What is this thing called Science?

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third edition

Open University Press

Open University Press
McGraw-Hill Education
McGraw-Hill House
Shoppenhangers Road
Maidenhead
Berkshire
SL6 2QL
United Kingdom

email: enquiries@openup.co.uk
world wide web: www.openup.co.uk

and
Two Penn Plaza
New York, NY 10121-2289, USA

First published in 1978 by Open University Press
Reprinted 1980

Second edition 1982
Reprinted 1983, 1985, 1986, 1987, 1988, 1990 (twice), 1992

Third edition 1999
Reprinted 2000, 2002, 2003 (twice), 2004, 2005 (twice), 2006, 2007 (twice)

A catalogue record for this book is available from the British Library.

ISBN-10: 0 335 20109 1 (pbk)
ISBN-13: 978 0 335 20109 9

Printed in Great Britain by Bell & Bain Ltd., Glasgow

"Like all young men I set out to be a
genius, but mercifully laughter intervened."

Clea Lawrence Durrell

This is guaranteed by the principle of induction which is presumed to form the basis of science.

Attractive as it may have appeared, we have seen that the inductivist position is, at best, in need of severe qualification and, at worst, thoroughly inadequate. We have seen that facts adequate for science are by no means straightforwardly given but have to be practically constructed, are in some important senses dependent on the knowledge that they presuppose, a complication overlooked in the schematisation in figure 2, and are subject to improvement and replacement. More seriously, we have been unable to give a precise specification of induction in a way that will help distinguish a justifiable generalisation from the facts from a hasty or rash one, a formidable task given nature's capacity to surprise, epitomised in the discovery that supercooled liquids can flow uphill.

In chapter 12 we will discuss some recent attempts to rescue the inductivist account of science from its difficulties. Meanwhile, we will turn in the next two chapters to a philosopher who attempts to sidestep problems with induction by putting forward a view of science that does not involve induction.

Further reading

The historical source of Hume's problem of induction is Hume's *Treatise on Human Nature* (1939, Part 3). Another classic discussion of the problem is Russell (1912, chapter 6). A thorough, technical investigation of the consequences of Hume's argument is Stove (1973). Karl Popper's claim to have solved the problem of induction is in Popper (1979, chapter 1). Reasonably accessible accounts of inductive reasoning can be found in Hempel (1966) and Salmon (1966), and a more detailed treatment is found in Glymour (1980). See also Lakatos (1968) for a collection of essays, including a provocative survey by Lakatos himself, of attempts to construct an inductive logic.

CHAPTER 5

Introducing falsificationism

Introduction

Karl Popper was the most forceful advocate of an alternative to inductivism which I will refer to as "falsificationism". Popper was educated in Vienna in the 1920s, at a time when logical positivism was being articulated by a group of philosophers who became known as the Vienna Circle. One of the most famous of these was Rudolph Carnap, and the clash and debate between his supporters and those of Popper was to be a feature of philosophy of science up until the 1960s. Popper himself tells the story of how he became disenchanted with the idea that science is special because it can be derived from the facts, the more facts the better. He became suspicious of the way in which he saw Freudians and Marxists supporting their theories by interpreting a wide range of instances, of human behaviour or historical change respectively, in terms of their theory and claiming them to be supported on this account. It seemed to Popper that these theories could never go wrong because they were sufficiently flexible to accommodate any instances of human behaviour or historical change as compatible with their theory. Consequently, although giving the appearance of being powerful theories confirmed by a wide range of facts, they could in fact explain nothing because they could rule out nothing. Popper compared this with a famous test of Einstein's theory of general relativity carried out by Eddington in 1919. Einstein's theory had the implication that rays of light should bend as they pass close to massive objects such as the sun. As a consequence, a star situated beyond the sun should appear displaced from the direction in which it would be observed in the absence of this bending. Eddington sought for this displacement by sighting the star at a time when the light from the sun was blocked out by an eclipse. It transpired that the displacement was observed and Einstein's theory was borne out. But Popper makes the point that it might not have been. By making a specific, testable prediction the general theory of relativity was at risk. It ruled out observations that clashed with that prediction. Popper drew the moral that genuine scientific theories, by making definite predictions, rule out a range of observable states of affairs in a way that he considered Freudian and Marxist theory failed to do. He arrived at his key idea that scientific theories are *falsifiable*.

