Thomas Kuhn's Relativistic Legacy

Abstract

The work of Thomas Kuhn is often held to be the origin of much of the relativism one finds in the philosophy of science and in science studies. Kuhn's relationship to relativism is, however, complex. In this article I examine and assess potential sources of relativism in (i) the paradigm concept; (ii) incommensurability; (iii) Kuhn's argument that truth is transcendent; (iv) Kuhn's claims about 'world-change'. I conclude by looking at Kuhn's relativistic influence on the history of science and the sociology of science.

1 Introduction

Relativism is an issue of considerable interest in the philosophy of science. As elsewhere, this originates in large measure with arguments of a sceptical nature or which regard realism as leading to scepticism. In philosophy of science the relevant arguments are those that suggest that science does not deliver truth. For example the pessimistic (meta-)induction argues that the falsity of past theories is a reason to doubt that current theories are true. If that conclusion is correct about even our best theories, then the notion of (objective) truth employed in this argument cannot be a useful one for the evaluation of theories. In the light of this it is tempting to look for a standard of evaluation that is not objective and absolute but is relative. A popular response to the pessimistic induction is to consider scientific progress in terms of increasing nearness to the truth (verisimilitude), which would retain an objective standard of evaluation. However, some philosophers of science regard any standard related to objective truth, including verisimilitude, as transcendent. Thomas Kuhn took that view, as we discuss below. Arguments for the transcendence of truth are typically general in nature. Of greater interest are the grounds for relativism that are more specific to science and which we find in Kuhn's work. Of particular importance is the idea that standards for the evaluation of theories are set by an established tradition of research (a disciplinary matrix or paradigm). This idea is an instance of a more general phenomenon identified by Kuhn, that of *incommensurability*, a topic that engaged Kuhn for much of his professional life. Incommensurability arises when there is a lack of shared standards for the interpretation or assessment of theories.

Kuhn's work has been influential in the philosophy, history, and sociology of science. The explicit relativism of the sociology of scientific knowledge is strongly influenced (if not exclusively) by Kuhn's thinking and many historians of science adopt a methodological relativism that reflects Kuhn's approach.

2 Paradigms, disciplinary matrices, and epistemological relativism

Kuhn's key theoretical concept, one that is often misunderstood, is the *paradigm*. Kuhn (1970: 175) explained that he used the term in two different ways, one more specific and the other more general. The more specific use equates to the notion of an *exemplar*, an exemplary instance of scientific problem solving. According to Kuhn, the aim of science is the solving of scientific problems (or 'puzzles', in Kuhn's terminology). Exemplars serve as models for future puzzle solving. The success of an exemplar will attract support among scientists, and a field in science can be defined by the exemplars it employs. The content of an exemplar includes not only its theoretical component; it includes the language the exemplar is framed in, the experimental techniques it employs, the metaphysical outlook it embodies, and theoretical or explanatory values. Consequently, participation in a certain field of science will involve commitment not just to certain theoretical claims, but also to a wider range of scientific and metaphysical practices and viewpoints. This set of commitments that characterise participation in a field of science constitutes the paradigm in the broader sense, for which Kuhn also used the term 'disciplinary matrix'.

By exemplifying good science, the exemplar functions both to direct the attention of scientists to certain kinds of problem and also as a measure of their success in solving those problems. Both a new problem and its proposed solution should bear an appropriate degree of resemblance to the exemplar. The exemplar will also establish values, for example, both promoting the value of theoretical elegance and also setting the standard of what counts as an elegant theory. Thus in a number of respects an exemplar plays a crucial role in setting standards.

