn's being perceived to be an irrationalist about science.) We have discussed Kuhn's innovative naturalism above: science is driven by judgments of similarity to exemplary scientific puzzles solutions—paradigms-as-exemplars; an ability to make such judgments may be learned by practice with the paradigmatic exemplars and related kinds of example (such as textbook and exam questions). Such acquired dispositions have significant effects; they can affect one's perceptual judgments and more generally they channel one's thinking so that only puzzle solutions akin to the exemplars are likely to be spotted.

The sociology of scientific knowledge and the Strong Programme in particular are also avowedly naturalistic. SSK aims to use the methods and findings of science—social science—to understand the creation of scientific consensus (for which the word 'knowledge' has become a term of art). David Bloor (1991: 21) tells us,

> The search for laws and theories in the sociology of science is absolutely identical in its procedure with that of any other science. This means that the following steps are to be found. Empirical investigation will locate typical and recurrent events. Such investigation might itself have been prompted by some prior theory, the violation of a tacit expectation or practical needs. A theory must then be invented to explain the empirical regularity. This will formulate a general principle or invoke a model to account for the facts.

So the sociology of science employs the same general method as other sciences, the same naturalistic approach that I attributed to Kuhn in *Structure* in Section 2.

One of the problems, however, for SSK is how to distinguish itself from history of science. After all, the mere reference to social factors in explaining some change in scientific belief doesn't make the explanation sociological. Most historical explanations of ordinary historical events appeal to social factors. Barry Barnes (1990) in an article on sociological theories of scientific knowledge mentions four studies that exemplify SSK by showing how specific objectives and interests within scientific communities play a role in explaining how consensus was formed on particular beliefs—Dean's work on theories of plant species, Pickering on elementary particle models, MacKenzie on statistical measurement, and Shapin on the pictorial representation of the human brain. But the most avowedly externalist studies, those of MacKenzie and Shapin, do not strike one as manifestly distinct from historical studies that seek social explanations of scientific belief change, and Dean's work, while focussing on to a greater extent on micro-social relations, is nevertheless largely historical in approach. Returning to the quotation from Bloor, these studies do not display laws and theories nor do they conspicuously formulate general principles or invoke models. Perhaps all that distinguishes them is that they see themselves as contributing to SSK in the sense of giving examples of how social forces play a role in science, but that is not enough to make them distinctively sociological. What would make a study distinctively sociological would be if it employed some genuinely sociological theory, which is to say some contentful and general set of hypotheses about exactly how social forces play a role in science. If SSK could do that it really would be naturalistic, in the sense of employing the methods and results of science, rather than in the weak sense of just being empirical, which provides no distinction from history. Barnes seems to recognize this, and so cites Mary Douglas's grid-group analysis as an example of the sort of the general theory I have mentioned, which would link kinds of social structure to kinds of belief. Yet such theories are few and well-confirmed instances of such theories are even fewer.

Pickering's work is closer in nature to that of Bruno Latour, and like Dean's is concerned with micro-social explanations. Latour (1987) in particular might seem to exemplify Bloor's desire for the sociology of science. For he employs the naturalistic methods of anthropology and deploys a general explanatory theory, Actor-Network theory. Latour's micro-sociological work at first sight also appears less externalist than that of Shapin or MacKenzie. However, in the light of Latour's metaphysics, this is an illusion. 'Social construction' for Barnes, MacKenzie *et al.* refers to the construction of ideas and beliefs by social forces. But for Latour and also for Pickering, the very objects of scientific study are themselves social. In such a context it is difficult to make much sense of the idea of naturalism or of an internal-external distinction.

As we discussed above, before the 1960s a consensus existed that the explanation of the contents of belief must normally be internalist. This allowed for a demarcation between sociology and history of science thus: historians give internalist explanations of the content of science while sociologists give explanations, which would often be externalist, of the form (structure) of science. But with the breakdown of that consensus against externalist explanations of content, the demarcation between history of science and sociology of science become blurred.

External explanation was hitherto exclusively the domain of the sociologists, so to was tempting to retain this as a characteristic feature of sociology, and to extend the reach of sociology to the content of science in addition to its form (we see this in the quotations from MacKenzie above). Yet the inability of studies of this kind, such as MacKenzie's, to distinguish themselves from externalist history, while not detracting form them as works of scholarship, does cast doubt on the success of SSK in developing a distinctively *sociological* study of science. Ironically, while few few products of SSK come especially close to fulfilling Bloor's naturalistic aims as outlined in the quotation above, the work that best fits those aims is Kuhn's *Structure of Scientific Revolutions*. For *Structure* is naturalistic, and does identify patterns which it explains using a general theory/model that appeals to social forces. Yet, as we have discussed, those social forces are resolutely internal.

