Overview

If observational evidence underdetermines theories, we need at least an explanation of what does determine the succession of theories which characterizes science's history. Even more, for philosophy's purposes, we need a justification for the claim that these observationally unsupported theories are epistemically rational and reasonable ones to adopt. Clearly, empiricism cannot by itself do this, as its resources in justification are limited to observation.

Thomas Kuhn, an important historian of science, was among the first to explore the history of science for these non-observational factors that explain theory-choice, and to consider how they might justify it as well. His book, *The Structure of Scientific Revolutions*, sought to explore the character of scientific change – how theories succeed one another – with a view to considering what explains and what justifies the replacement of one theory by another. The logical empiricists hold that theories succeed one another by reduction, which preserves what is correct in an earlier theory, and so illuminates the history of science as progress. Kuhn's research challenges this idea.

By introducing considerations from psychology and sociology as well as history, Kuhn reshaped the landscape in the philosophy of science and made it take seriously the idea that science is not a disinterested pursuit of the truth, successively cumulating in the direction of greater approximation to the truth, as guided by unambiguous observational test.

Kuhn's shocking conclusion suggests that science is as creative an undertaking as painting or music, and not to be viewed as more objectively progressive, correct or approximating to some truth about the world than these other human activities. The history of science is the history of change, but not progress; in a sense that Kuhn defends, we are no nearer the truth about the nature of things nowadays than we were in Aristotle's time. These shocking conclusions represent a great challenge to contemporary philosophy of science.

Much of the philosophical underpinnings for views like Kuhn's can be found in the work of an equally influential philosopher, W. V. O. Quine, who attacked logical empiricism "from within", so to speak. A student of the logical empiricists, Quine was among the first to see that the epistemology underlying their philosophy of science could not satisfy its own requirements for objective knowledge, and was based on a series of unsupportable distinctions. By casting doubt on the foundations of a tradition in philosophy that went back to Locke, Berkeley and Hume, Quine made it impossible for philosophers of science to ignore the controversial claims of Kuhn and those sociologists, psychologists and historians ready to employ his insights to uncover the status of science as a "sacred cow".

1 A place for history?

In the last chapter we traced the development of philosophy's traditional analysis of scientific knowledge as the outcome of attempts to explain our observations which are themselves "controlled" by our observations. Empiricism, the ruling "ideology" of science, assures us that what makes scientific explanations credible, and what insures the self-correction of science, as well as its ever-increasing predictive powers, is the role that observation, experiment and test play in the certification of scientific theory.

But we have also seen that actually making this role precise is not something the philosophy of science has been able to do. Not only can philosophy of science not provide an uncontroversial empiricist justification for our knowledge of the existence of theoretical entities, it cannot even assure that the terms that name these entities are meaningful. Even worse, the simplest evidential relation between a piece of data and a hypothesis which that data might test seems equally difficult to express with the sort of precision that both science and the philosophy of science seem to require. One might hold that this is not a problem for scientists, just for philosophers of science. After all, we know that theoretical terms are indispensable, because theoretical entities exist and we need to invoke them in explanations and predictions. And we know that scientific hypotheses' abilities to withstand empirical test is what makes them knowledge. Formalizing these facts may be an interesting exercise for philosophy but it need not detain the working scientist.

This would be a superficial view of the matter. To begin with, it would be a double standard not to demand the same level of detail and precision in our understanding of science as science demands of itself in its understanding of the world. Scientific empiricism bids us test our ideas against experience; we cannot do this if these ideas are vague and imprecise. The same must go for our ideas about the nature of science itself. Second, if we cannot provide a precise and detailed account of such obvious and straightforward matters as the existence of theoretical entities and the nature of scientific testing, then this is a symptom that there may be something profoundly wrong in our understanding of science. This will be of particular importance to the extent that less well-developed disciplines look to the philosophy of science for guidance, if not recipes on how to be scientific.

The dissatisfaction with philosophy of science's answers to fundamental questions about theories and their testing of course led philosophers of science to begin rethinking the most fundamental presuppositions of the theory of science embodied in logical empiricism. The re-examination began with the uncontroversial claim that the philosophy of science should provide a picture of the nature of science that mirrors what we know about its history and its actual character. This may sound uncontroversial until it is recalled how much traditional philosophy of science relied on considerations from formal logic coupled with a narrow range of examples from physics.

Among the earliest, and certainly the most influential document in the reconsideration of the nature of science from the perspective of its history, was Thomas Kuhn's *The Structure of Scientific Revolutions*. This slim work set out to bring the philosophy of science face to face with important episodes from its history. But it ended up completely undermining philosophy's confidence that it understood anything about science. And it became the single most heavily cited work in the second half of the twentieth century's absorption with science. How could this have happened?

The study of the history of science since well before Newton suggested to Kuhn that claims about the world we might now view as pre- or unscientific myths were embraced by learned people whose aim was to understand the world for much the same sort of reasons that we embrace contemporary physical theory. If it is the sort of reasons that support a belief which makes it scientific, then these myths were science too. Or alternatively, our latest scientific beliefs are myths, like the pre- and unscientific ones they replaced. Kuhn held that the first of these alternatives was to be preferred. Adopting this perspective makes the history of long-past science an important source of data in any attempt to uncover the methods that make science objective knowledge. The second alternative, that contemporary science is just the latest successor in a sequence of mythic "world-views", no more "objectively true" than its predecessors, seemed to most philosophers of science (if not always to Kuhn), preposterous. The trouble is that Kuhn's account of the nature of science was widely treated outside philosophy of science as having supported this second alternative at least as much as the first one.

Kuhn's ostensible topic was scientific change, how the broadest theories replace one another during periods of scientific revolution. Among the most important of these was the shift from Aristotelian physics to Newtonian mechanics, from phlogiston chemistry to Lavoisier's theories of reduction and oxidation, from non-evolutionary biology to Darwinism, and from Newtonian mechanics to relativistic and quantum mechanics. Periods of revolutionary change in science alternate with periods of what Kuhn called "**normal science**", during which the direction, the methods, the instruments and the problems that scientists face are all fixed by the established theory. But Kuhn considered that the term "theory" did not aptly describe the intellectual core of a program of "normal science". Instead he coined the term "**paradigm**", a word which has gone into common usage. Paradigms are more than just equations, laws, statements encapsulated in the chapters of a textbook. The paradigm of Newtonian mechanics was not just Newton's laws of motion, it was also the model or picture of the universe as a deterministic clockwork in which the fundamental properties of things were their position and momentum from which all the rest of their behavior could eventually be derived when Newtonian science was completed. The Newtonian paradigm also included the standard set of apparatus or lab equipment whose behavior was explained, predicted and certified by Newton's laws, and with it a certain strategy of problem-solving. The Newtonian paradigm includes a methodology, a philosophy of science, indeed an entire metaphysics. In his later writing Kuhn placed more emphasis on the role of the **exemplar** – the apparatus, the practice, the impedimenta – of the paradigm than on any verbal expression of its content. The exemplar more than anything defines the paradigm.

Paradigms drive normal science, and normal science is in a crucial way quite different from the account of it advanced by empiricist philosophers of science. Instead of following where data, observation and experiment lead, normal science dictates the direction of scientific progress by determining what counts as an experiment that provides data we should treat as relevant, and when observations need to be corrected to count as data. During normal science, research focuses on pushing back the frontiers of knowledge by applying the paradigm to the explanation and prediction of data. What it cannot explain is outside of its intended domain, and within its domain what it cannot predict is either plain old experimental error or the clumsy misapplication of the paradigm's rules by a scientist who has not fully understood the paradigm.

Under the auspices of normal science, three sorts of empirical inquiries flourish: those which involve redetermining of previously established observational claims to greater degrees of precision, certifying the claims of the current paradigm against its predecessor; the establishment of facts without significance or importance for themselves but which vindicate the paradigm; and experiments undertaken to solve problems to which the paradigm draws our attention. Failure to accomplish any of these three aims reflects on the scientist attempting them, not the paradigm employed. None of these sorts of inquiry is to be understood on the empiricist model of experience testing theory.

The grandest example of the success of normal science in giving priority of belief to theory over data (and thus undermining empiricism) is found in the story of Newtonian mechanics and the planets of Neptune and Pluto. One of the great successes of Newtonian mechanics in the 1700s was predicting the appearance and reappearance of Halley's comet by enabling astronomers to calculate its orbit. In the nineteenth century, apparent improvements in telescopes enabled astronomers to collect data on the path of Uranus which suggested a path different from that

Newtonian theory predicted. As we have seen in Chapter 5, this apparently falsifying observation discredits the "package" of Newton's laws, along with a large number of auxiliary hypotheses about how telescopes work and what corrections have to be made to derive data from observations using them, as well as assumptions about the number and mass of the known planets whose forces act upon Uranus. The centrality of the Newtonian paradigm to normal science in physics did not in fact leave matters underdetermined in the way Chapter 5 suggests. The ruling paradigm dictated that the data on Uranus be treated as a "puzzle", that is, a problem with a "correct" answer to be discovered by the ingenuity of physicists and astronomers applying the paradigm. A physicist's failure to solve the paradigm simply discredited the physicist, not the physics! There could be no question that the theory was wrong; it had to be the instruments, the astronomers, or the assumptions about the number and mass of the planets. And indeed, this was how matters turned out. Accepting the force of the Newtonian paradigm, and the reliability of the instruments which the Newtonian paradigm certified, left only the option of postulating one or more additional planets, as yet undetected (because too small or too distant or both), whose Newtonian gravitational forces would cause Uranus to move in the way the new data suggested. Training their telescopes in the direction from which such forces must be exerted, astronomers eventually discovered first Neptune and then Pluto, thus solving the puzzle set by the Newtonian paradigm. Whereas the empiricist would describe the outcome as an important empirical confirmation of Newton's theory, followers of Kuhn would insist that the paradigm was never in doubt and so neither needed nor secured additional empirical support from the solution to the puzzle.