A consequence of this approach to theory-evaluation is that assessment of piece of scientific research is relative to a paradigm. The disciplinary matrix will encompass a range of commitments and criteria that will be used to assess the research, and which are exhibited in the paradigms that define the field. Consequently, the evaluation of the same proposal but from the perspective of another disciplinary matrix would be different. For example, any proposal for an improved account of planetary motion, would, in the fifteenth century, have to show that the motions proposed involved circular motion in a fundamental way, since this is a key part of the tradition established by the exemplars contained in Ptolemy's *Almagest*. Even Copernicus's heliocentric proposal, revolutionary though it was, sought to preserve this aspect of Ptolemy's work (along with many others). On the other hand, after Kepler, and especially when we get to Newton, circular motion is no longer a necessary part of any good account of planetary motion. Likewise, for Ptolemy, the crystalline spheres provide a mechanism for explaining how planetary motion comes about. Newton does away with mechanisms altogether and replaces them by universal laws that permit instantaneous action at a distance. Consequently a Ptolemaic or even Copernican account of the motion of some plant will be judged poorly by Newtonian standards, in particular for its complexity, and also for requiring different patterns of explanation for planets from objects on the Earth. A Newtonian account of the same motion will be regarded be lacking in important respect from a Ptolemaic perspective, in virtue of the absence of any reference to the circular motion characteristic of heavenly bodies and providing no mechanism for the explanation of their motion. In particular, we can see that the virtue of simplicity although respected in both paradigms, is conceived differently. In Ptolemaic (and Copernican) paradigms, a simple account of the motion of a planet concerns the use of minimum number of superimposed circles (epicycles), whereas for Kepler and especially Newton, simplicity concerns the mathematical form of the general law that the motion obeys.

The significance of this relativism of theory assessment is that it contrasts with the absolutism about scientific rationality that is assumed by Kuhn's principal predecessors. For example, if one thinks that science progresses by the application of one single, general scientific method, then one will think that theory evaluation is not relative to a tradition or paradigm. Rather every theory can be assessed by the one scientific method. The logical positivists exemplified this approach, embodied in particular by Carnap's attempts at constructing an inductive logic. Kuhn rejected the idea of a logic of science—it is similarity to exemplars of successful science that determines the evaluation of a piece of research. And he rejected, more generally, the thought that the means of theory evaluation are common to all fields and to all eras of modern science. Many of Kuhn's contemporaries, even if not positivists, shared the earlier conception of rationality as absolute. They thus saw Kuhn's paradigm relativism as irrationalist. Lakatos (1970: 178), for example, takes it to be a consequence of the claim that all theory-assessment is paradigm-relative, without there being any standards to judge between paradigms, that 'scientific revolution is irrational, a matter for mob psychology'. Popper (1970) accused Kuhn of succumbing to the 'Myth of the Framework' according to which rational criticism presupposes a common framework, such as a shared paradigm. Popper's own falsificationist methodology is supposedly frame-independent, because the relationship between a generalization and a falsifying singular statement is a logical one.

Kuhn's relativism, however, is not as clear-cut as Lakatos's criticism suggests, as if scientific revolutions are irrational free-for-alls in which factors such as rhetoric, social forces, and so forth determine the outcome, and a scientific assessment of the evidence plays no role. Nor is it the case that Kuhn thinks that there are no standards for evaluating theories from different periods of science. Kuhn makes it very clear that there is genuine sense in which science does get better as a result of a revolution; indeed the final chapter of *The Structure of Scientific Revolutions* is titled 'Progress Through Revolutions'.

It is true that Kuhn (1970: 152–3) does mention factors outside science as playing a part in influencing the outcomes of scientific revolutions—e.g. cultural and religious climate, personality, reputation, nationality. Kuhn's brief remarks on this point have been taken up by social constructivists. But Kuhn's few sentences on this must be weighed against several pages in which he explains how the success of a proposed revolutionary paradigm depends on its ability to solve scientific puzzles—recall that for Kuhn the aim of science is above all puzzle solving.

A revolution is the response to a *crisis*, in which the failure of scientists to solve particularly significant anomalies (puzzles that do no find a ready solution within normal science) leads to discontent with the existing paradigm and the search for an alternative. A satisfactory alternative must therefore solve a significant proportion of the anomalous puzzles if it is to be acceptable. And so the paradigm that follows a revolution will generally have more puzzle-solving power than its pre-revolutionary predecessor. On the other hand, that does not provide an absolute and paradigm-independent rule for judging between paradigms. As Kuhn (1970: 109–10) explains:

> when paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions. . . . To the extent, as significant as it is incomplete, that two sci-

> entific schools disagree about what is a problem and what a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms. In the partially circular arguments that regularly result, each paradigm will be shown to satisfy more or less the criteria that it dictates for itself and to fall short of a few of those dictated by its opponent. There are other reasons, too, for the incompleteness of logical contact that consistently characterizes paradigm debates. For example, since no paradigm ever solves all the problems it defines and since no two paradigms leave all the same problems unsolved, paradigm debates always involve the question: Which problems is it more significant to have solved?