Is there then a different approach to the study of science, such that SSK could be a genuinely naturalistic and distinctive study of science? I believe that there is. Such an approach would, like Merton's project, concern itself with the structure and function of modern science. It would permit the sociological explanations of why such a structure exists and what social forces help or hinder it. Like Kuhn's theory it would aim to be a general model of the structure of science, which when applied to particular episodes where scientific beliefs have changed would appeal in the normal case exclusively to the internal features of science. I suggest that this approach should take its cue from the work of Émile Durkheim (1893) and the functionalist school his thought gave rise to.[8] Thus we should ask what the *function* of science is, the answers to which question should explain the structure of science. And similar questions may be asked of the substructures of science.

Now some SSK practitioners may claim that they have answered the question about the function of science. Its function is to serve the objectives and interests of those engaged in science. Such a view is more or less explicit in Barnes's account of SSK. But I find such an answer unilluminating. It is the sort of explanation that can be applied to any institution. Of course, SSK may assert that is what science is, just another institution But not all institutions are the same; they have different values (as Merton emphasizes), they have different internal structures, and they relate to other institutions and external structures in different ways. I

[8] Barnes et al. (1996) also use Durkheim's ideas—but a different subset thereof. They refer to Durkheim's *The Elementary Forms of the Religious Life* (1912) in order to draw an analogy between science and religion. Durkheim's key distinction between the sacred and the profane, they say, explains why there is resistance to the naturalistic study of science itself. But I think this fails to appreciate the function of religion, as Durkheim understands it, which is to be a source of social solidarity, causing the individual to put the group's interests ahead of his own. Science does not have this function, and so there is no reason, in Durkeimian terms, why the sacred-profane distinction should be applied to science.

believe that Durkheim's broader conception of function in sociology will help us answer such detailed questions concerning the nature of science. His analogy between society and an organism requires us to see the function of an institution as a matter of its interaction with other institutions and parts of society so that together they contribute to the proper functioning of the whole. This analogy has proved fruitful in sociology in general and I believe that it may be particularly fruitful in relation to the sociology of science.

Many biological organisms have organs or systems whose function is cognitive—which is to say that they are supposed to give the organism information about its environment, thereby influencing its behaviour. One might suppose that social 'organisms' might similarly possess institutions that perform a cognitive function. Certainly that is how one should understand military intelligence and signals corps in relation to the armed forces, and one may consider that publicly organized science plays a cognitive function with respect to society at large. The biological analogy is at best only suggestive here—one should not suppose that every biological organ (the liver, the pancreas etc.) has a sociological analogue. But the fact that science does exist as an institution with an avowedly cognitive role adds weight to the suggestion, as does the fact, as social epistemologists have recognized, that there is an irreducibly collective notion of knowledge, which is used in particular in connection with science, and which is best understood as a social analogue of individual knowing.

Such an approach has been ignored not only because sociological explanations have hitherto been interest-focussed but also because the sociology of science has eschewed use of notions such as 'truth' or 'knowledge' (in the sense of 'knows' that implies that what is known is true). This is in part motivated by an entirely reasonable, methodological preference, expressed in the symmetry principle, that the truth or falsity of some specific scientific belief should not appear in the explanation of why it is held. This is reasonable since the most immediate and informative explanation of a scientist's belief involves the evidence she possesses, not the fact that may be the ultimate cause of that evidence. However, the disdain for the concept of truth in SSK typically goes somewhat further than this, partly because of scepticism about the accessibility of truth and partly because of more general relativist pressures.[9] But if one refuses to avail oneself of standard notions of truth and knowledge, it becomes impossible to think of an institution as having a cognitive function. The very point of a cognitive function is to deliver reliable information, to provide a link with the truth, and to filter out what is false. On the other hand, if one can talk about the truth or knowledge seeking function of

[9] In *Structure* Kuhn talks little about truth, as if he is obeying a methodological symmetry principal. However in the Postscript to the second edition, Kuhn (1970: 206–7) does succumb to a sceptical argument of a Kantian kind; see Bird 2000: 225–32.

science then one is in a position to examine the structure of science with a view to identifying its truth-tropic character and more interestingly, the pathologies from which it may suffer. (The fact that some thing may be a pathology, such as fraud or the distortion caused by commercial science does not mean that it is rare.) For example, the world's leading medical journals have agreed to publish only the results of studies that have been registered prior to their being carried out. The aim of this is to combat the problem of selective publication of results. One could see this simply as some sort of power struggle between journal editors and pharmaceutical companies. But it is much more enlightening to see this as a corrective to a pathology. It strikes me that sociologists could usefully take more interest in such developments.