Normal science is characterized by textbooks, which despite their different authors convey largely the same material, with the same demonstrations, experiments and similar lab manuals. Normal science's textbooks usually contain the same sorts of problems at the back of each chapter. Solving these puzzles in effect teaches scientists how to treat their subsequent research agendas as sets of puzzles. Naturally, some disciplines are, as Kuhn put it, in "pre-paradigm" states, as evinced for example by the lack of textbook uniformity. These disciplines are ones, like many of the social sciences (but not economics), where the lack of commonality among the textbooks reveals the absence of consensus on a paradigm. How the competition in pre-paradigm science gives way to a single winner, which then determines the development of normal science, Kuhn does not tell us. But he does insist paradigms do not triumph by anything like what the experimental method of empiricism suggests. And the reason Kuhn advances is an epistemologically radical claim about the nature of observation in science.

Recall the distinction between observational terms and theoretical terms so important to the project of empiricism. Observational terms are used to describe the data which epistemically control theory, according to the empiricist. The empiricist's problem is that observation seems inadequate to justify the explanatory theories about unobservable events, objects and processes with which science explains the observable regularities we experience in the lab and the world. This problem for empiricism is not a problem for Kuhn, because he denies that there is a vocabulary that describes observations and that is neutral between competing theories. According to Kuhn, paradigms extend their influence not just to theory, philosophy, methodology and instrumentation, but to the lab-bench and the field notebook, dictating observations, not passively receiving them.

Kuhn cited evidence from psychological experiments about optical illusions, gestalt-switches, expectation-effects and the unnoticed theoretical commitments of many apparently observational words we incautiously suppose to be untainted by presuppositions about the world. Consider some examples. Kuhn's example was a red jack of spades and a black jack of hearts which most people don't notice are red and black as they are accustomed to black spades and red hearts. Since Kuhn first made the point, other examples have become common knowledge. In the Mueller-Lyer illusion, two lines of equal length, one with an arrow at each end pointing out, and the other with arrows pointing in, are viewed by western eyes as unequal; but the illusion does not fool people from "non-carpentered societies" without experience of straight lines. The Necker cube, a simple two-dimensional rendering of a transparent cube, is not so identified by those without experience of perspective, and the front–back switch or reversal which we can effect in our perception shows that the act of seeing is not a cognitively innocent one. When Galileo first described the moon as "cratered", his observations already presupposed a minimal theoretical explanation of how the lunar landscape was created – by impacts from other bodies.

Kuhn was not alone in coming to this conclusion. Several opponents of empiricism came in the 1950s to hold this view about observation. They held that the terms in which we describe observations, whether given by ordinary language or scientific neologisms, presuppose divisions or categorizations of the world of experience in ways that reflect prior "theories": the categories we employ to classify things, even categories as apparently theory-free as color, shape, texture, sound, taste, not to mention size, hardness, warmth/coldness, conductivity, transparency, etc., are shot through with interpretation. Instead of seeing a glass of milk, we see "it" as a glass of milk, where the "it" is not something we can describe separately in a theory-neutral vocabulary. Even the words "white", "liquid", "glass", "wet", "cold", or however we seek to describe our sensory data, are as much theory-bound as "magnetic" or "electric" or "radioactive".

Since Kuhn first wrote, this claim that the theoretical/observational distinction is at least unclear and perhaps baseless, has become a lynchpin for non-empiricist philosophy of science. Its impact upon the debate about the nature, extent and justification of scientific knowledge cannot be understated. In particular it makes much more difficult to understand the nature of scientific testing – the most distinctive of science's differences from everything else. Kuhn recognized this consequence, and his way of dealing with it is what made *The Structure of Scientific Revolutions* so influential a work.

A revolution occurs when one paradigm replaces another. As normal science progresses, its puzzles succumb to the application or, in Kuhn's words, "the articulation" of the paradigm. A small number of puzzles continue to be recalcitrant: unexpected phenomena that the paradigm cannot explain, phenomena the paradigm leads us to expect but that don't turn up, discrepancies in the data beyond the margins of error, or major incompatibilities with other paradigms. In each case, there is within normal science a rational explanation for these anomalies; and often enough further work turns an anomaly into a solved puzzle. Revolutions occur when one of these anomalies resists solution long enough, while other anomalies succumb, to produce a crisis. As more and more scientists attach more importance to the problem, the entire discipline's research program begins to be focused around the unsolved anomaly. Initially small numbers of especially younger scientists without heavy investment in the ruling paradigm cast about for a radical solution to the problem the anomaly poses. This will happen usually when a paradigm has become so successful that few interesting puzzles are left to solve. More and more of the younger scientists, especially, with ambitions and names to make, decide to attach more importance to the remaining unsolved puzzle. Sometimes, a scientist will decide that what could reasonably be treated as experimental error is something entirely new and potentially paradigm-wrecking. If the ultimate result is a new paradigm, what the scientist has done is retrospectively labeled a new discovery. When Roentgen first produced X-rays, he treated the result as contamination of photographic plates. The same plates became evidence of a significant phenomenon once paradigm shift had allowed for it. If the ultimate result is not incorporated by a paradigm shift, it gets treated as error – poly-water for example – or worse, fraud – cold-fusion.

In developing a new paradigm, revolutionaries are not behaving in the most demonstrably rational way; nor are their usually elderly establishment opponents who defend the ruling paradigm against their approach, acting irrationally. During these periods of crisis when debate in a discipline begins to focus inordinately on the anomaly, neither side can be said to be acting irrationally. Defenders of the old paradigm have the weight of all its scientific successes to support their commitment. Exponents of the new one have only at most its solution to the anomaly recalcitrant to previous approaches.

Note that during these periods of competition between old and new paradigms, nothing between them can be settled by observation or experiment. This is for several reasons. To begin with, often there is little or no difference between the competing paradigms when it comes to predictive accuracy. Ptolemaic geocentric astronomy with its epicycles was predictively as powerful, and no more mathematically intractable, than its Copernican heliocentric rival. Moreover, "observational" data are already theoretically charged. It does not constitute an unbiased court of last resort. For Kuhn there is in the end no evidentiary court that will decide between competing paradigms which is more rational to embrace, which is closer to the truth, which constitutes scientific progress. This is where the radical impact of Kuhn's doctrine becomes clear.

A persistently unsolved and paradigmatically important anomaly will result in a scientific revolution only when another paradigm appears that can at least absorb the anomaly as a mere puzzle. In the absence of an alternative paradigm, a scientific discipline will continue to embrace its received one. But the grip of the paradigm on scientists is weakened; some among them begin to cast around for new mechanisms, new rules of research, new equipment, and new theories to explain the relevance of the novelties to the discipline. Usually in this "crisis-situation", normal science triumphs; the anomaly turns out to be a puzzle after all, or else it just gets set aside as a problem for the long-term future, when we have more time, money and better research apparatus to throw at it. Revolutions occur when a new paradigm emerges. A new paradigm disagrees radically with its predecessor. Sometimes new paradigms are advanced by scientists who do not realize their incompatibility with ruling ones. For instance, Maxwell supposed that his electromagnetic theory was compatible with the absolute space of Newtonian mechanics, when in fact Einstein showed that electrodynamics requires the relativity of spatiotemporal relations. But the new paradigm must be radically different from its predecessor just insofar as it can treat as a mere puzzle what the previous one found an increasingly embarrassing recalcitrant anomaly. Paradigms are so all-encompassing, and the difference between paradigms is so radical, that Kuhn writes that scientists embracing differing paradigms find themselves literally in different worlds – the Aristotelian world versus the Newtonian one, the Newtonian world versus the quantum-realm. Paradigms are, in Kuhn's words, "incommensurable" with one another. Kuhn took the word from geometry, where it refers to the fact that, for instance, the radius of a circle is not a "rational" fraction of its circumference, but is related to it by the irrational number π. When we calculate the value of π the result is never complete but always leaves a "remainder". Similarly, Kuhn held that paradigms are incommensurable: when one is invoked to explain or explain away another, it always leaves a remainder. But mathematical **incommensurability** is a metaphor. What is this remainder?

According to Kuhn, though a new paradigm may solve the anomaly of its predecessor, it may leave unexplained phenomena that its predecessor successfully dealt with or did not need to deal with. There is a trade-off in giving up the old paradigms for the new, an explanatory loss is incurred at the expense of the gain. For example, Newtonian mechanics cannot explain the mysterious "action at a distance" it required – the fact that gravity exerted its effects instantaneously over infinite distances; this disturbing commitment is something the Aristotelian physics did not have to explain. In effect, "action at a distance" – how gravity is possible – became the anomaly that in part and after two hundred and fifty years or so eventually undid Newtonian mechanics. But explanatory loss is not all there is to incommensurability. For even with some explanatory loss, there might yet be net gain in explanatory scope of the new paradigm. Kuhn suggests that incommensurability is something much stronger than this. He seems to argue that paradigms are incommensurable in the sense of not being translatable one into the other, as poems in one language are untranslatable into another. And this sort of radical incommensurability which makes explanatory loss immeasurable underwrites the further claim that paradigms do not improve on one another, and that therefore science does not cumulate in the direction of successive approximation to the truth. Thus the history of science is like the history of art, literature, religion, politics or culture, a story of changes, but not over the long haul a story of "progress".

Kuhn challenges us to translate seventeenth-century phlogiston chemistry into Lavoisier's theories of oxidation and reduction. It cannot be done, without remainder, without leaving some part of the older theory out, and not necessarily the part of phlogiston theory that was wrong. Perhaps you are inclined to say that phlogiston chemistry was all wrong, and needed to be replaced by a new paradigm. This is the sort of ahistorical approach to the nature of science which Kuhn condemned so strongly. After all, phlogiston chemistry was the best science of its day, it had a long record of success in solving puzzles, organizing instrumentation and securing experimental support. And in the period before the heyday of phlogiston many scientists bent their genius towards alchemy. Isaac Newton was so devoted to the search for how to transmute lead into gold

that he may have died of lead poisoning as a result of his many experiments. Are we to say that his mechanics was the greatest scientific achievement of a transcendent genius in physics while his alchemy was the pseudo-scientific mischief of a crackpot? Either we must condemn a century of scientific work as irrational superstition or design a philosophy of science that accepts phlogiston chemistry as science with a capital S. If phlogiston theory is good science, and cannot be incorporated into its successor, it is hard to see how the history of science can be a history of cumulative progress. It seems more a matter of replacement than reduction.