A revolutionary paradigm will not be an addition to its predecessor but will differ in significant ways. Consequently, some features of the previous paradigm will have to be rejected. These may even include some features that give it its puzzle-solving power. The fact that a revolutionary paradigm does not recover all its predecessor's puzzle-solving power is referred to as *Kuhn-loss*. Kuhn-loss is exemplified by Newton's quantitative force acting at a distance account of gravitation in comparison to Descartes' qualitative vortex account; the former superseded the latter because of its superior predictive and unifying power, but is unable to account for the fact that planetary rotation is co-planar and all in the same sense (direction), features neatly explained by the vortex view. Thus an improvement in puzzle-solving power cannot be decided in a simple logical manner (e.g. A has more puzzle-solving power than B when it solves all the puzzles of B and some additional puzzles). Nor cannot it be decided arithmetically (e.g. A has more puzzle-solving power than B when it solves a greater number of puzzles than B) because that ignores the greater significance of some puzzles in comparison to others. And the significance of a puzzle is paradigm relative (e.g. the measurement of molar masses of chemical elements becomes a much less significant task once isotopes are discovered). Furthermore, the full puzzle-solving power of a new paradigm will not typically be apparent, but will be a matter of future promise (Kuhn 1970: 158).

Thus there is a general criterion of paradigm choice, puzzle-solving power, but the application of the criterion is itself subject to some paradigm-relativity. Analogous comments may be made with respect to the five values that Kuhn identifies as being common to all paradigms: accuracy, consistency, breadth of scope, simplicity, and fruitfulness. Although these values are super-paradigmatic, their application is not. What counts as simplicity may differ from one paradigm to another: Are Copernican orbits simple (because based on circular motion) or complex (because they require many epicycles)? Are Newtonian orbits complex (because not following, exactly, the path of any simple geometric figure) or simple (because they are determined by a few simple equations)? Furthermore, the weighting of these values may vary from paradigm to paradigm. Thus, for Kuhn, the choice between two or more competing paradigms, e.g. between an existing paradigm and its potential replacement, is not decided by logic or any other super-paradigmatic rule. Preferences between competing paradigms will be influenced by paradigm-relative factors. But this relativity is nuanced. It is not an 'anything goes' relativity. Not just any proposed would-be paradigm can be set up in opposition to an existing one and claim to be better by its own standards. The new proposal may not solve any of the puzzles of the current paradigm and may not be more fruitful by any sensible standard. For example, defenders of homeopathy often describe it as a new paradigm in medicine in opposition to standard 'allopathic' medicine (e.g. Rubik 1995). For this to be true, it would have to be the case that homeopathic medicine solves at least a significant proportion of the scientific puzzles solved by allopathic medicine—explaining their causation and providing a route to their treatment. But only extreme supporters of homeopathy claim that it can do this with respect to the majority of serious diseases. Gerald Doppelt (1978) makes an important distinction between *long-run relativism* and *short-run relativism*. According to Doppelt, Kuhn makes a strong case for the latter: in it not the case that every revolutionary scientific development constitutes a determinately progressive and rational advance. That, however, is consistent with a denial of long-run relativism, the latter being the view that scientific development as whole does not objective progress.

As we have seen Lakatos and others took Kuhn's epistemological relativism to have the implication that science is irrational. Is this correct? Lakatos and Popper clearly thought that for science to be rational, the principles upon which science makes its judgments must be ones that can be acknowledged to be rational or correct by anyone, irrespective of their background beliefs. But it is doubtful whether there are any such principles. Popper founds his philosophy on the fact that Hume showed that no inductive principle satisfies this requirement. From the perspective of a naturalistic epistemology, the requirement is too strong. Belief-forming mechanisms, if they are reliable and acquired in the right way, may generate knowledge without that fact being apparent to every reasonable subject, irrespective of their background beliefs. Naturalized epistemology endorses the idea that our mechanisms for generating knowledge will develop as science progresses. Kuhn's picture of the development of science is easily reconciled with such an epistemology, and as a consequence we do not have to regard the nuanced epistemological relativism of *The Structure of Scientific Revolutions* as irrationalist. Kuhn, however, did not avail himself of such an approach, although he did rightly remark that his view in effect simply states that rational people may disagree (1977a: 332). By the time naturalized epistemology had become widespread in the philosophical community, Kuhn had abandoned interest in paradigm relativism to concentrate his philosophical efforts on developing the idea of incommensurability.

