Chapter 5
Scientific change and scientific revolutions

Scientific ideas change fast. Pick virtually any scientific discipline you like, and you can be sure that the prevalent theories in the discipline will be different from those of fifty years ago, and very different from those of 100 years ago. Compared with other areas of intellectual endeavour, science is a rapidly changing activity. A number of interesting philosophical questions centre on the issue of scientific change. Is there a discernible pattern to the way scientific ideas change over time? When scientists abandon their existing theory in favour of a new one, how should we explain this? Are later scientific theories objectively better than earlier ones?

Most modern discussion of these questions takes off from the work of Thomas Kuhn, an American historian and philosopher of science. In 1963 Kuhn published a book called *The Structure of Scientific Revolutions*, which had an enormous influence on subsequent philosophy of science. The impact of Kuhn's ideas has also been felt in academic disciplines such as sociology and anthropology, and in the intellectual culture at large. (*The Guardian* newspaper included *The Structure of Scientific Revolutions* in its list of the 100 most influential books of the 20th century.) To understand why Kuhn's ideas caused such a stir, we need to look briefly at the state of philosophy of science prior to the publication of his book.

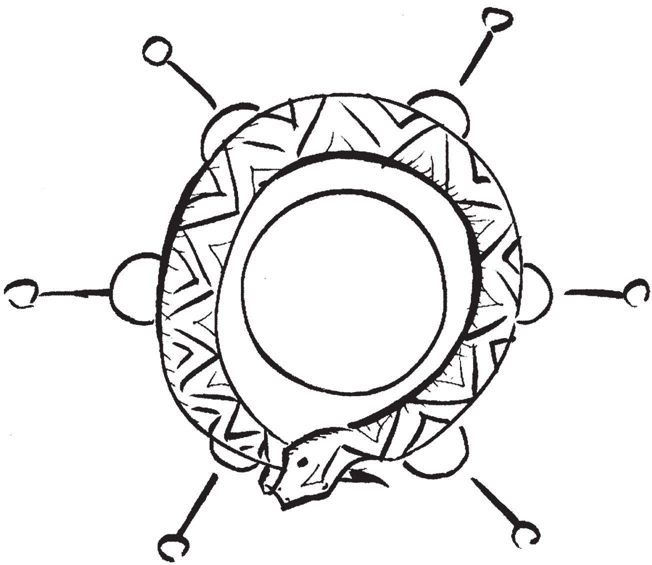
Logical empiricist philosophy of science

The dominant philosophical movement in the English-speaking world in the post-war period was *logical empiricism*. The original logical empiricists were a loosely knit group of philosophers, logicians, and scientists who gathered in Vienna and Berlin in the 1920s and early 1930s. (Carl Hempel, whom we met in Chapter 3, was closely associated with the group, as was Karl Popper.) Fleeing persecution by the Nazis, most of the logical empiricists emigrated to the United States, where they and their followers exerted a powerful influence on academic philosophy until about the mid-1960s, by which time the movement had begun to disintegrate.

The logical empiricists had a high regard for the natural sciences and also for mathematics and logic. The early years of the 20th century witnessed exciting scientific advances, particularly in physics, which impressed them tremendously. One of their aims was to make philosophy itself more 'scientific', in the hope that this would allow similar advances to be made in philosophy. What impressed the logical empiricists about science was its apparent objectivity. Unlike in other fields, where much turned on the subjective opinion of enquirers, scientific questions could be settled in a fully objective way, they believed. Techniques such as experimental testing allowed a scientist to compare their theory directly with the facts, and thus reach an informed, unbiased decision about the theory's merits. Science for the logical empiricists was thus a paradigmatically *rational* activity, the surest route to the truth that there is.