Reduction, recall, is the empiricist's analysis of the interrelation of theories to one another, both synchronically, in the way that chemistry is reducible to physics, and diachronically, in the way that Newtonian seventeenth-century discoveries in mechanics are reducible to the twentieth century's special theory of relativity. But does this reduction really obtain in the way the empiricist supposes. Kuhn explicitly denies that it does. And the reason is incommensurability. Reduction of the laws of one theory to the laws of a more basic theory require that the terms of the two theories share the same meaning. Thus, the notions of space, time and mass should be the same in Newton's theory and in Einstein's special theory of relativity if the latter is just the more general case and the former is the special case, as reduction requires. The derivation of the laws of Newtonian mechanics from those of the special theory of relativity looks simple. All one requires is that "c", the speed of light, travels (like gravity) at infinite speed. The reason one requires this false but simplifying assumption to go from Einstein to Newton is that the special theory of relativity tells us that the mass of an object varies as the ratio of its velocity to that of the speed of light with respect to an observer's frame of reference; Newton's theory tells us, however, that mass is conserved, and independent of relative or absolute velocity whether in proportion to the speed of light or not.

Though the two theories share the same word with the same symbol, **m**, do they share the same concept? Emphatically not. In Newtonian mechanics mass is an absolute, intrinsic, "monadic" property of matter, which can neither be created nor destroyed; it is not a relational property that chunks of matter share with other things, like "is bigger than". In Einstein's theory, mass is a complex "disguised" relation between the magnitude of the speed of light, a chunk of matter and a location or "reference frame" from which the velocity of the chunk is measured; it can be converted to energy (recall $\mathbf{e} = \mathbf{mc}^2$). The change in the meaning of the word "mass" between these two theories reflects a complete transformation in world-view, a classical "paradigm shift". Once we as historians and philosophers of science see the difference between the meaning of crucial terms in the two theories, and discover that there is no common vocabulary – either observational or theoretical – which they share, the incommensurability between them becomes clearer. But, the physicist is inclined to say, "Look here, the way we teach the special theory of relativity in the textbooks is by first teaching Newton's theory and then showing it's a special case via the Lorenz transformations. It is after all a case of reduction. Einstein was standing on the shoulders of Newton, and special relativity reflects the cumulative progress of science from the special case to the more general one."

To this Kuhn has two replies. First, what is reduced is not Newton's theory, but what we, in the thrall of the post-Newtonian, Einsteinian paradigm imagine is Newton's theory. To prove otherwise requires a translation which would inevitably attribute incompatible properties to mass. Second, it is essential to the success of normal science that once it is up and running, it rewrites the history of previous science to make it appear just another step in the long-term continuous progress of science to cumulative knowledge of everything. The success of normal science requires the disciplining of scientists not to continually challenge the paradigm, but to articulate it in the solution of puzzles. Science would not show the pattern of cumulation which normal science exhibits without this discipline. One way to enforce the discipline of normal science is to rewrite their textbooks to make it appear as much as possible that what went before today's paradigm is part of an inevitable history of progress that leads up to it. Whence the invisibility of previous paradigms, and the empiricist's blindness to what the history of science really teaches. For the empiricist's understanding of science comes from its contemporary textbooks, and their "potted" history.

According to Kuhn we must take seriously the notion that scientific revolutions really are changes of world-view. The crucial shift from Aristotle to Newton was not the discovery of "gravity". It was in part the apparently slight change from viewing the distinction between rest and motion as the difference between zero and non-zero velocity to viewing it as the difference between zero and non-zero acceleration. The Aristotelian sees a body moving at constant velocity as under the influence of a force, "impetus" they called it. The Newtonian sees the body as being at rest, under the influence of no (net) forces. The Aristotelian sees the swinging pendulum bob as struggling against constraining forces. The Newtonian sees the pendulum as in equilibrium, at rest. There is no way to express the notion of "impetus" in Newton's theory, just as there is no way to express Einsteinian mass in Newton's theory. More broadly, Aristotelian science views the universe as one in which things have purposes, functions, roles to play; Newtonian mechanics bans all such "teleological".

whose position and momentum at any time together with the laws of nature determine their position and momentum at all other times.

Because a new paradigm is literally a change in world-view, and at least figuratively a change in the world in which the scientist lives, it is often too great a shift for well-established scientists. These scientists, wedded to the old paradigm, will not just resist the shift to the new one, they will be unable to make the shift; what is more, their refusal will be rationally defensible. Or at any rate, arguments against their view will be question-begging because they will presume a new paradigm they do not accept. To some extent we have already recognized the difficulty of falsifying a theory, owing to the underdetermination problem discussed in Chapter 5. Because paradigms encompass much more than theories, it is relatively easy to accommodate what some might call falsifying experience when adjustments can be made not just in auxiliary hypotheses but across a vast range of the intellectual commitments that constitute a paradigm. What is more, there is, recall, no neutral ground on which competing paradigms can be compared. Even if underdetermination of theory by evidence were not a problem, the observational findings on which empiricists admit differing theories may agree, are missing. When allegiance is transferred from one paradigm to another, the process is more like a religious conversion than a rational belief shift supported by relevant evidence. Old paradigms fade away as their exponents die off, leaving the proponents of the new paradigm in command of the field.

Progress is to be found in science, according to Kuhn, but like progress in evolution, it is always a matter of increasingly local adaptation. The Darwinian theory of natural selection tells us that over generations the random variations in traits are continuously filtered by the environment so as to produce an increasing spread of increasingly adaptative variations across a species. But environments change, and one environment's adaptation – say, white coats in the arctic – is another environment's maladaptation – white coats in the temperate forest. So it is with science. During periods of normal science, there is progress as more and more puzzles succumb to solution. But revolutionary periods in science are like changes in the environment, which completely restructure the adaptive problems a paradigm must solve. In this respect, science shows the same sort of progress as other intellectual disciplines show. And this is not surprising, for among the morals many draw from *The Structure of Scientific Revolutions* has been the conclusion that science is pretty much like other disciplines, and can make no claims to epistemic superiority. Rather, we should view the succession of paradigms in the way we view changes in fashion in literature, music, art and culture broadly. We should view competing paradigms the way we view alternative normative these units of culture, progress in approximating to the truth is rarely at issue. So too for science: in one of the last pages of his book, Kuhn writes, "We may, to be more precise, have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth" (*The Structure of Scientific Revolutions*, 1st edition, ch. 13, p. 170).

2 No place for first philosophy?

The Structure of Scientific Revolutions was published in 1962. The impact of its doctrines within and beyond the philosophy of science is difficult to overstate. Kuhn's doctrine became the lever with which historians, psychologists, sociologists, dissenting philosophers, scientists, politicians, humanists of every strip, sought to undermine the claims of science to objective knowledge, its claims to greater credence than alternative claims about the world. Meanwhile, within philosophy of science, developments that began earlier in the 1950s were reinforcing Kuhn's impact. These developments owe a great deal to the work of a philosopher, W. V. O. Quine, whose thought provided some of the philosophical foundations often held to support Kuhn's historical conclusions.

The traditional objectives of the philosophy of science were to justify science's claims to objective knowledge and to explain its record of empirical success. The explanatory project of the philosophy of science is to identify the distinctive methods that the sciences share which enables them to secure knowledge; the justificatory project consisted in showing that this method is the right one, providing its foundations in logic – both inductive and deductive – and epistemology – whether empiricist, rationalist or some third alternative. These ongoing projects came up against traditional philosophical problems. In particular the underdetermination of theoretical knowledge by observational knowledge has made both the explanatory task and the justificatory one far more difficult. If observations underdetermine theory, then discovering the actual inference rules – the methods – that in fact are employed by science is a complicated matter that will require more than armchair logical theorizing. Philosophy will have to surrender exclusive domain over the explanatory task, if it ever had such domain, to psychologists, historians and others equipped empirically to explore the cognitive processes that take scientists from hypotheses to data and back to theory. More radical has been the effect of underdetermination on the justificatory program. Underdetermination of theory by data means that no single hypothesis is supported or disconfirmed by any amount of observation. If data support theory at all they do so in larger units than the single hypothesis. So it was that

empiricist philosophers of science were driven to a "**holism**" about justification: the unit of empirical support is the entire theory – both the hypothesis directly under test, every other part of the theory that supports the tested hypothesis, and all the auxiliary hypotheses needed to deliver the test.

Even more radically, the traditional philosophical gulf between justification and explanation came to be challenged by philosophers themselves. Explanations, as we noted in Chapter 2, cite causes, and causal claims are contingent, not necessary truths. The world could have been otherwise arranged and the laws of nature might have been different. That is why we need to undertake factual inquiry, not logical analysis, to uncover causes and provide explanations. Justification is, however, not a causal but a logical relationship between things. What may cause you to believe something does not thereby constitute evidence that supports your belief as well justified. Observing one thing happen may cause you to believe something, but it won't justify that belief unless there is the right sort of logical relation between the things observed. These logical relations are studied naturally enough by philosophers, who seek their grounds: what makes the rules of logic – deductive or inductive – the right rules for justifying conclusions derived from premises, i.e. from evidence. The traditional philosophical answer to the question what makes these the right rules is that they are necessary truths that could not be otherwise.

Empiricists have a difficulty with this answer because they hold that knowledge is justified by experience and that experience cannot demonstrate necessity. Therefore, logical principles which are to justify reasoning were at risk of being ungrounded themselves. For at least two hundred years the empiricist's solution to the problem is to treat all necessary truths, whether in logic or mathematics, as true by definition, as reports about the meaning of words, conventions we adopt to communicate. As such these statements are true by stipulation. The logical rule which tells us that all inferences of the form

if **p** then **q**

p

therefore

q

is true because it reflects the meanings of the terms "if", "then", "therefore". Similarly, all the truths of mathematics, from 2 + 2 = 4 to the

Pythagorean theorem to Fermat's last theorem (there are no positive integer values of **n** greater than 2 such that $\mathbf{x}^\mathbf{n} + \mathbf{y}^\mathbf{n} = \mathbf{z}^\mathbf{n}$) are simply logically deduced from premises which are themselves definitions.