3 Incommensurability

Kuhn, along with Paul Feyerabend, introduced the term 'incommensurability' (from mathematics) to name the phenomenon whereby two paradigms, or scientific claims produced within different paradigms, cannot be assessed or understood according to common criteria or concepts. The form of incommensurability we encountered in the preceding section is *methodological incommensurability*. Kuhn argued that revolutionary scientific changes also lead to *semantic incommensurability*, and spent much of his career developing this idea. Kuhn took a holistic view of the meanings of scientific terms—they acquire their meanings from the theory and, more generally, the paradigm in which they are used. This can be seen as a development of the double-language model of Carnap (1956) and Nagel (1961). In the double-language model, theoretical terms get their meanings from the theories in which they are embedded, whereas observational terms get their meaning by referring to observable features of the world. Thus the double-language model allows for *meaning variance*—change in the meaning of theoretical terms—as a result of theory change. But it still permits the comparison of theories, since the observational consequences of the theories can be compared; observational language does not change with theory-change and is constant across theories. Kuhn adds to this a semantic thesis of theory-dependence of observation, as a result of which the meanings of *both* theoretical *and* observational terms change with a scientific revolution. Scientific claims from different paradigms, whether nominally theoretical or observational, are semantically incommensurable.

Semantic incommensurability is a form of conceptual relativism. A theory may seem, syntactically, directly to contradict an earlier theory or to have incompatible observational consequences—the Newtonian assertion 'mass is conserved' appears to contradict the Einsteinian 'mass is not conserved'. But 'mass', Kuhn (1970: 101–2) tells us, means something different in Newton's physics from what it means in Einstein's physics. Nor, for the same reason, can we say that Einstein's theory is superior to Newton's on the grounds that the former has the latter as a special case for low velocities. Thus theories are semantically isolated from one another. Scientific assertions have their meanings relative to theoretical contexts in a highly constrained manner. Kuhn expresses this conceptual relativism in a manner that implies ontological relativism: 'the physical referents of these Einsteinian concepts [space, time, and mass] are by no means identical with those of the Newtonian concepts that bear the same name.' Dudley Shapere (1966) and Israel Scheffler (1967) argue that Kuhn's semantic incommensurability leads to an implausibly strong version of relativism. Incommensurability and its implications for relativism are explored in further detail in Chapter 26.

4 The transcendence of truth

As I have explained, Kuhn thinks that the processes of theory evaluation are paradigm-relative. It is important to note that the relativity of theory evaluation does not imply that what is being evaluated is paradigm-relative. We can think of there being two standards. One concerns, for example, the truth of a theory or its nearness to the truth. Whether or not a theory meets that standard, or to what degree, is typically not easily judged. This we may call the distal standard. At the same time there are other standards that are more easily judged: how well a theory explains the evidence, how effectively it solves certain puzzles and so forth (which is not to say that assessing such things is always straightforward). This is the proximal standard. For the realist we assess a theory against the second, proximal set of standards in order to find out whether it meets the first, distal set. That is to say, whether a theory explains the evidence well is a guide to its truth. It might be that the proximal standard is paradigm-relative, but that the distal standard is absolute and paradigm-independent. Indeed, a naturalized epistemology might well endorse precisely this picture: as our scientific knowledge advances, we can improve our cognitive practices; the ultimate target, truth, remains constant. So the relativity of theory evaluation is consistent with the objectivity of truth.

Kuhn however did not think that the aim of theory evaluation was to assess the truth of a theory. Kuhn took the aim of science to be the solving of scientific puzzles, and consequently this is what the assessment of a theory or puzzle-solution aims to adjudicate. As a result, Kuhn scarcely mentions truth at all in the first edition of *The Structure of Scientific Revolutions*. But in the second edition, in a lengthy postscript, Kuhn (1970: 206–7) takes a more engaged approach to philosophical issues surrounding truth:

> Often one 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."
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> 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 seems to me illusive in principle. Besides, as a historian I am impressed with the implausibility of the view. I do not doubt, for example, that Newton's mechanics improves on Aristotle's and that Einstein's improves on Newton's as instruments for puzzle-solving. But I can see in their succession no coherent direction of ontological development. On the contrary, in some important respects, though by not means in all, Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's.

