theory is about unobservable entities, or about observable but unobserved entities, makes no difference – the logic of the situation is the same in both cases. Of course, showing that the underdetermination argument is just a version of the problem of induction does not mean that it can be ignored. For there is little consensus on how the problem of induction should be tackled, as we saw in Chapter 2. But it does mean that there is no *special* difficulty about unobservable entities. Therefore the anti-realist position is ultimately arbitrary, say the realists. Whatever problems there are in understanding how science can give us knowledge of atoms and electrons are equally problems for understanding how science can give us knowledge of ordinary, medium-sized objects.

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 that discipline will be very different from those of 50 years ago, and extremely different from those of 100 years ago. Compared with other areas of intellectual endeavour such as philosophy and the arts, 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? Or does the concept of objectivity make sense at all?

Most modern discussion of these questions takes off from the work of the late Thomas Kuhn, an American historian and philosopher of science. In 1963 Kuhn published a book called *The Structure of Scientific Revolutions*, unquestionably the most influential work of philosophy of science in the last 50 years. The impact of Kuhn's ideas has also been felt in other academic disciplines such as sociology and anthropology, and in the general 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.) In order 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 positivist philosophy of science

The dominant philosophical movement in the English-speaking world in the post-war period was *logical positivism*. The original logical positivists were a loosely knit group of philosophers and scientists who met in Vienna in the 1920s and early 1930s, under the leadership of Moritz Schlick. (Carl Hempel, whom we met in Chapter 3, was closely associated with the positivists, as was Karl Popper.) Fleeing persecution by the Nazis, most of the positivists 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.

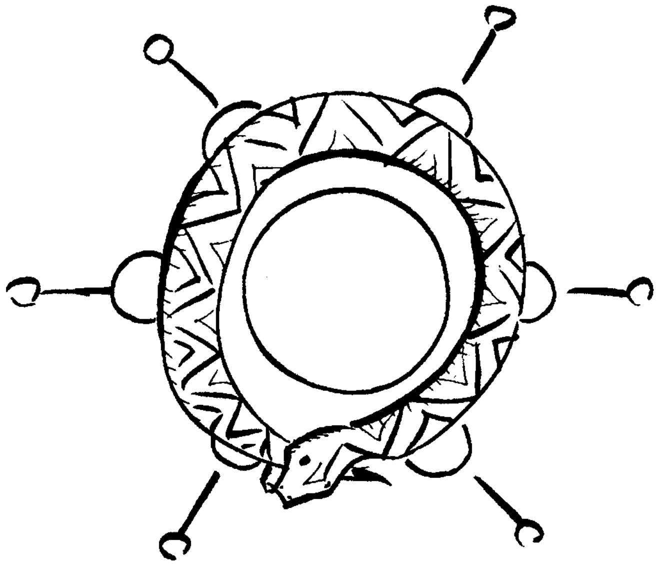
The logical positivists had a very high regard for the natural sciences, and also for mathematics and logic. The early years of the 20th century witnessed exciting scientific advances, particularly in physics, which impressed the positivists tremendously. One of their aims was to make philosophy itself more 'scientific', in the hope that this would allow similar advances to be made in philosophy. What particularly impressed the positivists 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 his theory directly with the facts, and thus reach an informed, unbiased decision about the theory's merits. Science for the positivists 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 positivists paid little attention to the history of science. Indeed, they believed that philosophers had little to learn from studying history of

science. 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 his theory once it is already there – which includes testing the theory, searching for relevant evidence, and so on. The positivists believed that the former was a subjective, psychological process that 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 make this idea clearer. In 1865 the Belgian scientist Kekule discovered that the benzene molecule has a hexagonal structure. Apparently, he hit on the hypothesis of a hexagonal structure for benzene after a dream in which he saw a snake trying to bite its own tail (Figure 11). Of course, Kekule then had to test his hypothesis scientifically, which he did. 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 positivists would argue 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. How Kekule first arrived at his hypothesis was immaterial; what mattered was how he justified it.

This sharp distinction between discovery and justification, and the belief that the former is 'subjective' and 'psychological' while the latter is not, explains why the positivists' approach to philosophy of science was so ahistorical. For the actual historical process by which scientific ideas change and develop lies squarely in the context of discovery, not the context of justification. That process might be of interest to historians or psychologists, but had nothing to teach philosophers of science, according to the positivists.