Falsificationists freely admit that observation is guided by and presupposes theory. They are also happy to abandon any claim implying that theories can be established as true or probably true in the light of observational evidence. Theories are construed as speculative and tentative conjectures or guesses freely created by the human intellect in an attempt to overcome problems encountered by previous theories to give an adequate account of some aspects of the world or universe. Once proposed, speculative theories are to be rigorously and ruthlessly tested by observation and experiment. Theories that fail to stand up to observational and experimental tests must be eliminated and replaced by further speculative conjectures. Science progresses by trial and error, by conjectures and refutations. Only the fittest theories survive. Although it can never be legitimately said of a theory that it is true, it can hopefully be said that it is the best available; that it is better than anything that has come before. No problems about the characterisation and justification of induction arise for the falsificationists because, according to them, science does not involve induction.

The content of this condensed summary of falsificationism will be filled out in the next two chapters.

A logical point in favour of falsificationism

According to falsificationism, some theories can be shown to be false by an appeal to the results of observation and

experiment. There is a simple, logical point that seems to support the falsificationist here. I have already indicated in chapter 4 that, even if we assume that true observational statements are available to us in some way, it is never possible to arrive at universal laws and theories by logical deductions on that basis alone. However, it is possible to perform logical deductions starting from singular observation statements as premises, to arrive at the falsity of universal laws and theories by logical deduction. For example, if we are given the statement, "A raven which was not black was observed at place x at time t", then it logically follows from this that "All ravens are black" is false. That is, the argument:

Premise	A raven, which was not black, was at place x at time t.
Conclusion	Not all ravens are black.

is a logically valid deduction. If the premise is asserted and the conclusion denied, a contradiction is involved. One or two more examples will help illustrate this fairly trivial logical point. If it can be established by observation in some test experiment that a ten-kilogram weight and a one-kilogram weight in free fall move downwards at roughly the same speed, then it can be concluded that the claim that bodies fall at speeds proportional to their weight is false. If it can be demonstrated beyond doubt that a ray of light passing close to the sun is deflected in a curved path, then it is not the case that light necessarily travels in straight lines.

The falsity of universal statements can be deduced from suitable singular statements. The falsificationist exploits this logical point to the full.

Falsifiability as a criterion for theories

The falsificationist sees science as a set of hypotheses that are tentatively proposed with the aim of accurately describing or accounting for the behaviour of some aspect of the world or universe. However, not any hypothesis will do. There is one fundamental condition that any hypothesis or system of hypotheses must satisfy if it is to be granted the status of a scientific law or theory. If it is to form part of science, an hypothesis must be *falsifiable*. Before proceeding any further, it is important to be clear about the falsificationist's usage of the term "falsifiable".

Here are some examples of some simple assertions that are falsifiable in the sense intended.

1. It never rains on Wednesdays.
2. All substances expand when heated.
3. Heavy objects such as a brick when released near the surface of the earth fall straight downwards if not impeded.
4. When a ray of light is reflected from a plane mirror, the angle of incidence is equal to the angle of reflection.

Assertion 1 is falsifiable because it can be falsified by observing rain to fall on a Wednesday. Assertion 2 is falsifiable. It can be falsified by an observation statement to the effect that some substance, x, did not expand when heated at time t. Water near its freezing point would serve to falsify 2. Both 1 and 2 are falsifiable and false. Assertions 3 and 4 may be true, for all I know. Nevertheless, they are falsifiable in the sense intended. It is logically possible that the next brick to be relased will "fall" upwards. No logical contradiction is involved in the assertion, "The brick fell upwards when released", although it may be that no such statement is ever supported by observation. Assertion 4 is falsifiable because a ray of light incident on a mirror at some oblique angle could conceivably be reflected in a direction perpendicular to the mirror. This will never happen if the law of reflection happens to be true, but no logical contradiction would be involved if it did. Both 3 and 4 are falsifiable, even though they may be true.

An hypothesis is falsifiable if there exists a logically possible observation statement or set of observation statements that are inconsistent with it, that is, which, if established as true, would falsify the hypothesis.