Despite the high esteem in which they held science, the logical empiricists paid little attention to the history of scientific ideas. This was primarily because they drew a sharp distinction between what they called the 'context of discovery' and the 'context of justification'. The context of discovery refers to the actual historical process by which a scientist arrives at a given theory. The context of justification refers to the means by which the scientist tries to justify the theory once they already have it—which includes testing the theory, searching for relevant evidence, and comparing it with rival theories. The logical empiricists believed that the former was a subjective, psychological process which wasn't governed by precise rules, while the latter was an objective matter of logic. Philosophers of science should confine themselves to studying the latter, they argued.

An example can help illustrate this idea. In 1865 the German chemist Kekulé discovered that the benzene molecule has a hexagonal structure. Apparently, he hit on the hypothesis of a hexagonal structure after a dream in which he saw a snake trying to bite its own tail (see Figure 6). Of course, Kekulé then had to



6. Kekulé arrived at the hypothesis of the hexagonal structure of benzene after a dream in which he saw a snake trying to bite its own tail.

test his hypothesis scientifically before it could be accepted. This is an extreme example, but it shows that scientific hypotheses can be arrived at in the most unlikely of ways—they are not always the product of careful, systematic thought. The logical empiricists held that it makes no difference how a hypothesis is arrived at initially. What matters is how it is tested once it is already there—for it is this that makes science a rational activity.

Another theme in logical empiricist philosophy of science was the distinction between theories and observational facts; this is related to the observable/unobservable distinction discussed in Chapter 4. The logical empiricists believed that disputes between rival scientific theories could be solved in a fully objective way—by comparing the theories directly with the 'neutral' observational facts, which all parties could accept. How exactly this set of neutral facts should be characterized was a matter of debate among the logical empiricists, but they were adamant that it existed. Without a clear distinction between theories and observational facts the rationality and objectivity of science would be compromised, and they were resolute in their belief that science was rational and objective.

Kuhn's theory of scientific revolutions

Kuhn was a historian of science by training, and firmly believed that philosophers had much to learn from studying the history of science. Insufficient attention to the history of science had led the logical empiricists to form an inaccurate and naive picture of the scientific enterprise, he maintained. As the title of his book indicates, Kuhn was especially interested in scientific revolutions—periods of great upheaval when existing scientific ideas are replaced with radically new ones. Examples include the Copernican revolution in astronomy, the Einsteinian revolution in physics, and the Darwinian revolution in biology. Each of these revolutions led to a fundamental change in the scientific worldview—the overthrow of an existing set of ideas by a completely different set.

Of course, scientific revolutions happen relatively infrequently—most of the time any given science is not in a state of revolution. Kuhn coined the term 'normal science' to describe the ordinary day-to-day activities that scientists engage in when their discipline is not undergoing revolutionary change. Central to Kuhn's account of normal science is the concept of a *paradigm*. A paradigm consists of two main components: first, a set of fundamental theoretical assumptions which all members of a scientific community accept; secondly, a set of 'exemplars' or particular scientific problems which have been solved by means of those theoretical assumptions, and which appear in the textbooks of the discipline in question. But a paradigm is more than just a theory (though Kuhn sometimes uses the words interchangeably). When scientists share a paradigm they do not just agree on certain scientific propositions, they agree also on how future research in their field should proceed, on which problems are the pertinent ones to tackle, on what the appropriate methods for solving those problems are, and on what an acceptable solution of the problems would look like. In short, a paradigm is an entire scientific outlook—a constellation of shared assumptions, beliefs, and values which unite a scientific community and allow normal science to take place.

What exactly does normal science involve? According to Kuhn it is primarily a matter of *puzzle-solving*. However successful a paradigm is, it will always encounter certain problems—phenomena which it cannot easily accommodate, or mismatches between the theory's predictions and the experimental facts. The job of the normal scientist is to try to eliminate these minor puzzles while making as few changes as possible to the paradigm. So normal science is a conservative activity—its practitioners are not trying to make any earth-shattering discoveries, but rather just to develop and extend the existing paradigm. In Kuhn's words, 'normal science does not aim at novelties of fact or theory, and when successful finds none'. Above all, Kuhn stressed that normal scientists are not trying to *test* the paradigm. On the contrary, they accept the paradigm unquestioningly, and conduct their research within the limits it sets. If a normal scientist gets an experimental result which conflicts with the paradigm, they will usually assume that their experimental technique is faulty, not that the paradigm is wrong.