Of course, if the Strong Programme is genuinely naturalistic then it ought not issue a blanket prohibition on employing the concept of truth or against any view that treats in certain cases true beliefs and false beliefs differently. David Bloor explicitly sets up the Strong Programme against those who assume *a priori* that true beliefs and false beliefs require different forms of explanation. But why is it any better to assume *a priori* that they must always be treated the same? Of course this opposition is more subtle than Bloor sets it up to be, since no-one, not even Lakatos, supposed that *every* false scientific belief requires a special kind of explanation different from true ones. No traditionalist supposed that Newton's belief in action at a distance or in his own equations of motion, or Maxwell's belief in the aether, or Dalton's belief that water is HO, and so forth require some special kind of explanation reserved for science that has gone awry, even though these are all false beliefs. So the distinctions among different kinds of belief and different explanations of them was always more subtle than the distinction between the true and the false. A naturalist ought to regard it as an empirical question whether there are any interesting distinctions among kinds of belief and their explanations, and it ought to be a matter of discovery whether truth or rationality play no part, or some part, or a major part in those explanations. As a corrective to the weak programme a bald symmetry principle may be useful. But if we are going to be more careful, a naturalistic symmetry principle ought to be stated thus: do not adopt *a priori* (i.e. *unless supported by appropriate evidence*) any principle or method that assumes that true and false beliefs must be treated differently. Whether or not one adopts this particular more liberalized version of the symmetry principle, the Strong Programme should, I suggest, be seen as a meta-methodological standpoint, telling us how we should choose our methods and explanatory principles. Understood thus its methodological relativism can be seen to align with naturalism: allow empirical, sociological investigation to reveal whether we (the analysts) need to make use of categories such as 'truth' and 'knowledge'. As such methodological relativism rejects philosophical relativism which gives a non-empirical, *a priori* (negative) answer to this question.

Finn Collin (2011) argues that STS (science and technology studies) have failed to live up to its avowed empirical, naturalistic aims, in his view because of the very intensity of its agenda against science and traditional philosophy of science. According to Collin the need to show that science's susceptibility to social forces is *inevitable* has led Bloor and Barnes to deploy *philosophical* arguments to support their conclusions and so retreat from their stated empiricist commitments. I have mentioned Barnes's use of the Duhem–Quine underdetermination argument. Prominent in the work of both Barnes and Bloor and also Kusch and others is a finitist theory of meaning that draws upon Wittgenstein's private language argument. It is certainly striking that philosophical theory is more prominent than sociological theory in Bloor, Barnes, and Henry *Scientific Knowledge: A sociological analysis* (1996). Collin himself advocates a new role for STS, what he calls *critical sociology of knowledge society*, by analogy with classical sociology conceived of as the science of industrial society. The sociology of knowledge society would ask important and critical questions about the nature of society in which knowledge plays such a central role, for example: Is knowledge society still a class society and, if so, which are its classes? Does a liberal, democratic society offers the best conditions for the generation of scientific knowledge and for its transformation into new consumer goods, produced according to innovative, knowledge-intensive methods? Is the classical Leninist theory of colonialism still valid, or has some parity between the old industrialized world and the 'third world' been achieved through the outsourcing of highly skilled job functions in the new 'knowledge economy'? While Collin's proposal has a different focus from mine, it strikes me that they are at least consistent and may form a coherent whole. For Collin says that a reformed STS would try to identify those social mechanisms that make science more responsive to reality. Like my proposal, it would be 'veritistic' in Goldman's sense (1999), taking *truth* seriously as an explanatory concept (Collin proposes realism as a methodological stance, whose fruitfulness would be evaluated empirically). It is worth noting that some of what has been suggested will not be entirely against the grain of recent STS. Collins and Evans (2002) call for a 'third wave' in science studies which recognizes 'expertise' as a real, analyst's category, which is to say that 'expert' is not just an actor's category (used to designate persons taken *by the groups being studied* to fulfil a certain function) but is a category used by the sociologist to categorize persons with particular objective attributes. This is important, for while it does not directly rehabilitate 'truth' and 'knowledge' as analyst's categories, the implication is that categories of this sort have an objective reality that the sociologist needs to exploit.

4 Thomas Kuhn's naturalistic legacy

I started this article by noting two (related) strands in Kuhn's naturalism: a historical-sociological one and a cognitive-psychological one. While SSK has sought to carry forward the former, I have suggested that it has failed to do so because of its over-emphasis on externalist explanations of scientific belief and a failure to commit fully to naturalism, in particular by equivocating between methodological and *a priori* relativism. By contrast I want to suggest that there is a growing body of research that draws rather more effectively upon the second strand of Kuhn's naturalism. In this section I shall give a very brief overview of this new direction for Kuhn's ideas.