But twentieth-century work in the foundations of mathematics showed that mathematics cannot simply be composed of definitions and the consequences of them. When it was proved by Kurt Gödel that no set of mathematical statements can be both complete (enabling us to derive all the truths of arithmetic) and consistent (including no contradictions), the empiricist claim that necessary truths were all definitions came undone. Empiricism needed a new theory of necessary truths, or it needed to deny that there are any. This is where holism and underdetermination re-enter the story.

A necessary truth, whether trivially true, like "All bachelors are unmarried" or less obviously true, like "the internal angles of a triangle equal 180 degrees" is one that cannot be disconfirmed by experience. But holism teaches us that the same can be said for statements we consider to be contingent truths about the world, statements like "the spin angular momentum of an electron is quantized" or "the speed of light is the same in all reference frames", or, in the past, Newton's laws of motion. Scientists always prefer to make adjustments elsewhere rather than give up these statements. If holism is right, we can always preserve statements like these as true "come what may", simply by revising some other part of our system of beliefs about the world. But then, what does the difference between necessary truths and contingent ones we are unwilling to surrender come to? Well, necessary truths are true just in virtue of the meaning of words that express them, and contingent ones are true in virtue of facts about the world. But if two statements are both unrevisable, how can we tell empirically whether one is protected from revision because of meanings and the other because of beliefs about the world? Notice this is an empiricist challenge to an empiricist thesis, or as Quine put it, a "dogma": that we can distinguish truth in virtue of meanings from truth in virtue of facts.

What are meanings? Recall the empiricist theory sketched in Chapter 4, which holds that meanings are ultimately a matter of sensory experience: the meaning of a word is given by definition in terms of some basement level of words that name sensory qualities – colors, shapes, smells, textures, etc. This theory of language resonates with our pre-philosophical belief that words name images or ideas in the head. But as we have seen, it cannot make sense of the meaning of many terms in theoretical science. What is more, it is hard to see how we could empirically tell the difference between a truth about sensations which defines a term, and a sentence that reports a fact about the world: suppose we define salty thus: "salty is the taste one gets under standard conditions from sea water". What is the difference between this sentence and "salty is the taste one gets under standard conditions from dissolved potassium chloride". One cannot say the former is true in virtue of meaning, because it is meaning that we are trying to elucidate empirically by contrasting these two sentences. One cannot say that "potassium chloride" is a theoretical term and that makes the difference, because "sea water" is equally not a label we can pin on a sample of clear liquid by mere visual inspection. We had to add the "standard conditions" clause to both sentences, because without them, they would both be false (an anesthetized tongue won't taste either as salty). But having added the clause, both can be maintained as true, come what may in our experience. In short, the meaning of words is not given by the sensory data we associate with them. Or if it is given by sensory experience, the relation is very complex. The conclusion Quine came to was that the "meanings" were suspect and no self-respecting empiricist philosopher should want to trade in them. A conclusion with wider support in the philosophy of science was "holism about meaning", a doctrine similar to and mutually supportive of the epistemological thesis of holism in the way data tests theory.

If there are no meanings, or no truths of meaning distinct from truths about the world, if theory meets data as a whole, and the meaning of a theory's terms are given by their place or role in a theory, then we have not just a philosophical explanation for underdetermination, but a philosophical foundation for incommensurability as well. Or at least we will if we part company from Quine in one respect. Despite his rejection of the empiricist theories of meaning and of evidence, Quine did not surrender his commitment to an observational language with a special role in adjudicating competing scientific theories.

Given a continuing role for observation, we may not be able to compare theories sentence by sentence for observational support, or to translate the purport of competing theories into statements about what exactly we will observe under mutually agreed-upon circumstances. But we will be able rationally to choose between theories on the basis of their all-around powers to systematize and predict observations. The result for Quine and his followers was a sort of pragmatism that retained for science its claim to objectivity.

However, the implications of Quine's critique of empiricism's theory of meaning and of evidence make for a more radical holism about mathematics, all the empirical sciences and philosophy for that matter. If we cannot distinguish between statements true in virtue of meaning and statements true in virtue of facts about the world, then there is no distinction of kind between the formal sciences, like mathematics, and the empirical sciences, such as physics or biology. Traditionally, mathematics –

geometry, algebra, and logic – were held to be necessary truths. In epistemology empiricists differed from rationalists about our knowledge of these necessities. Empiricists held them to be truths of meaning without content; this is why they are necessary, because they reflect our decisions about how to use the concepts of mathematics. Rationalists held that these truths were not empty or trivial disguised definitions and their consequences, but truths which experience could not justify. Rationalism could not provide in the end a satisfactory account of how we can acquire such knowledge and so went into eclipse, at least as the basis for a viable philosophy of mathematics and science. But, to the extent that empiricism could not draw an empirically well-grounded distinction between truth in virtue of meaning and truth in virtue of facts about the world, its account of how we have knowledge of necessary truths collapses. Quine's conclusion is that all statements we take to be true are of one kind, that there is no grounded distinction between necessary truths and contingent ones. So, mathematical truths simply turn out to be the most central and relatively unrevisable of our scientific hypotheses.

What goes for mathematics, goes for philosophy too – including metaphysics, epistemology, logic and the study of scientific methodology. Theories in these compartments of philosophy turn out also to be no different from theoretical claims in the sciences. A theory of the nature, extent and justification of knowledge will turn out for Quine to be a compartment of psychology; metaphysics – the study of the basic categories of nature – will turn out to be continuous with physics and the other sciences, and its best theory will be the one which, when put together with what we know from the rest of science, gives us the most adequate account of the world, judged as a whole by its ability to explain and predict our observations. Methodology and logic also are inquiries to be pursued together with, and not as independent foundations for, the rest of science. Those methods and those logical principles are most well-supported which are reflected in the pursuit of successful science. Here the notion of "empirical adequacy" which we met in Chapter 4 is relevant. Quine's criterion for theory choice in philosophy and in science is empirical adequacy.

Instrumentalists argue for their doctrine from the privileged position of a prior philosophical theory, adherence to a strict empiricism. Quine rejects the claim that there is some body of knowledge, say, a philosophy or an epistemology, which has greater credibility than science, and might provide a foundation for it. Though he holds that science should aim for empirical adequacy, he does so because this is the criterion of adequacy which science sets itself; what is more, unlike the instrumentalist, and like the scientist, Quine takes the theoretical claims of science about unobservables not just literally but as among the most well-founded of our

beliefs, because in the package of our beliefs we call science, these are among the most central, secure and relatively unrevisable. In fact, for Quine and his followers, science is as much a guide to philosophy as philosophy is to science. The difference between science and philosophy is one of degree of generality and abstractness, not a difference between necessary truths and factually contingent ones.

The resulting philosophy of science has come to be called "**naturalism**". Among philosophers, naturalism became the successor to empiricism largely as a result of Quine's influence. The label, "naturalist" is one many philosophers of science subsequently adopted despite differences among their philosophies of science. But as Quine defended it, naturalism's chief tenets are, first, the rejection of philosophy as the foundation for science, the arbiter of its methods, or the determinant of its nature and limits; second, the relevance of science to the solution of philosophical problems; third, the special credibility of physics as among the most secure and well-founded portion of human knowledge; and, fourth, the relevance of certain scientific theories as of particular importance to advancing our philosophical understanding, in particular the Darwinian theory of natural selection. The importance of Darwinian theory as a scientific guide to the solution of philosophical problems is owing to its account of how blind mechanistic processes can give rise to the *appearance* to us of purpose and design in a world of blind variation and natural selection. Recall the problem of teleological or goal-directed processes and their causal explanation discussed in Chapter 2. Physical science has no conceptual room for final causes, for effects in the future bringing about causes in the past. Still less does it have scope for an omnipotent designer who brings things about to suit his or her desires. This is why the physical world-view finds so attractive a theory like Darwin's, which provided a causal mechanism – the perpetual occurrence of variation (through mutation and recombination) in traits that just happened to be heritable, and the long-term winnowing out by the environment of those variations that work worse than others. If we can use the same mechanism of random heritable variation and selection by the environment to explain other apparently purposive non-physical processes, especially human affairs, we will have accommodated these processes at least in principle to a single scientifically coherent world-view – a naturalistic philosophy.

Exploiting Darwinism, philosophers have sought to provide a naturalistic account of scientific change, similar in some respects to Kuhn's account of scientific progress as local adaptation. Others have sought [illegible] epistemology or an account of how scientists actually reason and theorize as random variation (i.e. creative theoretical speculation) and selection by the environment (i.e. experiment and observation). Others have [illegible] an account of the nature of thought in general by appeal to Darwinian processes. Still other philosophers have made common cause with social scientists in building theories of human behavior from a Darwinian basis. Applying Darwinian theory as a research program in philosophy has expanded widely from Quine's original articulation. Doing so makes concrete naturalism's claim that science and philosophy are of a piece and that our most well-established scientific claims should have as much influence on the framing of philosophical theories as our philosophy may have on science.

But naturalism leaves as yet unsolved a major problem. Recall the distinction between justification and causation. Justification gives grounds for the truth of belief; causation does not. Or at least so it seems. In the empiricist's hands, justification is a logical relation (employing deductive or inductive logic) between evidence (sensory experience) and conclusion, and logic is a matter of meanings. Naturalists, or at least Quineans, cannot help themselves to this way of drawing the distinction between causation and justification. Yet draw it they must. Without recourse to a "first philosophy", some body of *a priori* truths, or even definitions, naturalism can only appeal to the sciences themselves to understand the inference rules, methods of reasoning, methodologies of inquiry and principles of epistemology which will distinguish between those conclusions justified by evidence and those not justified by it.