Here Kuhn, rather than ignoring truth, actively rejects the notion of truth as usually construed. In a condensed form he employs an argument familiar from Kant and from the logical empiricists (among others) to the effect that truth is transcendent. As Paul Hoyningen-Huene (1993: 263–4) reconstructs Kuhn's argument, the idea is that we cannot possibly find out whether a theory is true, for that requires that we are able to compare the theory and reality, which in turn requires having an independent grasp on what reality is like. And that is precisely what we do not have—and if we did have it, we would not need the theory after all.

It is worth reflecting that this is analogous to Berkeleian concerns about perception as conceived by Locke and other indirect realists, as involving an intermediary (ideas, sense-data) that is the direct object of perception and which may represent the world, truly or otherwise. To know that a sense-datum represents the world would require the ability to compare the two. But that is impossible, given that all access to the world is mediated by sense-data.

Note that although the arguments are analogous and have very similar conclusions, they are not the same—one concerns the mediation of perception by sense-data, the other concerns the mediation of scientific knowledge of a largely imperceptible world by theory. Indeed the Kuhnian concern is on firmer ground insofar as the Berkleian argument depends on a contentious theory of perception, and would be clearly invalid if direct realism were true. On the other hand, it is undeniable that our (putative) knowledge of the existence and structure of atoms, of the history of the solar system, of the nature and function of DNA along with almost all other scientific claims, is mediated by theory.

Although the Kuhnian version has that advantage over the Berkleian version, I do not regard either as being a good argument. Let us grant that knowing that theory T is true is a matter of knowing that there is a match between T and the world. It does not follow from this that in order to know that T is true one must *first* know that there is a match between T and the world. Furthermore, knowing that there is a match between T and the world need not require having an independent grasp of each and seeing that they match (Bird 2000: 227). If one regards inference to the best explanation as a potential way of gaining scientific knowledge, one will infer that the explanation provided by one's best (potential) explanation of the evidence is the actual explanation. The hypothesis expounding that explanation is thus true. Since it is true, there is a match between it and the world. There may be other ways of articulating inference to the best explanation, but they will all share the feature that the match between theory and world is not known by independent knowledge of each, but is instead inferred. Other accounts of theoretical knowledge may also have that feature. In brief, the match between theory and world may be inferred *from* the theory's success rather than vice-versa or by being established independently.

Above I remarked that relativism about the process of theory choice, about the proximal standard, and about evaluation is consistent with absolutism about the truth of theories, the distal standard. However, given that Kuhn rejects the notion of truth, all that remains is the proximal standard. All we can say about a theory is how well is solves the puzzles of science. 'Solving a puzzle' in this sense is not an absolute, distal notion; it is not equivalent to 'getting the right answer'. Rather it means something like 'providing an answer to the research question that sufficiently resembles the paradigm answers to paradigm puzzles'.

In summary, Kuhn's own approach to truth is principally that it is irrelevant. We do not need to appeal to the distal standard of truth, since we can explain well we need to in the history of science by reference to the proximal standard of scientific puzzle solving. That summarises his view in 1962. By 1969 Kuhn was willing, albeit briefly, to add to this an argument for the transcendence of truth. This further emphasizes the irrelevance of truth, to the extent of questioning whether 'true' is even meaningful. Furthermore, Kuhn adds the historical argument that theories have been too variable over time for there to be any increasing truth content. Later he went so far as to assert (1992: 14), 'All past beliefs about nature have sooner or later turned out to be false.' Some philosophers have concurred with Kuhn in such views, which themselves go back to Poincaré (1943), most notably Larry Laudan, who likewise promotes a version of the pessimistic meta-induction and argues for the transcendence of truth. Others, however, have been more reluctant to abandon the notion of truth.