Typically a period of normal science lasts many decades, sometimes even centuries. During this time scientists gradually articulate the paradigm—fine-tuning it, filling in details, and extending its range of application. But over time *anomalies* are discovered—phenomena which simply cannot be reconciled with the paradigm, however hard scientists try. When anomalies are few they tend to just get ignored. But as anomalies accumulate, a burgeoning sense of crisis envelops the scientific community. Confidence in the existing paradigm breaks down, and the process of normal science grinds to a halt. This marks the beginning of a period of 'revolutionary science' as Kuhn calls it. During such periods, fundamental scientific ideas *are* up for grabs. A variety of alternatives to the old paradigm are proposed, and eventually a new paradigm becomes established. A generation is usually required before all members of the scientific community are won over to the new paradigm—an event which marks the completion of a scientific revolution. The essence of a scientific revolution is thus the shift from an old paradigm to a new one.

Kuhn's characterization of the history of science as long periods of normal science punctuated by occasional scientific revolutions struck a chord with many scholars. A number of examples from the history of science fit Kuhn's description quite well. In the transition from Ptolemaic to Copernican astronomy, for example, or from Newtonian to Einsteinian physics, many of the features that Kuhn describes are present. Ptolemaic astronomers did indeed share a paradigm, based around the theory that the earth is stationary at the centre of the universe, which formed the unquestioned back-drop to their investigations. The same is true of Newtonian physicists in the 18th and 19th centuries, whose paradigm was based around Newton's theory of mechanics and gravitation. And in both cases, Kuhn's account of how an old paradigm gets replaced by a new one applies fairly accurately. There are also scientific revolutions which do not fit the Kuhnian model so neatly—for example the molecular revolution in biology in the 1950s and 1960s. But nonetheless, most people agree that Kuhn's description of the history of science contains much of value.

Why did Kuhn's ideas cause such a storm? Because in addition to his descriptive claims about the history of science, Kuhn advanced some controversial philosophical theses. Ordinarily we assume that when scientists trade their existing theory for a new one, they do so on the basis of evidence. But Kuhn argued that adopting a new paradigm involves a certain act of faith on the part of the scientist. He allowed that a scientist could have good reasons for abandoning an old paradigm for a new one, but he insisted that reasons alone could never rationally *compel* a paradigm shift. 'The transfer of allegiance from paradigm to paradigm', Kuhn wrote, 'is a conversion experience which cannot be forced.' And in explaining why a new paradigm rapidly gains acceptance in the scientific community, Kuhn emphasized the peer pressure of scientists on one another. If a given paradigm has very forceful advocates, it is more likely to win widespread acceptance.

Many of Kuhn's critics were appalled by these claims. For if paradigm shifts work the way Kuhn says, it is hard to see how science can be regarded as a rational activity at all. Surely scientists are meant to base their beliefs on evidence and reason, not on faith and peer pressure? Faced with two competing paradigms, surely the scientist should make an objective comparison of them to determine which has more evidence in its favour? Undergoing a 'conversion experience', or allowing oneself to be persuaded by the most forceful of one's fellow scientists, hardly seems like a rational way to behave. One critic wrote that on Kuhn's account, theory choice in science was 'a matter for mob psychology'.

Kuhn also made some controversial claims about the overall direction of scientific change. According to a widely held view, science progresses towards the truth in a linear fashion, as older incorrect ideas get replaced by newer, correct ones. Later theories are thus objectively better than earlier ones, so scientific knowledge accumulates over time. This linear, cumulative conception of science is popular among laypeople and scientists alike, but Kuhn argued that it is both historically inaccurate and philosophically naive.