Now, suppose one asks of a principle of logic, or a methodology, whether this method or rule which justifies conclusions is itself justified or well-grounded. The empiricist has an answer to this question: the rule or method is necessarily true, and its necessity rests on our decision about how to use language. We may dispute this argument, and naturalists will do so, because it trades on notions in dispute between empiricists and naturalists – notions like "necessity" and "meaning". But what can naturalists say when asked to ground their own justificatory rules and methods. Appeal to a "first philosophy", an epistemology prior to and more secure than science, is out of the question. And naturalism cannot appeal to science or its success to ground its rules. For the appeal to a "first philosophy" would be circular, and grounding its rules on science's technological success would be to surrender naturalism to a first philosophy – in this case, one called "pragmatism".

Naturalism justifies the epistemology, logic and methodology it recommends because this trio of theories and rules emerges from successful science – i.e. research programs which provide knowledge – justified conclusions – about the way the world works. But if asked why they claim that successful science provides such justified conclusions, naturalists cannot then go on to cite the fact that successful science proceeds by rules and methods which certify its conclusions as justified, because these rules and methods are themselves certified by science's success. Naturalism would be reasoning in a circle. This is a particularly acute problem for Quine, because many of his arguments against empiricism's answers to these questions, by appeal to concepts of logical necessity and meaning, accused these answers of circular reasoning.

To appeal to the practical, technological, applied success of science might solve the naturalist's justificatory problem. But the result would no longer be naturalism. Science does in fact have a magnificent track record of technological application with practical, pragmatic success. But why should this provide a justification for its claims to constitute knowledge or its methods to count as an epistemology? It does so only if we erect a prior first philosophy. Call it pragmatism, after the early twentieth-century American philosophers – William James, C. S. Peirce and John Dewey – who explicitly adopted this view. This philosophy may have much to recommend it, but it is not naturalism, for it begins with a philosophical commitment prior to science, and may have to surrender those parts of science incompatible with it.

Naturalism is thus left with an as yet unfulfilled obligation. It aims to underwrite the objectivity of science, its status as ever-improving knowledge of the nature of things. It also aims to reflect the actual character of science in its philosophy of science, without giving either philosophy or history a privileged role in the foundations of science or the understanding of its claims about the world. But it needs to answer in a way consistent with its own principles, and its critique of competing conceptions, the question of its own justification.

Summary

According to Kuhn, the unit of scientific thought and action is the paradigm, not the theory. Specifying what a paradigm is may be difficult, for it includes not just textbook presentations of theory, but exemplary problem-solutions, standard equipment, a methodology, and usually even a philosophy. Among the important paradigms of the history of science have been the Aristotelian, the Ptolemaic and the Newtonian in physics. Chemistry before Lavoisier, and biology before Darwin, were "pre-paradigm" disciplines, not yet really "scientific", for without the paradigm there is no "normal science" to accumulate information that illuminates the paradigm. The paradigm controls what counts as data relevant to testing hypotheses. There is, Kuhn argued, along with other opponents of empiricism, no observational vocabulary, no court of final authority in experience. Experience comes to us already laden with theory.

Crisis emerges for a paradigm when a puzzle cannot be solved, and begins to be treated like an anomaly. When the anomaly begins to occupy most of the attention of the figures at the research frontier of the discipline, it is ripe for revolution. The revolution consists in a new paradigm that solves the anomaly, but not necessarily while preserving the gains of the previous paradigm. What the old paradigm explained, the new one may fail to explain, or even to recognize. Whence it follows that scientific change – the succession of paradigms – need not be a progressive change in the direction of successive approximation to the truth.

Observation does not control inquiry, rather inquiry is controlled by scientists, articulating the paradigm, enforcing its discipline, assuring their own places in its establishment, except at those crucial moments in the history of science when things become unstuck and a revolution ensues – a revolution which we should understand as more in the nature of a palace coup than the overthrow of an old theory by one rationally certifiable as better or more correct.

This picture of science is hard to take seriously from the perspective of empiricism, historical or logical. It gained currency among historians, sociologists and psychologists, at the same time as, and in part because of, the influence of the philosopher W. V. O. Quine, who unraveled the tapestry of philosophical theories of science as cumulative observational knowledge about the nature of reality.

Quine began by undermining two distinctions: that between statements true as a matter of logic or form, and statements true as a matter of content or empirically observable fact. It may be surprising, but once this distinction, well-known to philosophy since Kant, is surrendered, everything in epistemology and much in the philosophy of science becomes unstuck. The denial of this distinction gives rise to holism about how theory confronts experience, and to the underdetermination which spawns Kuhn's approach to the nature of science. But it also gives rise to a stronger commitment to science, by some philosophers, than even to philosophy, or at least it gives rise to the idea that we must let contemporary science guide our philosophy, instead of seeking science's foundations in philosophy. Philosophers, largely followers of Quine, who have adopted this view label themselves "naturalists", a term unfortunately that others, especially sociologists adopting incompatible views, have also adopted.

Naturally, neither Quine nor other philosophers are prepared to accept Kuhn's apparent subjectivism about science as the correct conclusion to draw from their attack on empiricism. The problem therefore remains of finding a foundation for science as objective knowledge consistent with these arguments. This is the subject of the next chapter.

Questions

1 Which among various approaches to the study of science – philosophy, history, sociology – is the more fundamental? Do these disciplines compete with one another in answer to questions about science?
2 How would a defender of Kuhn respond to the claim that the history of technological progress which science has made possible refutes Kuhn's claim that science is not globally progressive?
3 Kuhn's arguments against the existence of a level of observation free from theory date back to the 1950s. Have subsequent developments in psychology tended to vindicate or undermine his claims?
4 Quine once said, "philosophy of physics is philosophy enough". Give an interpretation of this claim that reflects Quine's claims about the relation between science and philosophy.
5 Is naturalism question-begging? That is, does according the findings of science control over philosophical theorizing rest on mere assertion that science is our best guide to the nature of reality?

Further reading

Every student of the philosophy of science must read T. S. Kuhn, *The Structure of Scientific Revolutions*. Other important works of Kuhn's include *The Essential Tension*, which includes important reflections on the earlier book. An important review of *Structure* is D. Shapere, "Review of *Structure of Scientific Revolutions*". This and other commentaries on Kuhn are reprinted in G. Gutting, *Paradigms and Revolutions*. A *festschrift* for Kuhn containing several important retrospective papers is Horwich, *World Changes: Thomas Kuhn and the Nature of Science*.

P. Feyerabend, *Against Method*, summarizes a series of papers in which the author champions a philosophically informed version of the most radical interpretation of Kuhn's views.

Quine's attack on empiricism emerges in *From a Logical Point of View*, which contains his extremely influential essay, "Two Dogmas of Empiricism". This too is required reading for anyone interested in the philosophy of science. Quine, *Word and Object*, is a later work that deepens the attack on empiricism, and develops the doctrine of underdetermination so influential on Kuhn and others.

Naturalism is expounded and defended in P. Kitcher, *The Advancement of Science*.

CHAPTER 7
The nature of science and the fundamental questions of philosophy

Overview

Kuhn's doctrines have generally been interpreted so as to give rise to relativism – the theory that there are no truths, or at least that nothing can be asserted to be true independent of some point of view, and that disagreements between points of view are irreconcilable. The result of course is to deprive science of a position of strength from which it can defend its findings as more well-justified than those of pseudo-science; it also undermines the claims of the so-called "hard sciences" – physics and chemistry – to greater authority for their findings, methods, standards of argument and explanation, and strictures on theory-construction, than can be claimed by the "soft sciences" and the humanities. Postmodernists and deconstructionists took much support from a radical interpretation of Kuhn's doctrines for the relativism they embraced.

Among sociologists of science especially, a "**strong program**" emerged to argue that the same factors which explain scientific successes must also explain scientific failures, and this deprives facts about the world – as reported in the results of observations and experiments – of their decisive role in explaining the success of science.

These doctrines had a liberating effect on the social and behavioral sciences and other disciplines which had hitherto sought acceptance by aping "scientific methods" but no longer felt the need to do so. The sociological and even more the political focus on science revealed its traditional associations with the middle classes, and with capitalism, its blindness towards the interests of women, and its indifference to minorities.

But in the end the doctrine that science is not a distinctive body of knowledge, one which attains higher standards of objectivity and reliability than other methods, is not sustainable. This conclusion, however, requires that we return to the fundamental problems in epistemology, the philosophy of language and metaphysics in order to see where philosophy went wrong and led the followers of Kuhn to conclusions of such patent preposterousness. It may also require that we attend to the finds of relevant sciences, such as cognitive and perceptual psychology, to discover whether there are theory-free sources of data and hypothesis-formation in our psychological make-up.

1 From philosophy to history to relativism

The interaction of the naturalism that Quine inspired, and the reading of the history of science which Kuhn provided, together have had a profoundly unsettling impact on the philosophy of science. It shook literally centuries of philosophy's confidence that it understood science. This sudden loss of confidence that we know what science is, whether it progresses and how it does so, and what the sources of its claims to objectivity can be, left an intellectual vacuum. It is a vacuum into which many sociologists, psychologists, political theorists, historians and other social scientists were drawn. One result of the heated and highly visible controversy which emerged was to make it apparent that the solution to problems in the philosophy of science requires a re-examination of the most fundamental questions in other compartments of philosophy, including epistemology, metaphysics, the philosophy of language, and even portions of moral and political philosophy.

Kuhn held that paradigms are incommensurable. This means that they cannot be translated into one another, at least not completely and perhaps not at all; incommensurability also implies explanatory losses as well as gains, and no common measuring system to tell when the gains are greater than losses; incommensurability between paradigms reaches down to their observational vocabulary, and deprives us of a paradigm-neutral position from which to assess competing paradigms. The result is a picture of science not as the succession of more and more complete explanations of a wider and deeper range of phenomena, nor even the persistent expansion of predictive power and accuracy over the same range of phenomena. Rather, the history of science is more like the history of fashions, or political regimes, which succeed one another not because of their cognitive merits, but because of shifts in political power and social influence. This conception of the history of science is an invitation to **epistemic relativism**.