Here are some examples of statements that do not satisfy this requirement and that are consequently not falsifiable.

5. Either it is raining or it is not raining.
6. All points on a Euclidean circle are equidistant from the centre.
7. Luck is possible in sporting speculation.

No logically possible observation statement could refute 5. It is true whatever the weather is like. Assertion 6 is necessarily true because of the definition of a Euclidean circle. If points on a circle were not equidistant from some fixed point, then that figure would just not be a Euclidean circle. "All bachelors are unmarried" is unfalsifiable for a similar reason. Assertion 7 is quoted from a horoscope in a newspaper. It typifies the fortune-teller's devious strategy. The assertion is unfalsifiable. It amounts to telling the reader that if he has a bet today he might win, which remains true whether he bets or not, and if he does, whether he wins or not.

Falsificationists demand that scientific hypotheses be falsifiable, in the sense discussed. They insist on this because it is only by ruling out a set of logically possible observation statements that a law or theory is informative. If a statement is unfalsifiable, then the world can have any properties whatsoever, and can behave in any way whatsoever, without conflicting with the statement. Statements 5, 6 and 7, unlike statements 1, 2, 3 and 4, tell us nothing about the world. A scientific law or theory should ideally give us some information about how the world does in fact behave, thereby ruling out ways in which it could (logically) possibly behave but in fact does not. The law "All planets move in ellipses around the sun" is scientific because it claims that planets in fact move in ellipses and rules out orbits that are square or oval. Just because the law makes definite claims about planetary orbits, it has informative content and is falsifiable.

A cursory glance at some laws that might be regarded as typical components of scientific theories indicates that they satisfy the falsifiability criterion. "Unlike magnetic poles attract each other", "An acid added to a base yields a salt plus water" and similar laws can easily be construed as falsifiable. However, the falsificationist maintains that some theories, while they may superficially appear to have the characteristics of good scientific theories, are in fact only posing as scientific theories because they are not falsifiable and should be rejected. Popper has claimed that some versions at least of Marx's theory of history, Freudian psychoanalysis and Adlerian psychology suffer from this fault. The point can be illustrated by the following caricature of Adlerian psychology.

A fundamental tenet of Adler's theory is that human actions are motivated by feelings of inferiority of some kind. In our caricature, this is supported by the following incident. A man is standing on the bank of a treacherous river at the instant a child falls into the river nearby. The man will either leap into the river in an attempt to save the child or he will not. If he does leap in, the Adlerian responds by indicating how this supports his theory. The man obviously needed to overcome his feeling of inferiority by demonstrating that he was brave enough to leap into the river, in spite of the danger. If the man does not leap in, the Adlerian can again claim support for his theory. The man was overcoming his feelings of inferiority by demonstrating that he had the strength of will to remain on the bank, unperturbed, while the child drowned.

If this caricature is typical of the way in which Adlerian theory operates, then the theory is not falsifiable. It is consistent with any kind of human behaviour, and just because of that, it tells us nothing about human behaviour. Of course, before Adler's theory can be rejected on these grounds, it would be necessary to investigate the details of the theory rather than a caricature. But there are plenty of social, psychological and religious theories that give rise to the suspicion that in their concern to explain everything they explain nothing. The existence of a loving God and the occurrence of some disaster can be made compatible by interpreting the disaster as being sent to try us or to punish us,

whichever seems most suited to the situation. Many examples of animal behaviour can be seen as evidence supporting the assertion, "Animals are designed so as best to fulfil the function for which they were intended". Theorists operating in this way are guilty of the fortune-teller's evasion and are subject to the falsificationist's criticism. If a theory is to have informative content, it must run the risk of being falsified.

Degree of falsifiability, clarity and precision

A good scientific law or theory is falsifiable just because it makes definite claims about the world. For the falsificationist, it follows fairly readily from this that the more falsifiable a theory is the better, in some loose sense of more. The more a theory claims, the more potential opportunities there will be for showing that the world does not in fact behave in the way laid down by the theory. A very good theory will be one that makes very wide-ranging claims about the world, and which is consequently highly falsifiable, and is one that resists falsification whenever it is put to the test.