For example, he noted that Einstein's theory of relativity is in some respects more similar to Aristotelian than Newtonian physics—so the history of mechanics is not simply a linear progression from wrong to right. Moreover, Kuhn questioned whether the concept of objective truth actually makes sense at all. The idea that there is a fixed set of facts about the world, independent of any particular paradigm, was of dubious coherence, he believed. Kuhn suggested a radical alternative: the facts about the world are paradigm-relative, and thus change when paradigms change. If this suggestion is right, then it makes no sense to ask whether a given theory corresponds to the facts 'as they really are', nor therefore to ask whether it is objectively true. This led Kuhn to espouse a radical form of anti-realism about science.

Incommensurability and the theory-ladenness of data

Kuhn had two main philosophical arguments for these claims. First, he argued that competing paradigms are typically 'incommensurable' with one another. To understand this idea, recall that for Kuhn a scientist's paradigm determines their entire worldview. So when an existing paradigm is replaced by a new one in a scientific revolution, scientists have to abandon the whole conceptual framework which they use to make sense of the world. Indeed Kuhn even claims, somewhat metaphorically, that before and after a paradigm shift scientists 'live in different worlds'.

Incommensurability is the idea that two paradigms may be so different as to render impossible any straightforward comparison of them with each other—there is no common language into which both can be translated. As a result, the proponents of different paradigms 'fail to make complete contact with each other's viewpoints', Kuhn claimed.

This is an interesting if somewhat vague idea. The doctrine of incommensurability stems from Kuhn's belief that scientific concepts derive their meaning from the theory in which they play a role. So to understand Newton's concept of mass, for example, we need to understand the whole of Newtonian theory—concepts cannot be explained independently of the theories in which they are embedded. This idea, which is sometimes called 'holism', was taken very seriously by Kuhn. He argued that the term 'mass' actually meant something different for Newton and Einstein, since the theories in which each embedded the term were so different. This implies that Newton and Einstein were in effect speaking different languages, which obviously complicates the attempt to compare their theories. If a Newtonian and an Einsteinian physicist tried to have a rational discussion, they would end up talking past each other.

Kuhn used the incommensurability thesis both to rebut the view that paradigm shifts are fully 'objective', and to bolster his non-cumulative picture of the history of science. Traditional philosophy of science saw no huge difficulty in choosing between competing theories—one simply makes an objective comparison of them in the light of the available evidence. But this clearly presumes that there is a common language in which both theories can be expressed. If Kuhn is right that proponents of old and new paradigms are quite literally talking past each other, no such simplistic account of paradigm choice can be correct. Incommensurability is equally problematic for the traditional linear picture of scientific history. If old and new paradigms are incommensurable, then it cannot be correct to think of scientific revolutions as the replacement of 'wrong' ideas by 'right' ones. For to call one idea right and another wrong implies the existence of a common framework for evaluating them, which is precisely what Kuhn denies. Incommensurability implies that scientific change, far from being a straightforward progression towards the truth, is in a sense directionless: later paradigms are not better than earlier ones, just different.

Not many philosophers were convinced by Kuhn's incommensurability thesis. Part of the problem was that Kuhn also claimed old and new paradigms to be *incompatible*. This claim is plausible, for if old and new paradigms were not incompatible there would be no need to choose between them. And in many cases the incompatibility is anyway obvious—the Ptolemaic claim that the planets revolve around the earth is obviously incompatible with the Copernican claim that they revolve around the sun. But as Kuhn's critics were quick to point out, if two things are incommensurable then they cannot be incompatible. To see why not, consider the proposition that an object's mass depends on its velocity. Einstein's theory says this proposition is true while Newton's says it is false. But if the doctrine of incommensurability is right, then there is no actual disagreement between Newton and Einstein here, for the proposition means something different for each. Only if the proposition has the *same* meaning in both theories is there a genuine conflict between the two. Since everybody (including Kuhn) agrees that Einstein's and Newton's theories *do* conflict, this is strong reason to regard the incommensurability thesis with suspicion.