Ethical relativism is the claim that which actions are morally right varies from culture to culture and that there is no such thing as objective rightness in morality. Ethical relativism is seen by its proponents as an open-minded and multicultural attitude of tolerance and understanding about ethnic differences. Ethical relativism leads inevitably to skepticism about whether there really is any such thing as absolute moral rightness at all. Epistemic relativism similarly makes knowledge (and therefore truth) relative to a conceptual scheme, a point of view or perspective. It denies that there can be an objective truth about the way the world is, independent of any paradigm, nor consequently any way to compare paradigms for truth, objectivity or epistemic warrant. Kuhn was ambivalent about whether to plead guilty to the charge of epistemic relativism among paradigms.

But the situation may be even more fraught than Kuhn supposed. For there were philosophers and others eager to transform Kuhn's claims about the broadest paradigms that characterize century-long epochs of normal science, into the incommensurability of individual scientific theories even within the ambit of normal science. And Quine's fundamental philosophical arguments gave them the resources to do so. Most influential among these philosophers was Paul A. Feyerabend. Adopting Kuhn's insights about the irreducibility of Aristotelian mechanics to Newton's theory, and Newtonian mechanics to Einstein's, Feyerabend argued that the impossibility of translating the key concepts of impetus into inertia, or absolute mass into relative mass, reflects a barrier to reduction among all theories. The reason is the holism about meaning that Quine's insights spawned. The meaning of a theoretical term is not given by its connection, direct or indirect, to observation, because theory does not meet observation word by word or even sentence by sentence, but only as a whole. So, meanings are theoretical. The meaning of a theoretical term is given by its place in the structure of the theory in which it figures. Change one or more parts of a theory and the result is not an improvement on the same theory, but an altogether new and different one. Why? Because the new theory is not about the same subject-matter as the old theory, since its words have different meanings. "Electron", though it may be an inscription in Bohr's theory, Thomson's theory, Heisenberg's and Schrodinger's, no more means the same thing in each of them as does "cat" mean the same in "pussy cat", "catastrophe", "cool cat", and "cat o' nine tails".

Denying this holistic claim about meanings requires an entire theory of meaning, or at least a reasoned objection to Quine's attack on meanings. When added to the denial of an observational language that could frame statements about data, statements that might enable us to choose between theories, the result is what Feyerabend praised as "methodological anarchy". He called it methodological anarchy because the result is that there is no cognitive basis to choose between theories. In particular, earlier and "well-established" theories have no claim to our adherence above later and less well-established ones. And Feyerabend praised this outcome because he held that such anarchy stimulates scientific originality and creativity. After all, if Newton had been required to advance a theory which could treat Aristotle's as a special case, or had Einstein been required to do so for Newton, just because of the explanatory and predictive successes of Aristotle's or Newton's theory, neither Newton nor Einstein would have produced the great scientific revolutions which bear their names. Just as moral relativists think their insight emancipatory and enlightened, so did Feyerabend think his epistemic relativism a good thing.

Feyerabend and other relativists would stigmatize naturalism from just this perspective. Like Kuhn, and like naturalists for that matter, relativists will agree that an epistemology and a methodology are parts of a paradigm, or in fact components of a theory, although perhaps these components are expressed grammatically in the imperative instead of the indicative. As such, epistemology and methodology don't provide an independent position from which to adjudicate scientific advance, or even the status of a discipline as "Scientific" with a capital "S". These relativists would seize upon the problem of circularity that faces naturalism to substantiate their claim that any particular theory, paradigm or discipline is but one among many "ways of knowing", and that there is no such thing as one of them being correct and the others mistaken. So far as the relativist is concerned, "Anything Goes". This in fact was the title of a book in which Feyerabend most forcefully argued for this view. Instead of a brief biography, Feyerabend provided his astrological chart on the book's dust-jacket. He meant to suggest that astrology was as informative about the author as personal facts about his education, career and previous books might have been.

But if, from the philosophical point of view, anything goes, the question emerges, why has science taken the particular route that it has over time? For the relativists the answer cannot be that the history of science is the history of inquiry "tracking the truth", changing in the direction of a closer and closer approximation to the truth about the world. Indeed, the way the world is, independently of science, can have no role in determining the shape of particular sciences or science in general. That is because there is literally no way the world is, independent of science. We can take this claim either literally or figuratively, as we will see. If the history of science is not explained by the dispassionate study of the way the world is by objective and disinterested scientists, it must, like all the history of all other social institutions, be the outcome of social, political, psychological, economic and other "non-cognitive" factors. So, to understand science, the particular sciences and the nature of scientific change, relativists argue, we must do social science. For example, to learn why Darwin's theory of evolution as gradual selection of locally fitter traits triumphed does not require that we understand the fossil record, still less the sources of variation and environmental filters. It requires that we understand the social and political forces that shaped theory construction and acceptance in the nineteenth century. Once we understand the ideological needs of nineteenth-century *laissez-faire* capitalism to justify relentless competition in which the less fit were ground under and progress was a matter of market competition, the emergence of the Darwinian paradigm should be no surprise. That the history of science should be rewritten by each successive paradigm is now understandable

not just because normal science requires ideological discipline, but because political domination requires it as well.

The denial that tracking the truth had a special role in the explanation of scientific change, which it lacks in, say, changes in literature or fashion, led in the 1980s to an important new movement in the sociological study of science, and a concomitant claim by this movement that sociology must displace philosophy as our source for understanding science. The so-called "strong program" in the sociology of science set out to explain both scientific successes and failures on the same basis. Since what distinguishes those scientific developments that are accepted as advances from those rejected (with hindsight) as mistaken cannot be that the former reflect the way the world works and that the latter do not, both must be explained in the same way. The sociologist David Bloor described this as the "symmetry thesis": it leaves no space for any argument that what explains successful scientific theorizing is that it is more rational than unsuccessful theorizing.

These sociologists and other social scientists sought to study the close details of scientific work, and concluded that, like other social products, scientific agreement was "constructed" through "negotiation" between parties whose interests are not exclusively or perhaps even predominantly describing the way the world works. Rather their interests are personal advancement, recognition, material reward, social status, and other benefits which bear no connection to the declared, publicly stated, advertised objectives of science: the disinterested pursuit of truth. In the hands of radical postmodern students of science, the thesis that scientific findings are constructed becomes the claim that the world external to scientific theory, which realists identify as the independent reality that makes scientific claims true or false, is itself a construction without existence independent of the scientists who agree upon their descriptions of it. This "idealism", according to which to exist is nothing more than to be thought of, goes back in philosophy of science to the eighteenth-century philosopher George Berkeley, and certainly has the explicit support of at least some perhaps incautious remarks of Thomas Kuhn: those which suggest that exponents of differing paradigms live in differing worlds. Moreover, the postmodern sociologists held that the unit of scientific belief is not the individual scientist but the community of scientists at work in a particular research program. Rejecting the first-person point of view associated with traditional scientific philosophy since Descartes, these scholars argued that facts in science were constructed through negotiation among interested parties, instead of discovered by individuals, and researchers subject to replication by other individual researchers.

The social character of knowledge will not only explain the enforcement of consensus, it will also explain certain defects and deficiencies in science, ones which mirror the character of western culture as a whole. Thus, some social scientists – for example, feminists and post-colonialism scholars – have sought to explain the character of science as at least in part the product of patriarchal or racialist agendas. Beginning with blatant examples of sexism among scientists – such as constructing human research populations that exclude women or minorities – or racist pursuit of evolutionary research which stigmatizes races as bearing hereditary limitations which cannot be ameliorated by environmental compensation, these social students of science went on to conclude that much of science reflects such limitations, though few scientists, even those whose work produces such baleful consequences, are conscious of them.

Social critics, commentators and humanists have drawn much inspiration from this social study of science, mainly to "dethrone" science from a position of undue and unjustified authority and respect to which western society has accorded it over the half a millennium since the Renaissance. Critics have begun with the evident fact that science and scientific findings have been misused in two ways. First, they have provided more efficient and effective ways of harming people, other organisms and the environment. Second, these critics of science have gone on to note that science has provided unwarranted rationalization for policies that affect such harms – eugenics for example. Even societies which have not blatantly misused science are often guilty of "scientism" – the unwarranted attribution to science of special epistemic authority. There are, according to these critics, other ways of knowing besides the methods science employs. Disciplines stigmatized as pseudo-science, such as astrology or parapsychology; the theories that stand behind alternative "holistic"therapies in medicine, like homeopathy; non-standard cultivation practices, such as playing music to one's houseplants – these are of equal standing. To deny their epistemic status is simply to argue from the blinkered and question-begging perspective of the Newtonian paradigm, a paradigm for that matter now superseded by scientific advances in cosmology and quantum physics for which we have as yet no acceptable philosophical interpretation. Who can say that when the dust settles in these areas, alternative non-Newtonian ways of knowing will not have been vindicated. To the extent that the social study of science deriving from Kuhn has undermined the credentials of traditional natural science, it has made more controversial the public support for the sciences in those countries, especially Great Britain, where the sociology of science has been most visible and intellectually influential. Some physicists have attacked the social studies of science as weakening public support for natural scientific research. Most scientists simply take these views no more seriously than claims to the effect that the earth is flat. The philosophy of science cannot afford so cavalier an attitude.

Less extreme versions of the relativism associated with the social study of science motivate certain philosophies of social science and certain accounts of the nature of knowledge in the humanities. Thus, qualitative social science has come to defend its methods and results against attack from empirical and quantitative social scientists by claiming for itself the status of a separate and incommensurable paradigm. These defenders of qualitative social science go on to the counter-attack, arguing that the empirical, quantitative, experimental paradigm is incapable of dealing with human meaning, significance and interpretation; that these are the essential dimensions along which human action, emotion and value are to be understood; that the natural-science paradigm cannot even accommodate the notion of semantic meaning, let alone human significance; and that the sterility and frustration of much social science is the result of slavishly attempting to implement an inappropriate paradigm from the natural sciences. The inability to surrender this paradigm in the face of anomalies of the sort that should lead to the questioning of normal science is a tribute to the social and cultural power of natural science as a model for all compartments of human knowledge. Nevertheless, it is the wrong model. So these critics of scientism argue.