The point can be illustrated by means of a trivial example. Consider these laws:

(a) Mars moves in an ellipse around the sun.
(b) All planets move in ellipses around their sun.

I take it that it is clear that (b) has a higher status than (a) as a piece of scientific knowledge. Law (b) tells us all that (a) tells us and more besides. Law (b), the preferable law, is more falsifiable than (a). If observations of Mars should turn out to falsify (a), then they would falsify (b) also. Any falsification of (a) will be a falsification of (b), but the reverse is not the case. Observation statements referring to the orbits of Venus, Jupiter, etc. that might conceivably falsify (b) are irrelevant to (a). If we follow Popper and refer to those sets of observation statements that would serve to falsify a law or theory as *potential falsifiers* of that law or theory, then we can say that the potential falsifiers of (a) form a class that is a subclass of the potential falsifiers of (b). Law (b) is more falsifiable than law (a), which is tantamount to saying that it claims more, that it is the better law.

A less-contrived example involves the relation between Kepler's theory of the solar system and Newton's. Kepler's theory I take to be his three laws of planetary motion. Potential falsifiers of that theory consist of sets of statements referring to planetary positions relative to the sun at specified times. Newton's theory, a better theory that superseded Kepler's, is more comprehensive. It consists of Newton's laws of motion plus his law of gravitation, the latter asserting that all pairs of bodies in the universe attract each other with a force that varies inversely as the square of their separation. Some of the potential falsifiers of Newton's theory are sets of statements of planetary positions at specified times. But there are many others, including those referring to the behaviour of falling bodies and pendulums, the correlation between the tides and the locations of the sun and moon, and so on. There are many more opportunities for falsifying Newton's theory than for falsifying Kepler's theory. And yet, so the falsificationist story goes, Newton's theory was able to resist attempted falsifications, thereby establishing its superiority over Kepler's.

Highly falsifiable theories should be preferred to less falsifiable ones, then, provided they have not in fact been falsified. The qualification is important for the falsificationist. Theories that have been falsified must be ruthlessly rejected. The enterprise of science involves the proposal of highly falsifiable hypotheses, followed by deliberate and tenacious attempts to falsify them. To quote Popper (1969, p. 231, italics in original):

> I can therefore gladly admit that falsificationists like myself much prefer an attempt to solve an interesting problem by a bold conjecture, *even (and especially) if it soon turns out to be false*, to any recital of a sequence of irrelevant truisms. We prefer this because we believe that this is the way in which we can learn from our mistakes; and that in finding that our conjecture was false we shall have learnt much about the truth, and shall have got nearer to the truth.

We learn from our *mistakes*. Science progresses by trial and *error*. Because of the logical situation that renders the derivation of universal laws and theories from observation statements impossible, but the deduction of their falsity possible, *falsifications* become the important landmarks, the striking achievements, the major growing-points in science. This somewhat counter-intuitive emphasis of the more extreme falsificationists on the significance of falsifications will be criticised in later chapters.

Because science aims at theories with a large informative content, the falsificationist welcomes the proposal of bold speculative conjectures. Rash speculations are to be encouraged, provided they are falsifiable and provided they are rejected when falsified. This do-or-die attitude clashes with the caution advocated by the extreme inductivist. According to the latter, only those theories that can be shown to be true or probably true are to be admitted into science. We should proceed beyond the immediate results of experience only so far as legitimate inductions will take us. The falsificationist, by contrast, recognises the limitation of induction and the subservience of observation to theory. Nature's secrets can only be revealed with the aid of ingenious and penetrating theories. The greater the number of conjectured theories that are confronted by the realities of the world, and the more speculative those conjectures are, the greater will be the chances of major advances in science. There is no danger in the proliferation of speculative theories because any that are inadequate as descriptions of the world can be ruthlessly eliminated as the result of observational or other tests.

The demand that theories should be highly falsifiable has the attractive consequence that theories should be clearly stated and precise. If a theory is so vaguely stated that it is not clear exactly what it is claiming, then when tested by observation or experiment it can always be interpreted so as to be consistent with the results of those tests. In this way, it can be defended against falsifications. For example, Goethe (1970, p. 295) wrote of electricity that:

> it is a nothing, a zero, a mere point, which, however, dwells in all apparent existences, and at the same time is the point of origin whence, on the slightest stimulus, a double appearance presents itself, an appearence which only manifests itself to vanish. The conditions under which this manifestation is excited are infinitely varied, according to the nature of particular bodies.