For example, in a paper entitled 'How do scientists think? Capturing the dynamics of conceptual change in science' Nancy Nersessian (1992) introduces 'cognitive-historical analysis', which starts from the assumption that the representational processes and problem-solving strategies employed by scientists are highly sophisticated and refined versions of processes and strategies in more general use. It therefore combines historical case studies of actual episodes with insights from psychology and cognitive science to better understand scientific thinking. Nersessian acknowledges a Kuhnian backdrop to this work, which has the additional merit of explicitly linking the psychological and historical strands I have been discussing.

The programmes of research that connect to psychological aspects of the Kuhnian notion of paradigm themselves tend to fall into two (connected) categories. The first emphasizes the *cognitive* aspects of paradigms and the second emphasizes the *conceptual*. Roughly, the distinction corresponds to those who wish to further a naturalistic explanation of scientific development (problem-solving, theory-change and the like) and those who are focussed more on a naturalistic understanding of incommensurability and conceptual change.

Nersessian's work (1987; 2003), while embracing both directions, has tended to concentrate more on the second, conceptual side. Andersen, Barker, and Chen (1996; 1998; 2006) in particular have progressed this line of thought. Kuhn argues that exemplars fulfil a conceptual function, the extension of a concept being determined by similarity to a set of exemplary cases rather than by an intension. Andersen, Barker, and Chen show that this approach is supported by empirical work (Rosch 1973; Rosch and Mervis 1975) on the use of prototypes in category formation. They argue that this can be used to give a naturalistic explanation of semantic incommensurability, via an understanding of dynamic frames (Barsalou 1992).

In parallel, philosophers have drawn on the work of psychologists in further articulating Kuhnian ideas concerning the nature of paradigms and paradigm change. Howard Margolis's (1987) *Patterns, Thinking, and Cognition* was an early work in this direction. Tom Nickles (2003) has noted the link between Kuhn and recent work in case-based reasoning (c.f. Leake 1998), while I (2005) have related this to psychological work by Kevin Dunbar (1996, 1999) on analogical thinking by scientists.

The naturalistic Strong Programme should therefore be in good company. If we are looking for a Kuhnian legacy, this naturalistic pursuit of Kuhnian themes is very much in the manner of *The Structure of Scientific Revolutions*. But, as I have suggested, it is not clear that SSK has in general taken full advantage of the opportunities that naturalism offers. A telling contrast is between the work of Dunbar and that of Bruno Latour. Both use ostensibly the same methods—micro-investigation of the activities of scientists within a laboratory. Dunbar spent several years working in leading microbiological laboratories in the U.S., Canada, and Italy, filming their work, lab meetings especially, and recording interviews. Dunbar was interested in sociological issues, such as how the organization of a laboratory influences its effectiveness. But his principal interest was the ways in which scientists actually reason. His analysis revealed several types of inference all of which are analogical to a greater or lesser degree, at once giving us interesting general conclusions about the nature of scientific thought and also providing empirical confirmation of Kuhn's account of scientific thought in terms of exemplars. In contrast Latour and Woolgar (1986) show an interest in how scientists reach their scientific conclusions only insofar as this exhibits their techniques of persuasion and negotiation, rather than as mechanisms of cognition. In this respect, Dunbar is an heir to Kuhn's *Structure of Scientific Revolutions* whereas Latour is not.

The naturalism in Kuhn thus has an historical-sociological element and a cognitive-psychological element, and in science studies the former has been given precedence to the almost total exclusion of the latter. Both are necessary for Kuhn's model; within that model the psychological element is causally primary and the social element is secondary. The role of social forces in scientific training in particular but also in career success, changing paradigms, and so forth depends on the possibility of the psychological function of exemplars. A scientific education inculcates what Ludwik Fleck (1979) called a 'thought-style' by training with a recognized set of exemplary puzzle solutions. In normal science the conservative consensus is a consensus on exemplars. In revolutionary science what matters is the ability of scientists to alter their thought-styles. The psychology of a thought-style means that it resists change. But it is not absolutely resistant to change, especially in younger scientists among whom the thought-style has not taken such deep root. Given these kinds of explanation it is difficult to see how one could satisfactorily understand the historico-social significance of exemplars without understanding their psychological function.

5 Conclusion

All in all the link between Kuhn and much of contemporary science studies has been greatly exaggerated. Externalist explanations predominate in the latter which are simply inconsistent with a Kuhnian model of scientific development. As regards the Strong Programme in particular there is a common naturalistic approach, but even here there is a divergence. Kuhn's theory and his naturalism should lead one to focus on psychology as much as on sociology.

Furthermore, I suspect that the outlook for the Sociology of Scientific Knowledge is limited so long as it retains too strong a commitment to the externalism Kuhn rejected and to a version of the symmetry thesis that requires us to ignore truth altogether. So long as that remains we will be unable to ask important questions concerning the function and structure of science that once were and ought again to be central to the sociology of science.

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