11. Kekule 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.

Another important theme in positivist philosophy of science was the distinction between theories and observational facts; this is related to the observable/unobservable distinction discussed in the previous chapter. The positivists believed that disputes between rival scientific theories could be solved in a perfectly objective way – by comparing the theories directly with the 'neutral' observational facts, which all parties could accept. The positivists disagreed between themselves about how exactly this set of neutral facts should be characterized, 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 the positivists were resolute in their belief that science was rational and objective.

The structure 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 positivists to form an inaccurate and naïve 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 of scientific revolutions are 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 world-view – 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: firstly, a set of fundamental theoretical assumptions that all members of a scientific community accept at a given time; secondly, a set of 'exemplars' or particular scientific problems that have been solved by means of those theoretical assumptions, and that 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 scientific 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, on what an acceptable solution of the problems would look like, and so on. In short, a paradigm is an entire scientific outlook – a constellation of shared assumptions, beliefs, and values that 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 that it cannot easily accommodate, mismatches between the theory's predictions and the experimental facts, and so on. 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 highly 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 that conflicts with the paradigm, she will usually assume that her experimental technique is faulty, not that the paradigm is wrong. The paradigm itself is not negotiable.

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, solving more and more puzzles, extending its range of application, and so on. But over time *anomalies* are discovered – phenomena that simply cannot be reconciled with the theoretical assumptions of the paradigm, however hard normal scientists try. When anomalies are few in number they tend to just get ignored. But as more and more anomalies accumulate, a burgeoning sense of crisis envelops the scientific community. Confidence in the existing paradigm breaks down, and the process of normal science temporarily 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 or so is usually required before all members of the scientific community are won over to the new paradigm – an event

that 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 philosophers and historians of science. A number of examples from the history of science fit Kuhn's model quite well. When we examine 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 that do not fit the Kuhnian model so neatly – for example the recent molecular revolution in biology. 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 purely descriptive claims about the history of science, Kuhn advanced some highly controversial philosophical theses. Ordinarily we assume that when scientists trade their existing theory for a new one, they do so on the basis of objective 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. Kuhn's account of paradigm shifts seems hard to reconcile with the familiar positivist image of science as an objective, rational activity. 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. This 'cumulative' conception of science is popular among laymen and scientists alike, but Kuhn argued that it is both historically inaccurate and philosophically naïve. For example, he noted that Einstein's theory of relativity is in some respects more similar to Aristotelian than Newtonian theory – 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. Truth itself becomes relative to a paradigm.

Incommensurability and the theory-ladenness of data

Kuhn had two main philosophical arguments for these claims. Firstly, he argued that competing paradigms are typically 'incommensurable' with one another. To understand this idea, we must remember that for Kuhn a scientist's paradigm determines her entire world-view – she views everything through the paradigm's lens. 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, obviously 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 largely 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 choose between 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 – you simply make an objective comparison of them, in the light of the available evidence, and decide which is better. 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 very 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 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, i.e. only if there is no incommensurability, is there a genuine conflict between the two. Since everybody (including Kuhn) agrees that Einstein's and Newton's theories do conflict, that is strong reason to regard the incommensurability thesis with suspicion.

In response to objections of this type, Kuhn moderated his incommensurability thesis somewhat. He insisted that even if two paradigms were incommensurable, that did not mean it was impossible to compare them with each other; it only made comparison more difficult. *Partial* translation between different paradigms could be achieved, Kuhn argued, 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 the standards for evaluating paradigms, about which problems a good paradigm should solve, about what an acceptable solution to those problems would look like, and so on. 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 that will decide between the two – which is just what traditional philosophy of science recommended. But this will only be possible if there exist data that are suitably independent of the theories, in the sense that a scientist would accept the data whichever of the two theories she believed. As we have seen, the logical positivists 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.

The theory-ladenness of data had two important consequences for Kuhn. Firstly, it meant that the issue 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 she accepts. 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. For to be objectively true, our theories or beliefs 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 layman sees, for the scientist obviously has many beliefs about the apparatus that the layman lacks. There are a number of psychological experiments that supposedly 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 an unattainable ideal. The positivists' idea 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 it is not clear that this compromises the objectivity of paradigm shifts altogether. Suppose, for example, 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 such as 'on May 14th the sun rose at 7.10 a.m.' can be agreed on by a scientist whether they believe the geocentric or the heliocentric theory. Such statements may not be *totally* theory-neutral, but they are sufficiently free of theoretical contamination to be acceptable to proponents of both paradigms, which is what matters.