If we take this quotation at face value, it is very difficult to see what possible set of physical circumstances could serve to falsify it. Just because it is so vague and indefinite (at least when taken out of context), it is unfalsifiable. Politicians and fortune-tellers can avoid being accused of making mistakes by making their assertions so vague that they can always be construed as compatible with whatever may eventuate. The demand for a high degree of falsifiability rules out such manoeuvres. The falsificationist demands that theories be stated with sufficient clarity to run the risk of falsification.

A similar situation exists with respect to precision. The more precisely a theory is formulated the more falsifiable it becomes. If we accept that the more falsifiable a theory is the better (provided it has not been falsified), then we must also accept that the more precise the claims of a theory are the better. "Planets move in ellipses around the sun" is more precise than "Planets move in closed loops around the sun", and is consequently more falsifiable. An oval orbit would falsify the first but not the second, whereas any orbit that falsifies the second will also falsify the first. The falsificationist is committed to preferring the first. Similarly, the falsificationist must prefer the claim that the velocity of light in a vacuum is 299.8×10^6 metres per second to the less-precise claim that it is about 300×10^6 metres per second, just because the first is more falsifiable than the second.

The closely associated demands for precision and clarity of expression both follow naturally from the falsificationist's account of science.

Falsificationism and progress

The progress of science as the falsificationist sees it might be summed up as follows. Science starts with problems, problems associated with the explanation of the behaviour of some aspects of the world or universe. Falsifiable hypotheses are proposed by scientists as solutions to a problem. The conjectured hypotheses are then criticised and tested. Some will be quickly eliminated. Others might prove more successful. These must be subject to even more stringent criticism and testing. When an hypothesis that has successfully withstood a wide range of rigorous tests is eventually falsified, a new problem, hopefully far removed from the original solved problem, has emerged. This new problem calls for the invention of new hypotheses, followed by renewed criticism and testing. And so the process continues indefinitely. It can never be said of a theory that it is true, however well it has withstood rigorous tests, but it can hopefully be said that a current theory is superior to its predecessors in the sense that it is able to withstand tests that falsified those predecessors.

Before we look at some examples to illustrate this falsificationist conception of the progress of science, a word should be said about the claim that "Science starts with problems". Here are some problems that have confronted scientists in the past. How are bats able to fly so dexterously at night, when in fact they have very small, weak eyes? Why is the height of a simple barometer lower at high altitudes than at low altitudes? Why were the photographic plates in Roentgen's laboratory continually becoming blackened? Why does the perihelion of the planet Mercury advance? These problems arise from more or less straightforward *observations*. In insisting on the fact that science starts with problems, then, is it not the case that, for the falsificationist just as for the naive inductivist, science starts from observation? The answer to this question is a firm "No". The observations cited above as constituting problems are only problematic *in the light of some theory*. The first is problematic in the light of the theory that living organisms "see" with their eyes; the second was problematic for the supporters of Galileo's theories because it clashed with the "force of a vacuum" theory accepted by them as an explanation of why the mercury does not fall from a barometer tube; the third was problematic for Roentgen because it was tacitly assumed at the time that no radiation or emanation of any kind existed that could penetrate the container of the photographic plates and darken them; the fourth was problematic because it was incompatible with Newton's theory. The claim that science starts with problems is perfectly compatible with the priority of theories over observation and observation statements. Science does not start with stark observation.

After this digression, we return to the falsificationist conception of the progress of science as the progression from problems to speculative hypotheses, to their criticism and eventual falsification and thence to new problems. Two examples will be offered, the first a simple one concerning the flight of bats, the second a more ambitious one concerning the progress of physics.