In response to this objection, Kuhn moderated his incommensurability thesis somewhat. He argued that partial translation between paradigms could be achieved, so the proponents of old and new paradigms could communicate to some extent: they would not always be talking past each other entirely. But Kuhn continued to maintain that fully objective choice between paradigms was impossible. For in addition to the incommensurability deriving from the lack of a common language, there is also what he called 'incommensurability of standards'. This is the idea that proponents of different paradigms may disagree about what features a good paradigm should have, what problems it should be able to solve, and what an acceptable solution to those problems would look like. So even if they can communicate effectively, they will not be able to reach agreement about whose paradigm is superior. In Kuhn's words, 'each paradigm will be shown to satisfy the criteria that it dictates for itself and to fall short of a few of those dictated by its opponent'.

Kuhn's second philosophical argument was based on an idea known as the 'theory-ladenness' of data. To grasp this idea, suppose you are a scientist trying to choose between two conflicting theories. The obvious thing to do is to look for a piece of data which will decide between them, or to perform a 'crucial experiment' that will settle the matter. But this will only be possible if there exist data which are suitably independent of the theories, in the sense that a scientist could accept the data whichever of the two theories they believed. As we have seen, the logical empiricists believed in the existence of such theory-neutral data, which could provide an objective court of appeal between competing theories. But Kuhn argued that the ideal of theory-neutrality is an illusion—data are invariably contaminated by theoretical assumptions. It is impossible to isolate a set of 'pure' data which all scientists would accept irrespective of their theoretical persuasion, he argued.

The theory-ladenness of data had two important consequences for Kuhn. First, it meant that a dispute between competing paradigms could not be resolved by simply appealing to 'the data' or 'the facts', for what a scientist counts as data, or facts, will depend on which paradigm they accept. Perfectly objective choice between two paradigms is therefore impossible: there is no neutral vantage-point from which to assess the claims of each.

Secondly, the very idea of objective truth is called into question. To be objectively true, a theory must correspond to the facts, but the idea of such a correspondence makes little sense if the facts themselves are infected by our theories. This is why Kuhn was led to the radical view that truth itself is relative to a paradigm.

Why did Kuhn think that all data are theory-laden? His writings are not totally clear on this point, but at least two lines of argument are discernible. The first is the idea that perception is heavily conditioned by background beliefs—what we see depends in part on what we believe. So a trained scientist looking at a sophisticated piece of apparatus in a laboratory will see something different from what a layperson sees, for the scientist obviously has many beliefs about the apparatus that the layperson lacks. There are a number of psychological experiments which apparently show that perception is sensitive in this way to background belief—though the correct interpretation of these experiments is a contentious matter. Secondly, scientists' experimental and observational reports are often couched in highly theoretical language. For example, a scientist might report the outcome of an experiment by saying 'an electric current is flowing through the copper rod'. But this data report is obviously laden with a large amount of theory. It would not be accepted by a scientist who did not hold standard beliefs about electric currents, so it is clearly not theory-neutral.

Philosophers are divided over the merits of these arguments. On the one hand, many agree with Kuhn that pure theory-neutrality is unattainable. The logical empiricist ideal of a class of data statements totally free of theoretical commitment is rejected by most contemporary philosophers—not least because no one has succeeded in saying what such statements would look like. But this need not compromise the objectivity of paradigm shifts altogether. Suppose that a Ptolemaic and a Copernican astronomer are engaged in a debate about whose theory is superior. In order for them to debate meaningfully, there needs to be some astronomical data they can agree on. But why should this be a problem? Surely they can agree about the relative position of the earth and the moon on successive nights, for example, or the time at which the sun rises? Obviously, if the Copernican insists on describing the data in a way that presumes the truth of the heliocentric theory, the Ptolemaist will object. But there is no reason why the Copernican should do that. Statements like 'on May 14th the sun rose at 7.10 am' can be agreed on by a scientist whether they believe the geocentric or the heliocentric theory. Such statements are sufficiently theory-neutral to be acceptable to proponents of both paradigms, which is what matters.