2 Could the earth really be flat?

For all of Kuhn's insights into the history of science, something has gone seriously wrong in the development of the social studies of science since his time. So, at least, an unbiased observer (or perhaps someone in the grip of a scientistic paradigm) would suppose. Much of the motivation for the attempt to understand natural science stems from an appreciation of its predictive power and explanatory depth, from the desire to identify its methodological secrets so that they can be applied elsewhere (especially in the social and behavioral sciences) with the same theoretical insights and technological results. When an inquiry so motivated concludes that science is just another religion, just one of a wide variety of ways of looking at the world, none of which can claim greater objectivity than the others, then sometime, somewhere, we have taken a wrong turn in our inquiry.

But where? It is simply not enough to turn one's back on Kuhn's insights, nor on the arguments against the pretensions of science mounted on top of them. Many philosophers of science have concluded that Kuhn's historical account of scientific change has been "over-interpreted"; that he did not intend *The Structure of Scientific Revolutions* as a broadside attack on the objectivity of science. In this they had the support of Kuhn, at least while he still lived. It had not been his intention to cast science down from its claims to objectivity, but to enhance our understanding of it as a human enterprise. Similarly, Quine and his philosophical followers could not countenance the misapplication of their doctrine of underdetermination to support the conclusion that current scientific conclusions are not the most reasonable and well-supported conclusions we can draw about the world. But what Kuhn and Quine may have intended cannot decide what their arguments have in fact established or suggested.

What the defender of scientific objectivity, or at least its possibility, must do, is undermine the claims of incommensurability. To do this one must either attack the assimilation of observation to theorizing, or reconcile it with the possibility of testing theories by observation in a non-question-begging manner. And to show how science can make progress over theoretical change that cumulates knowledge, we will have to show how translation between theories can be effected.

One way defenders of objectivity in science have attempted to reconcile the assimilation of observation to theory with its continued role in testing is to draw a distinction between the categories we adopt for classifying particular items – objects, processes, events, phenomena, data – and the particular acts of classification themselves. Differing and even incommensurable categorical frameworks can be reconciled with agreement about actual findings, thereby making objectivity in the recording of data possible. The difference is like that between the letter-box pigeon-holes in a departmental office and the particular pieces of mail that are distributed to these pigeon holes. Adopting a particular set of labels for boxes doesn't prejudge what pieces of mail will come in. Observations are like pieces of mail. Their descriptions are the labels on the classes into which we sort observations. A hypothesis is a claim that members of one category will also fit into another, or always come together with members of another category. There may be agreement on what falls into any category, and thus a way of testing hypotheses, even when the hypotheses are expressed in terms of categories controlled by a theory that is not itself tested by what falls into its categories. It can even turn out that differing categorical schemes will substantially overlap, thus allowing for agreement about data even between differing categorical frameworks. For example, items which the categorical framework of Einstein's theory of special relativity would classify as "having mass" would also be so classified by Newton's theory, notwithstanding the fact that the two theories mean something quite different by "having mass". And of course, we may surrender categorical systems when they no longer work well, that is, when it becomes difficult to use them to file things uniquely, or too complicated to figure out in which boxes they belong, if some significant numbers of boxes go unexpectedly unfilled, or if we can uncover no interesting hypotheses about which boxes get filled at the same time by the same things. Thus, observation can control theory even when its most basic descriptions reflect pre-established theories, even theories we don't recognize as theories, like those embodied in common sense and ordinary language.

But when one thinks about the notion of a categorical scheme and instances which are classified in accordance with it, the conclusion that there is a place for theory-controlling observations here is simply question-begging. To begin with, items don't come with labels that match up with the labels on the categories: samples of gold don't have the word "gold" printed on them. The simplest act of classification requires hypotheses about other categories. Classifying something as gold requires that we invoke the hypothesis that gold dissolves only in aqua regia. This hypothesis presupposes another set of hypotheses which enable us to tell what aqua regia is. And so on, *ad infinitum*. The *ad infinitum* is due to the fact that there is no basement level of words defined directly by experiences, as the historical empiricists held.

Second, how do we tell the difference between hypotheses about correlations between items in our classifications, like "gold is a conductor", and hypotheses like the one about gold and aqua regia that we need to do the classifying. We need to be able to tell the difference between these hypotheses if we are to treat one set as open to objective test, while the other set is not, owing merely to its classificatory role. We can't argue that the classificatory statements are true by definition (gold = whatever dissolves only in aqua regia), and the "gold is a conductor"-hypothesis is a claim about the world. We cannot do this without first having established a way of empirically telling the difference between definitions and factual claims, and doing this requires still another argument against Quine.

Third, categorical schemes are in fact hypotheses about the world, so the whole distinction breaks down. Consider the most successful categorical scheme science has ever established, Mendeleev's Periodic Table of the Elements. It is a successful categorical scheme because it "divides nature at the joints". The differences between the elements it systematizes are given by atomic theory. In the century after Mendeleev advanced his categorical system, discoveries, especially about nuclear structure and electron-shell-filling, explained the relationship between Mendeleev's rows and columns, and showed that it was more than a merely convenient filing system: it was a set of hypotheses about similarities and differences among elements – known and unknown – which required further and deeper explanation.

Fourth, and finally, it is pretty clear, especially in the case of fundamental theories or paradigms, that the disagreements are not about the individual instances and which categories they are to be filed in. Rather, the disagreements are about the definitions of the categories that make these agreements about classifying impossible, and cannot be compromised: compare Aristotle and Newton on what counts as "rest". Differences in classification reflect incommensurabilities that preclude theory-comparison.

Acceding to the assimilation of observation to theory, while distinguishing categories from their instances, will not preserve the objectivity of science. Rather, the defender of scientific objectivity will have to seek out countervailing evidence from the history of science and better psychological theory and data that counter the psychological claims on which the denial of the distinction between observation and theory rests. Such evidence might show that all humans have some common inherited sensory categorical scheme shaped by evolution to be adapted to success at science or some other enterprise which science can make use of. This is certainly one approach which has been adopted, especially by naturalists. It is open to the question-begging objection of course: appealing to findings and theories in psychology is itself to adopt a non-observational and therefore non-objective basis from which to criticize opposition to objectivity. But then, this is the same kind of evidence which Kuhn and his followers originally cited to undermine the observational theoretical distinction.

Such opponents of objectivity cannot have it both ways. Indeed, one might even charge them with the deepest form of incoherence, for they purport to offer arguments against the objectivity of science. Why should we believe these arguments? Do they constitute an objective basis for their conclusions? What makes their arguments and evidence probative, when the arguments of their opponents are always question-begging? These rhetorical questions do not carry the debate very far. This is largely because opponents of scientific objectivity have little interest in convincing others that their view is correct. Their dialectic position is largely defensive; their aim is to protect areas of intellectual life from the hegemony of natural science. To do so, they need only challenge its pretensions to exclusivity as a "way of knowing". These opponents of scientific objectivity cannot and need not argue for a thesis stronger than epistemic relativism.

The opponent of scientific objectivity's strongest card therefore is the incommensurability of meanings that insulates paradigms and theories even from intertranslation. Incommensurability means that no critique of any theory from the perspective of another is even intelligible. Again, it is not enough to call this doctrine self-refuting, on the ground that in order to communicate it to someone with whom prior agreement has not been established, the doctrine must be false. Such a *reductio ad absurdum* argument is a matter of indifference to opponents of objectivity in science interested not in convincing others but in defending their own view as invincible.

One apparently attractive alternative to the *reductio* argument begins by drawing attention to a fundamental distinction in the philosophy of language: meaning versus reference. Meanings, all will admit, are a great difficulty for philosophy, psychology, linguistics; but reference, or denotation, or extension of a term, seems less problematical. What a word names, what it refers to, is something out there in the world, by contrast with what it means, which may be in the head of a speaker and/or a listener, or for that matter may be a social rule or convention, or a matter of use, or as Quine and his followers might have it, nothing at all. And because the reference of a term is something out there, as opposed to in here (pointing to the head), speakers may agree on what a term names without agreeing on what the term means. Or, in the case of terms that name properties instead of things, like "red" or "loud", we can agree on the instances of things and events that bear these properties. The things which are instances of "red" or "sweet" or "rigid" are members of the "extension" of the term "red" or "sweet" or "rigid". We can agree by inspection on whether things are in the extension of "red" or not, even when we can't get into one another's heads to find out whether what looks red to you looks red to me. We can agree that "Superman" names the same item as "Clark Kent" without concurring that the two expressions have the same meaning (indeed, proper names, like "Clark Kent", have no meaning). Reference and extension, it may be held, are more basic and more indispensable to language than is meaning. Moreover, it is tempting to argue, in the manner of the empiricists of the eighteenth century, that language cannot be learned unless it starts with terms that have only reference or extension or something like it. For if every term has meaning – given by other words – it will be impossible for a child to break into the circle of meaningful terms. To break into language, some words must come to us as understandable solely by learning what they refer to, or at least what events stimulate others to use them.

Finally, there are good arguments to suggest that what is really indispensable for science and mathematics is not that the meanings of terms be given, but that their references be fixed. Take any truth of arithmetic, for example, and substitute any term within it that preserves reference, and the statement will remain true. For example: $3^2 = 9$ remains true when it is expressed as the square of the number of ships in Columbus's 1492 fleet equals the number of fielders on a baseball diamond. If two scientists can agree on the reference of terms, or on the set of things a scientific term is true of – for example, the set of things that have mass, whether Einsteinian or Newtonian – they need not agree on the meaning of the term, or whether a translation is available from one meaning for the term to another. Could agreement on reference be enough to ensure commensurability between scientific hypotheses, theories or paradigms? So some defenders of objectivity, following Israel Sheffler, have argued.