We start with a problem. Bats are able to fly with ease and at speed, avoiding the branches of trees, telegraph wires, other bats, etc., and can catch insects. And yet bats have weak eyes, and in any case do most of their flying at night. This poses a problem because it apparently falsifies the plausible theory that animals, like humans, see with their eyes. A falsificationist will attempt to solve the problem by making a conjecture or hypothesis. Perhaps he suggests that, although bats' eyes are apparently weak, nevertheless in some way that is not understood they are able to see efficiently at night by use of their eyes. This hypothesis can be tested. A sample of bats is released into a darkened room containing obstacles and their ability to avoid the obstacles measured in some way. The same bats are now blindfolded and again released into the room. Prior to the experiment, the experimenter can make the following deduction. One premise of the deduction is his hypothesis, which made quite explicit reads, "Bats are able to fly avoiding obstacles by using their eyes, and cannot do so without the use of their eyes". The second premise is a description of the experimental set-up, including the statement, "This sample of bats is blindfolded so that they do not have the use of their eyes". From these two premises, the experimenter can derive, deductively, that the sample of bats will not be able to avoid the obstacles in the test laboratory efficiently. The experiment is now performed and it is found that the bats avoid collisions just as efficiently as before. The hypothesis has been falsified. There is now a need for a fresh use of the imagination, a new conjecture or hypothesis or guess. Perhaps a scientist suggests that in some way the bat's ears are involved in its ability to avoid obstacles. The hypothesis can be tested, in an attempt to falsify it, by plugging the ears of bats before releasing them into the test laboratory. This time it is found that the ability of the bats to avoid obstacles is considerably impaired. The hypothesis has been supported. The falsificationist must now try to make the hypothesis more precise so that it becomes more readily falsifiable. It is suggested that the bat hears echoes of its own squeaks rebounding from solid objects. This is tested by gagging the bats before releasing them. Again the bats collide with obstacles and again the hypothesis is supported. The falsificationist now appears to be reaching a tentative solution to the problem, although it has not been *proved* by experiment how bats avoid collisions while flying. Any number of factors may turn up that show the hypothesis to have been wrong. Perhaps the bat detects echoes not with its ears but with sensitive regions close to the ears, the functioning of which was impaired when the bat's ears were plugged. Or perhaps different kinds of bats detect obstacles in very different ways, so the bats used in the experiment were not truly representative.

The progress of physics from Aristotle through Newton to Einstein provides an example on a larger scale. The falsificationist account of that progression goes something like this. Aristotelian physics was to some extent quite successful. It could explain a wide range of phenomena. It could explain why heavy objects fall to the ground (seeking their natural place at the centre of the universe), it could explain the action of siphons and liftpumps (the explanation being based on the impossibility of a vacuum), and so on. But eventually Aristotelian physics was falsified in a number of ways. Stones dropped from the top of the mast of a uniformly moving ship fell to the deck at the foot of the mast and not some distance from the mast, as Aristotle's theory predicted. The moons of Jupiter can be seen to orbit Jupiter and not the earth. A host of other falsifications were accumulated during the seventeenth century. Newton's physics, however, once it had been created and developed by way of the conjectures of the likes of Galileo and Newton, was a superior theory that superseded Aristotle's. Newton's theory could account for falling objects, the operation of siphons and liftpumps and anything else that Aristotle's theory could explain, and could also account for the phenomena that were problematic for the Aristotelians. In addition, Newton's theory could explain phenomena not touched on by Aristotle's theory, such as correlations between the tides and the location of the moon, and the variation in the force of gravity with height above sea level. For two centuries Newton's theory was successful. That is, attempts to falsify it by reference to the new phenomena predicted with its help were unsuccessful. The theory even led to the discovery of a new planet, Neptune. But in spite of its success, sustained attempts to falsify it eventually proved successful. Newton's theory was falsified in a number of ways. It was unable to account for the details of the orbit of the planet Mercury and was unable to account for the variable mass of fast-moving electrons in discharge tubes. Challenging problems faced physicists, then, as the nineteenth century gave way to the twentieth, problems calling for new speculative hypotheses designed to overcome these problems in a progressive way. Einstein was able to meet this challenge. His relativity theory was able to account for the phenomena that falsified Newton's theory, while at the same time being able to match Newton's theory in those areas where the latter had

proved successful. In addition, Einstein's theory yielded the prediction of spectacular new phenomena. His special theory of relativity predicted that mass should be a function of velocity and that mass and energy could be transformed into one another, and his general theory predicted that light rays should be bent by strong gravitational fields. Attempts to refute Einstein's theory by reference to the new phenomena failed. The falsification of Einstein's theory remains a challenge for modern physicists. Their success, if it should eventuate, would mark a new step forward in the progress of physics.