What about Kuhn's rejection of objective truth? Few philosophers have followed Kuhn's lead here. Part of the problem is that, like many who reject the concept of objective truth, Kuhn failed to articulate a viable alternative. The radical view that truth is paradigm-relative is ultimately hard to make sense of. For like all relativist doctrines, it faces a critical problem. Consider the question: is the claim that truth is paradigm-relative *itself* objectively true or not? If the proponent of relativism answers 'yes', then they have admitted that the concept of objective truth does make sense and thus contradicted themselves. If they answer 'no', then they have no grounds on which to argue with someone who disagrees and says that, in their opinion, truth is *not* paradigm-relative. Not all philosophers regard this argument as completely fatal to relativism, but it does suggest that abandoning the concept of objective truth is easier said than done. Kuhn certainly raised some telling objections to the traditional view that the history of science is simply a linear progression to the truth, but the relativist alternative he offered in its place is not easy to accept.

Kuhn and the rationality of science

The Structure of Scientific Revolutions is written in a radical tone. Kuhn gives the impression of wanting to replace standard philosophical ideas about theory change in science with a radically different conception. His doctrines of paradigm shifts, incommensurability, and the theory-ladenness of data seem wholly at odds with the logical empiricist view of science as rational, objective, and cumulative. With some justification, Kuhn's readers took him to be saying that science is a largely non-rational activity, characterized by dogmatic adherence to a paradigm in normal periods, and sudden 'conversion experiences' in revolutionary periods.

But Kuhn himself was unhappy with this interpretation of his work. In a Postscript to the second edition of *Structure* published in 1970, and in subsequent writings, Kuhn moderated his tone considerably, distancing himself from the more radical views that he had seemed to endorse. He was not trying to cast doubt on the rationality of science, he argued, but rather to offer a more realistic, historically accurate picture of how science actually develops. By neglecting the history of science, the logical empiricists had been led to a simplistic account of how science works, and Kuhn's aim was to provide a corrective. He was not trying to show that science was irrational, but rather to provide a better account of what scientific rationality involves.

Some commentators regard Kuhn's Postscript as an about-turn—a retreat from his original position rather than a clarification of it. Whether this is a fair assessment is not a question we will consider here. But the Postscript did bring to light one important issue. In rebutting the charge that he had portrayed science as non-rational, Kuhn made the famous claim that there is 'no algorithm' for theory choice in science. What does this mean? An algorithm is a set of rules which allows us to compute the answer to a particular question. For example, an algorithm for multiplication is a set of rules which when applied to any two numbers, tells us their product. So an algorithm for theory choice is a set of rules which, when applied to two competing theories, would tell us which to choose. Much traditional philosophy of science was committed, implicitly or explicitly, to the existence of such an algorithm. The logical empiricists often wrote as if, given a set of data and two competing theories, the 'principles of scientific method' could be used to determine which theory was superior. This idea was implicit in their belief that although discovery was a matter of psychology, justification was a matter of logic.

Kuhn's insistence that there is no algorithm for theory-choice in science is probably correct. Certainly no one has ever succeeded in producing such an algorithm. Lots of philosophers and scientists have made plausible suggestions about what to look for in theories—simplicity, broadness of scope, close fit with the data, and so on. But these suggestions fall short of providing a true algorithm, as Kuhn knew well. For one thing, there may be trade-offs: theory A may be simpler than theory B, but B may fit the data more closely. So an element of subjective judgement, or scientific common sense, will often be needed to decide between competing theories. Seen in this light, Kuhn's suggestion that the adoption of a new paradigm involves a certain act of faith does not seem quite so radical, and likewise his emphasis on the persuasiveness of a paradigm's advocates in determining its chance of winning over the scientific community.