Kuhn himself leaves open, in the quotation given above, the possibility that some weaker notion of truth may be usable. It should be noted however that were Kuhn's argument sound, it would count against any notion of truth as objective, not just against the correspondence conception of truth. Later Kuhn (1993: 330–1) gestures towards a notion of truth that appears to be relative to a lexicon, in a manner that has both Wittgensteinian and Kantian overtones:

> Each lexicon makes possible a corresponding form of life within which the truth or falsity of propositions may be both claimed and rationally justified, but the justification of lexicons or of lexical change can only be pragmatic. With the Aristotelian lexicon in place it does make sense to speak of the truth of falsity of Aristotelian assertions in which terms like 'force' or 'void' play an essential role, but the truth values arrived at need have no bearing on the truth or falsity of apparently similar assertions made with the Newtonian lexicon.

As Sankey (2000: 72) points out, Kuhn is not saying that truth is fixed by a lexicon (a structure of conceptually related terms) or its associated theory; nor is he avowing a simple relativism whereby the truth of a given propositions varies from lexicon to lexicon. Rather, Kuhn is saying that truth is only possible relative to such a lexicon. Nor, on the other hand, is Kuhn making the trivial point that one can only express a proposition given suitable conceptual apparatus. For on a realist view, the truth or falsity of Aristotelian claims does bear on the truth and falsity of Newtonian claims (and vice versa), even if the Aristotelian claims cannot be framed in Newtonian terms (and vice versa). Thus the independence of lexicons seems not simply to be a logical matter, but a metaphysical one. Sankey interprets Kuhn's views here as a special of quasi-Kantian ontological relativism, as I explain in the next section.

5 World-change, idealism, and ontological relativism

Notoriously, Kuhn tells us that revolutions lead to changes in the world:

> In so far as their only recourse to that world is through what they see and do, we may want to say that after a revolution scientists are responding to a different world. (1970: 111)
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> The very ease and rapidity with which astronomers saw new things when looking at old objects with old instruments may make us wish to say that, after Copernicus, astronomers lived in a different world. (1970: 117)
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> At the very least, as a result of discovering oxygen, Lavoisier saw nature differently. And in the absence of some recourse to that hypothetical fixed nature that he "saw differently," the principle of economy will urge us to say that after discovering oxygen, Lavoisier worked in a different world. (1970: 118)

Some critics saw in this a strong claim to the effect that the world itself is created by our scientific beliefs. Indeed some would-be followers of Kuhn took his words to endorse a strong, ontological version of social constructivism. As ever, Kuhn's own view is rather more subtle. The context of all these quotations shows that Kuhn's claim is that the scientists' view of the world changes in a sufficiently dramatic way that we are inclined to describe such shifts as if they were differences in the world. And indeed, that kind of metaphor is common in everyday speech. So one reading of these comments is one that takes such talk to be a metaphorical reference to significant changes in the cognitive psychology of scientists. Because Kuhn held a version of the theory-ladenness of observation that encompassed perceptual experience, such psychological changes include, in Kuhn's view, a scientist's perception of the world. But he nonetheless continued to maintain, against the strong constructivist view, that there is an underlying physical world that remains unchanged. For example, in the postscript to the second edition of *The Structure of Scientific Revolutions* he writes:

> What follows seems obvious to me now, but the constant recourse in my original text to phrases like "the world changes" suggests that it has not always been so. If two people stand at the same place and gaze in the same direction, we must, under pain of solipsism, conclude that they receive closely similar stimuli. (If both could put their eyes at the same place, the stimuli would be identical.) But people do not see stimuli; our knowledge of them is highly theoretical and abstract. Instead they have sensations, and we are under no compulsion to suppose that the sensations of our two viewers are the same. (1970: 192)

There is an important idealist reading of Kuhn, that understands the forgoing textual evidence in a Kantian manner. Paul Hoyningen-Huene (1993) distinguishes between the world-in-itself which remains unchanged through scientific revolutions and the phenomenal world, which does change as a consequence. Thus Kuhn (1979) came to see himself as a Kantian with dynamic categories. A consequence of this view in that the scientists' phenomenal world is paradigm-relative. And since, in the Kantian vein, it is only the phenomenal world to which we have access, this does constitute an important, idealist species of ontological relativism.