It is even less obvious that the theory-ladenness of data forces us to abandon the concept of objective truth. Many philosophers would accept that theory-ladenness makes it hard to see how *knowledge* of objective truth is possible, but that is not to say that the very concept is incoherent. Part of the problem is that, like many people who are suspicious of 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 such 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 have 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 far from unproblematic.

Kuhn and the rationality of science

The Structure of Scientific Revolutions is written in a very radical tone. Kuhn gives every impression of wanting to replace standard philosophical ideas about theory change in science with a totally new conception. His doctrine of paradigm shifts, of incommensurability, and of the theory-ladenness of data seems wholly at odds with the positivist view of science as a rational, objective, and cumulative enterprise. With much justification, most of Kuhn's early readers took him to be saying that science is an entirely non-rational activity, one 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 *The Structure of Scientific Revolutions* published in 1970, and in subsequent writings, Kuhn moderated his tone considerably – and accused some of his early readers of having misread his intentions. His book was not an attempt 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 positivists had been led to an excessively simplistic, indeed idealistic, account of how science works, and Kuhn's aim was simply 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 simply 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 go into here. But the Postscript did bring to light one important issue. In rebutting the charge that he had portrayed paradigm shifts 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 of a set of rules that allows us to compute the answer to a particular question. For example, an algorithm for multiplication is a set of rules that when applied to any two numbers tells us their product. (When you learn arithmetic in primary school, you in effect learn algorithms for addition, subtraction, multiplication, and division.) So an algorithm for theory choice is a set of rules that when applied to two competing theories would tell us which we should choose. Much positivist philosophy of science was in effect committed to the existence of such an algorithm. The positivists 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 almost certainly correct. For 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 far short of providing a true algorithm, as Kuhn knew well. For one thing, there may be trade-offs: theory one may be simpler than theory two, but theory two 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 thesis that there is no algorithm for theory choice 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 positivists believed, in effect, that there *must* be an algorithm for theory choice on pain of scientific change being irrational. This is by no means 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 positivist conception of rationality is too demanding. In the Postscript Kuhn suggests that the latter is the correct reading of his work. The moral of his story is not that paradigm shifts are irrational, but rather that a more relaxed, non-algorithmic 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 pre-requisite 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 very 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, owed 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 very 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 enterprise, and the idea that there is no algorithm for theory choice. But strong programme sociologists were more radical than Kuhn, and less cautious. They openly rejected the notions of objective 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 *cultural relativism* in the humanities and social sciences. Cultural relativism is not a precisely defined doctrine, but the central idea is that there is no such thing as absolute truth – truth is always relative to a particular culture. We may think that Western science reveals the truth about the world, but cultural relativists would say that other cultures and societies, for example indigenous Americans, have their own truth. As we have seen, Kuhn did indeed embrace relativist ideas. However, there is actually a certain irony in his having influenced cultural relativism. For cultural relativists are normally very anti-science. They object to the exalted status that science is accorded in our society, arguing that it discriminates against alternative belief systems that are equally valuable. But Kuhn himself was strongly pro-science. Like the positivists, he regarded modern science as a hugely impressive intellectual achievement. His doctrine of paradigm shifts, of normal and revolutionary science, of incommensurability and of theory-ladenness was not intended to undermine or criticize the scientific enterprise, but rather to help us understand it better.

Chapter 6
Philosophical problems in physics, biology, and psychology

The issues we have studied so far – induction, explanation, realism, and scientific change – belong to what is called 'general philosophy of science'. These issues concern the nature of scientific investigation in general, rather than pertaining specifically to chemistry, say, or geology. However, there are also many interesting philosophical questions that are specific to particular sciences – they belong to what is called 'philosophy of the special sciences'. These questions usually depend partly on philosophical considerations and partly on empirical facts, which is what makes them so interesting. In this chapter we examine three such questions, one each from physics, biology, and psychology.

Leibniz versus Newton on absolute space

Our first topic is a debate between Gottfried Leibniz (1646–1716) and Isaac Newton (1642–1727), two of the outstanding scientific intellects of the 17th century, concerning the nature of space and time. We shall focus primarily on space, but the issues about time are closely parallel. In his famous *Principles of Natural Philosophy*, Newton defended what is called an 'absolutist' conception of space. According to this view, space has an 'absolute' existence over and above the spatial relations between objects. Newton thought of space as a three-dimensional container into which God had