The 'no algorithm' idea lends support to the view that Kuhn's account of paradigm shifts is not an assault on the rationality of science. For we can read Kuhn instead as rejecting a certain conception of rationality. The logical empiricists believed, in effect, that there *must* be an algorithm for theory-choice on pain of scientific change being irrational. This is not a crazy view: many paradigm cases of rational action do involve rules or algorithms. For example, if you want to decide whether a good is cheaper in England or Japan, you apply an algorithm for converting pounds into yen; any other way of trying to decide the matter is irrational. Similarly, if a scientist is trying to decide between two competing theories, it is tempting to think that the only rational way to proceed is to apply an algorithm for theory-choice. So if it turns out that there is no such algorithm, as seems likely, we have two options. Either we can conclude that scientific change is irrational *or* that the conception of rationality at work is too demanding. In his later writings Kuhn endorses the latter option. The moral of his story is not that paradigm shifts are irrational, but rather that a more relaxed, pragmatic concept of rationality is required to make sense of them.

Kuhn's legacy

Despite their controversial nature, Kuhn's ideas transformed philosophy of science. In part this is because Kuhn called into question many assumptions that had traditionally been taken for granted, forcing philosophers to confront them, and in part because he drew attention to a range of issues that traditional philosophy of science had simply ignored. After Kuhn, the idea that philosophers could afford to ignore the history of science appeared increasingly untenable, as did the idea of a sharp dichotomy between the contexts of discovery and justification. Contemporary philosophers of science pay much greater attention to the historical development of science than did their pre-Kuhnian ancestors. Even those unsympathetic to Kuhn's more radical ideas would accept that in these respects his influence has been positive.

Another important impact of Kuhn's work was to focus attention on the social context in which science takes place, something that traditional philosophy of science ignored. Science for Kuhn is an intrinsically social activity: the existence of a scientific community, bound together by allegiance to a shared paradigm, is a prerequisite for the practice of normal science. Kuhn also paid considerable attention to how science is taught in schools and universities, how young scientists are initiated into the scientific community, how scientific results are published, and other such 'sociological' matters. Not surprisingly, Kuhn's ideas have been influential among sociologists of science. In particular, a movement known as the 'strong programme' in the sociology of science, which emerged in Britain in the 1970s and 1980s, owes much to Kuhn.

The strong programme was based around the idea that science should be viewed as a product of the society in which it is practised. Strong programme sociologists took this idea literally: they held that scientists' beliefs were in large part socially determined. So to explain why a scientist believes a given theory, for example, they would cite aspects of the scientist's social and cultural background. The scientist's own reasons for believing the theory were never explanation enough, they maintained. The strong programme borrowed a number of themes from Kuhn, including the theory-ladenness of data, the view of science as an essentially social activity, and the idea that there is no objective algorithm for theory-choice. But strong programme sociologists were more radical than Kuhn, and less cautious. They openly rejected the notions of truth and rationality, which they regarded as ideologically suspect, and viewed traditional philosophy of science with great suspicion. This led to a certain amount of tension between philosophers and sociologists of science, which continues to this day.

Further afield, Kuhn's work has played a role in the rise of *social constructionism* in the humanities and social sciences. Social constructionism is the idea that certain phenomena, e.g. racial categories, are 'social constructs', as opposed to having an objective mind-independent existence. Given Kuhn's emphasis on the social context of science, and his rejection of the idea that scientific theories 'correspond to the objective facts', it is easy to see why he could be read as saying that science is a 'social construct'. However there is a certain irony here. For proponents of the idea that science is a 'social construct' have typically had an anti-scientific attitude, often objecting to the authority accorded to science in modern society. But Kuhn himself was strongly pro-science. Like the logical empiricists, he regarded modern science as a hugely impressive intellectual achievement. His doctrines of paradigm shifts, normal and revolutionary science, incommensurability, and theory-ladenness were not intended to undermine or criticize the scientific enterprise, but rather to help us understand it better.