A development of this ontological relativism is also to be found in Kuhn's final version of the incommensurability thesis. Instances of incommensurability arise when our classificatory schemes ('taxonomies') change. Thus the extensions (actual and potential) of the classificatory terms shift so that the new and old classifications are out of alignment, and the new term or new use fails to match the old term and its use. Because of local semantic holism, the whole of a taxonomy changes when one part does, so we cannot expect to capture the changed classifications by definition in terms of unchanged classifications. Nor can we simply adopt both old and new taxonomies in one all-encompassing taxonomy, since Kuhn holds to a no-overlap thesis, according to which the two classifications of a taxonomy are either disjoint or one is a subclassification of the other (i.e. a taxonomy has a tree structure). Ian Hacking (1979, 1993) develops this idea as a species of nominalism about natural kinds. According to the nominalist only particular things exist, not their properties or the kinds to which they belong. A nominalist may thus see our actual classifications as products of the mind; Hacking's Kuhn adds to this the claim that the human-created categories will change as a result of scientific revolutions. So world-change is not a change of the things in the world, but of their natural kinds. Consequently we have an idealism or constructivism about kinds that is also a relativism, since their constructed existence is paradigm-relative. However, as Sankey (2000: 68–9) argues, Kuhn's position is importantly different, in that he does not take the world of particulars as unchanging across revolutions, since the domain of particulars we take the world to contain depends on our descriptive and classificatory schemes. Thus if our classification changes, the domain of particulars changes also. The idealism and relativism that we ascribe to kinds as a consequence of Hacking's account, must therefore be extended to particulars also. A thorough-going ontological relativism ensues, which, Sankey remarks, is of a piece with Kuhn's later view of truth as described above. Because the phenomenal world can change in radical ways, with respect to both kinds an particulars, a relativism arises not where propositions change truth-values according to changes in paradigm, but where certain possibilities come into existence (cf. Kuhn 1993: 331). Consequently, a change in paradigm does not make certain propositions true but permits them to be true (or false).

6 Relativism in the sociology of science and the history of science

Kuhn's work has been highly influential in science studies: the sociology of scientific knowledge and the history of science in particular. The relativism that prevails in such subjects is apparent from the terminology of 'the sociology of scientific knowledge' (cf. David Bloor's (1991) *Knowledge and Social Imagery*), where 'knowledge' is not taken in a realist or objective manner, implying that what is known is objectively true. Rather 'knowledge' is that which achieves a certain social status—accepted as established by the relevant scientific elites, for example. A similar use is made of the term 'truth' (cf. Steven Shapin's (1995) *A Social History of Truth*).

Such uses may, however, be regarded as expressions of a methodological relativism that is central to much history of science. Shapin explains that he does not reject the more restrictive notion of truth, in which what it true and what locally counts as true may be different things. Rather, the social historian of science finds the broader use methodologically more suitable. Bruno Latour explains methodological relativism thus:

> Since the settlement of a controversy is the cause of Nature's representation, not its consequence, we can never use this consequence, Nature, to explain how or why a controversy has been settled. (Latour 1987: 258)

Latour's point is that when it comes to historical explanation it is irrelevant and inappropriate to explain why scientist A, who asserted that *p*, succeeded in a dispute with scientist B, who denied that *p*, by referring to the fact that *p* is in truth the case. What explains why their colleagues endorsed A's theory must refer to facts about those scientists and what they knew or believed. But they do not know that *p* in advance of the resolution of this dispute. In general, facts about the way the world is in respects that are not apparent to agents, and likewise which scientific claims are *true*, are not relevant to historical explanations, and consequently whether scientists *knew* such-and-such is irrelevant also. Thus historians of science adopt a methodological relativism according to which knowledge and truth are not explanatory categories: what is explanatorily relevant is what is accepted in the relevant community, and so some historians, such as Shapin, prefer to use the terms 'knowledge' and 'truth' to refer to such beliefs. The historian thereby adopts the perspective of the target community for her explanatory resources. She thereby avoids the sin of whiggism—explaining past beliefs in terms of facts known to us but not to the historical subjects. (Whether anti-whiggism and a thorough-going methodological relativism can be consistently maintained my be open to question. See Jardine 2003.)