Suppose inquirers could agree on the reference or extension of a set of terms "F" and "G" without even discussing their meanings. Suppose further that this agreement led them to agree on when the extensions of these terms overlap, or indeed are identical. In the latter case, they would have agreed that all Fs are Gs, even without knowing the meanings of "F" or "G". Such meaning-free agreement could be the basis for comparing the differing theories inquirers may embrace, even when these theories are incommensurable. A set of hypotheses about the correlations among objects named by categories on whose reference scientists agree would provide exactly the sort of theory-free court of final authority which would enable us to compare competing and incommensurable theories. Each hypothesis on which scientists concur under their purely referential construal, would be given different meaning by one or another incommensurable theory. But it would be an objective matter of mathematical or logical fact whether, thus interpreted, the hypotheses would be derivable from the theories to be compared. That theory would be best supported which deductively implied those hypotheses on the extension of whose terms there was agreement.

It doesn't take much thought to realize that the only hypotheses which will qualify as purely referential will be ones about objects on which agreement of reference can be established non-linguistically, i.e. by pointing or otherwise picking out things and properties without words. But the only candidates for such hypotheses will be those expressed in the vocabulary of everyday observations! In other words, the appeal to reference is but a covert way of bringing back into play the distinction between observational and theoretical vocabulary that started our problem. One way to see this is to consider how we establish the reference of a term. Suppose you wish to draw the attention of a non-English speaker to an object on your desk, say an apple. You could say "apple", but to a non-English speaker that will not discriminate the apple from anything else on your desk. Suppose you say "that" or "this", while pointing or touching the apple. Well, that will probably work, but it is because your interlocutor knows what an apple is and has a word for it. Now, suppose you wish to draw your interlocutor's attention to the stem of the apple, or the soft brown spot under the stem, or the worm wriggling out of the soft spot, or the depression just under the stem. How might you go about it? What you do now is just about what you did the first time: you point and say the words. And that reveals the problem of working with reference alone. There is no way to tell what you are referring to when you say "this" and point. It could be the apple, the soft spot, the darkest part of the soft spot, the stem, the space occupied by the apple, or any of a large number of other things in the general vicinity of your index finger. Of course this is not a problem when we have other descriptive terms to individuate the particular thing to which we are in fact referring. But the reason this works is of course that these other words have meaning and we know what their meanings are! In short, without a background of meanings already agreed to, reference doesn't work. Pure reference is a will-o'-the wisp. And the guide to reference is in fact meaning. The only purely referential terms in any language are the demonstrative pronouns – "this", "that" – and these fail to secure unique reference. Elsewhere in language the relation between reference and meaning is exactly the opposite of what we need. Securing reference relies on meaning. This is particularly apparent for scientific vocabulary, which is used to refer to unobservable things, processes and events, and their only indirectly detectable properties.

If meaning is our only guide to reference, and the meanings of each of the terms of a theory are given by the role which the terms play in the theory, then theoretical holism about meaning makes reference part of the problem for the defender of scientific objectivity, not part of the solution. If theories or paradigms come complete with categorical systems into which particular objects are classified, then exponents of two different paradigms or theories will not be able to agree on how particular things are classified except by the lights of their respective theories as a whole. This makes each of the theories recalcitrant to any experimental evidence that might disconfirm them. For in classifying events, things, processes, the entire theory is involved, and the description of a counterexample to the theory would simply be self-contradictory. Imagine, given the meaning of the word "rest" in Aristotle's physics, the idea that an object could be moving in a straight line at constant non-zero velocity and have no forces acting up on it? Movement for Aristotle is *ipso facto* not rest, and requires a continually acting force. Nothing would count as being free from the influence of forces which was moving at all. Similarly, whatever it is that an Einsteinian might treat as disconfirming Newton's principle of the conservation of mass, it cannot be anything that a Newtonian could even treat as having mass.

But suppose there is a way adequately to draw the distinction between observation and theorizing, and that we can establish at least in principle the possibility of translating across scientific theories and paradigms. Doing this will only put us in a position to take seriously the problem of underdetermination. For the underdetermination of theory by data in fact presupposes both the observational/theoretical distinction and the comparability of competing theories. Quine certainly did not claim the universality of underdetermination in order to undermine the objectivity of science, only our complacency about what its objectivity consists in. But historians, sociologists and radical interpreters of Kuhn's theory certainly have claimed that underdetermination means that, in science, theory choice is either not rational, or rational only relative to some social, psychological, political or other perspective.

Defenders of the objectivity of science need to show that scientific changes are in fact rational, and not just relative to a point of view. They need to show that the changes in a theory which new data provoke are not just arbitrary, that the acceptance of a new paradigm is not simply a conversion experience, but is justified even by the lights of the superseded paradigm. To do this the philosopher of science must perforce become a historian of science. The philosopher must scrutinize the historical record with at least the care of a Kuhn, to show that beneath the appearances of "madness" which Kuhn and his successor historians catalogued there is a reality of "method". That is, philosophers need to extract from the historical record the principles of reasoning, inference and argument that participants in paradigm shifts and theoretical change actually employed, and then to consider whether these principles can be vindicated as objectivity-preserving ones. This is a task which naturalistic philosophers in particular have set for themselves. They have begun to wrestle with the archives, lab notebooks, correspondence and papers of the scientists engaged in scientific revolutions, great and small, and at the same time kept an eye to what the sciences, especially cognitive science, can tell us about reasoning processes characteristic of humans and the adaptive significance of reasoning for our ability to survive and thrive. As noted above, however, naturalists must at the same time take seriously the charge of begging the question which dogs the attempt to preserve objectivity in the face of the holism of meanings and the want of a clear observational/theoretical distinction.

This charge of question-begging is central to the ways in which opponents of scientific objectivity, progress and cumulation would argue. They would hold that attempts to underwrite the traditional claims of science are not just paradigm-bound, but can be undermined by the very philosophical standards of argument and the substantive philosophical doctrines that defenders of objectivity embrace. If correct, this situation provides a major challenge to those who seek to both understand the nature of science and vindicate its traditional claims. The challenge is nothing less than that which faces philosophy as a whole: to articulate and defend an adequate epistemology, and philosophy of language. And then to show that episodes in the history of the sciences sustain these accounts of what constitutes knowledge and how reference can be secured to the same objects in the world by scientists with profoundly different beliefs

about the world. If the philosophy of science has learned one lesson from Thomas Kuhn it is that it cannot let the analysis of what actually happened in science fall exclusively into the hands of those with a relativistic or skeptical agenda.

Some scientists and exponents of "scientism" will be tempted to turn their backs on these issues. They may well suppose that if people who can't or won't do the hard work to understand science wish to pretend it isn't the best approximation we have to the truth about the world, this is their problem. And if there are people whose wish that there be a reality – religious, spiritual, holistic, metaphysical – that transcends anything the science can know about, leads them to the thought that science is blinkered and partial in its account of the truth, well, who are we scientists to wake them from their dogmatic slumbers? But the stakes for science and for civilization are too high simply to treat those who deny its objectivity in the way we would treat those who claim the earth is flat.

Summary

Sociologists, and others eager to reduce the baleful influence of a blinkered, narrow-minded, patriarchal, capitalist and probably racialist paradigm associated especially with Newtonian science, have adopted Kuhn's view of science as a version of epistemological relativism.

Relativism in epistemology, as in ethics, allows for the possibility of alternative and conflicting views without adjudicating which is objectively correct: none are, or rather each is correct from the perspective of some epistemic point of view, and all points of view have equal standing. So far as the strongest sociological interpretation of Kuhn was concerned, science is moved by social forces, not epistemic considerations. Science is a social institution, like any other; and this is how it is to be approached if we wish to understand it.

If the empiricist criticizes this argument as incoherent, the relativist is indifferent. All the relativist requires is an argument that convinces relativism, not one that is even intelligible to, let alone accepted by, the empiricist. But this is the end of all debate, and in recent years many of the most radical of sociologists of science have given up this degree of relativism.

As is evident from a survey of obvious moves in the attempt to restore the fortunes of an empiricist theory of knowledge and metaphysics as well as an empiricist account of language, easy solutions will not avail, and there is still much work to be done by philosophy if we are to understand fully the nature of science. Our project must include an understanding of categorization and observation, both philosophically and psychologically. We must clarify the relations between meaning and reference, and develop an epistemology adequate to deal with underdetermination or to show that it does not obtain, and the philosophy of science must come more fully to grips with the history of science. These are all tasks for a naturalistic philosophy.

Questions

1 According to Kuhn, to be successful, normal science must be authoritarian. Why does Kuhn make this claim, and does it constitute a moral deficiency of science?
2 Defend or criticize: "Now at last we can see that science is just another religion."
3 Explain why epistemic relativism cannot be asserted to be true. To what degree if any does this limit the force of the doctrine of epistemic relativism.
4 "Poetry is untranslatable. Science is not. Therefore, incommensurability is false." Sketch an argument for this view.
5 Go back to the study questions at the end of Chapter 1 and reconsider your answers to them.

Further reading

The classical text predating Kuhn's influence in the sociology of science is R. K. Merton, *The Sociology of Science.*

Many of the works about Kuhn's books – especially collections of papers – mentioned in the last chapter are of great relevance here. Among the most radical of relativist sociologists of science in the period after 1970 are B. Latour and S. Woolgar, *Laboratory Life;* A. Pickering, *Constructing Quarks;* B. Barnes, *Scientific Knowledge and Social Theory;* and D. Bloor, *Knowledge and Social Imagery.* Bloor and Barnes significantly qualified their views 20 years later in B. Barnes, D. Bloor and J. Henry, *Scientific Knowledge: A Sociological Analysis.*

Important work in the philosophy of science sympathetic to the sociological approach is due to H. Longino, *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry.* Longino has also made contributions to feminist philosophy of science.

A defense of classical empiricist theories of knowledge and language and of a realist metaphysics for science along the lines developed in this chapter are to be found in I. Sheffler, *Science and Subjectivity.* Nagel attacks Feyerabend's version of theoretical incommensurability in *Teleology Revisited,* as does P. Achinstein, *The Concepts of Evidence.* L. Laudan, *Progress and its Problems,* develops a problems-based account of the nature of science which seeks to incorporate substantial evidence from the history of science.