This methodological relativism is encapsulated in the 'symmetry principle' of the sociology of science (Barnes and Bloor 1982), according to which true beliefs and false beliefs, rational beliefs and irrational beliefs, should all be explained in similar ways. Although it has been pointed out that this fails as a fully general principle (Newton-Smith 1981: 252), as a methodological heuristic it serves to remind us that we should look for the explanations of both belief in Priestley's phlogiston theory and belief in Lavoisier's oxygen theory in the same sorts of place, e.g. in the available evidence. Likewise, although past beliefs may seem irrational, as Aristotle's physics initially seemed to Kuhn, we should attempt to understand them from the scientific and cultural perspective (i.e. the paradigm science) of the subject, which may permit us to see the beliefs as entirely reasonable.

We saw that the outcome of a scientific revolution cannot be determined by the application of any rule of scientific rationality—there is room for rational disagreement. This might be seen as a variant of the underdetermination of theory by data. According to the latter thesis, the choice between competing theories cannot be decided by reference to the evidence alone—the evidence will always support more than one theory equally well. Kuhn's position differs in that he regards the nature and import of the evidence as being a component of the dispute between the theories. It is common among sociologists and historians of science to hold that because the evidence cannot decide the outcome of such scientific debates, we must appeal to extra-scientific factors in order to explain the outcomes that do occur (Bloor 1982; cf. Longino 2002). Kuhn's (1970: 152–3) remarks on the significance of nationality, reputation, cultural background and the like, to which I referred earlier, were taken to support such a view. If scientific disputes are settled not by the evidence but by social forces, then it is natural to be sceptical of the claims of even today's science to deliver any kind of objective truth or knowledge. Consequently, one will conclude that the methodologically broader uses of 'truth' and 'knowledge' are the only reasonable of the terms. Or indeed those terms may be seen primarily as rhetorical devices (a view which goes back at least as far as Nietzsche 1968; see O'Neill 1998 for more recent discussion). A methodological relativism may thus be followed by a relativism about truth also. In fact Kuhn (1992) rejected what he took to be the excessive weight placed upon external explanations of scientific development: the explanatory factors in scientific change will almost always concern evidence, puzzle-solving, scientific values, and so forth. The absence of an algorithm to fix a unique rational preference for an individual scientist does not imply that extra-scientific factors need to be invoked in the causal explanation of a preference.

A different use of Kuhn's work has been made in order to support a sociologically inspired relativism. In his (1977b), Kuhn illustrates the functioning of paradigms by considering the case of a child learning the concepts *goose*, *duck*, and *swan*. The child is presented with instances of each during a walk in a park, while his parent names the kind to which each belongs. Thanks to this training with exemplars, the child acquires the concepts by acquiring a suitable sense of similarity, a learned similarity relation. Each instance, with the correct usage sanctioned by the community (in the person of the parent), forms and reinforces the learned similarity relation. From this, Barry Barnes (1982: 28–30), draws the moral that 'past experience and the past usage of a concept can never suffice to determine future usage'. What makes a usage correct is (possibly negotiated) agreement with the community, not anything in the 'nature of things, or the nature of language, or the nature of past usage.' Consequently 'nothing determines the truth or falsity of verbal statements . . . This leads directly to a radical view of the conventional character of knowledge.'

Barnes thus draws a version of relativism about truth from Kuhn, while acknowledging that Kuhn himself might not endorse such conclusions. While Kuhn did take the sociological aspect of his example to be significant, the key moral for him concerns the tacit nature of knowledge: learned similarity relations cannot be articulated in formal sets of criteria. But that does not undermine their objectivity—indeed Kuhn (1977b: 310) says that this kind of learning can be modelled on a computer. Kuhn does acknowledge that there is open-endedness to our concepts, as would be shown were we to try to fix their boundaries. But it does not follow from this that no application of a concept is fixed by the world and past usage, nor that the correctness of every application is subject to community agreement. Although Barnes states his conclusions in general terms, his illustrative examples concern borderline cases rather than the application of the concept to 'easy', central cases. To support a *finitism* that regards the truth of every proposition as dependent on community assent, one must go well beyond the philosophical resources supplied by Kuhn, to those of Wittgenstein (1953) and, more obviously, Kripke (1982) (see Kusch 2002). The relation of relativism to the sociology of scientific knowledge is discussed in detail in Chapter 25.

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