So runs a typical falsification account of the progress of physics. Later we shall have cause to doubt its accuracy and validity.

From the foregoing, it is clear that the concept of progress, of the growth of science, is a conception that is a central one in the falsificationist account of science. This issue is pursued in more detail in the next chapter.

Further reading

The classic falsificationist text is Popper in *The Logic of Scientific Discovery* (1972), first published in German in 1934 and translated into English in 1959. More recent collections of his writings are Popper (1969) and Popper (1979). Popper's own story about how he came to his basic idea through comparing Freud, Adler and Marx with Einstein is in chapter 1 of his 1969 text. More sources related to falsificationism will be given at the end of the next chapter.

CHAPTER 6

Sophisticated falsificationism, novel predictions and the growth of science

Relative rather than absolute degrees of falsifiability

The previous chapter mentioned some conditions that an hypothesis should satisfy in order to be worthy of a scientist's consideration. An hypothesis should be falsifiable, the more falsifiable the better, and yet should not be falsified. More sophisticated falsificationists realise that those conditions alone are insufficient. A further condition is connected with the need for science to progress. An hypothesis should be more falsifiable than the one for which it is offered as a replacement.

The sophisticated falsificationist account of science, with its emphasis on the growth of science, switches the focus of attention from the merits of a single theory to the relative merits of competing theories. It gives a dynamic picture of science rather than the static account of the most naive falsificationists. Instead of asking of a theory, "Is it falsifiable?", "How falsifiable is it?" and "Has it been falsified?", it becomes more appropriate to ask, "Is this newly proposed theory a viable replacement for the one it challenges?" In general, a newly proposed theory will be acceptable as worthy of the consideration of scientists if it is more falsifiable than its rival, and especially if it predicts a new kind of phenomenon not touched on by its rival.

The emphasis on the comparison of degrees of falsifiability of series of theories, which is a consequence of the emphasis on a science as a growing and evolving body of knowledge, enables a technical problem to be bypassed. For it is very difficult to specify just how falsifiable a single theory is. An absolute measure of falsifiability cannot be defined simply because the number of potential falsifiers of a theory will

always be infinite. It is difficult to see how the question "How falsifiable is Newton's law of gravitation?" could be answered. On the other hand, it is often possible to compare the degrees of falsifiability of laws or theories. For instance, the claim "All pairs of bodies attract each other with a force that varies inversely as the square of their separation" is more falsifiable than the claim "The planets in the solar system attract each other with a force that varies inversely as the square of their separation". The second is implied by the first. Anything that falsifies the second will falsify the first, but the reverse is not true. Ideally, the falsificationist would like to be able to say that the series of theories that constitute the historical evolution of a science is made up of falsifiable theories, each one in the series being more falsifiable than its predecessor.

Increasing falsifiability and ad hoc modifications

The demand that as a science progresses its theories should become more and more faisifiable, and consequently have more and more content and be more and more informative, rules out modifications in theories that are designed merely to protect a theory from a threatening falsification. A modification in a theory, such as the addition of an extra postulate or a change in some existing postulate, that has no testable consequences that were not already testable consequences of the unmodified theory will be called *ad hoc* modifications. The remainder of this section will consist of examples designed to clarify the notion of an ad hoc modification. I will first consider some ad hoc modifications, which the faisificationist would reject, and afterwards these will be contrasted with some modifications that are not ad hoc and which the falsificationist would consequently welcome.

I begin with a rather trivial example. Let us consider the generalisation "Bread nourishes". This low-level theory, if spelt out in more detail, amounts to the claim that if wheat is grown in the normal way, converted into bread in the normal way and eaten by humans in a normal way, then those humans will be nourished. This apparently innocuous theory ran into trouble in a French village on an occasion when wheat was grown in a normal way, converted into bread in a normal way and yet most people who ate the bread became seriously ill and many died. The theory "(All) bread nourishes" was falsified. The theory can be modified to avoid this falsification by adjusting it to read, "(All) bread, with the exception of that particular batch of bread produced in the French village in question, nourishes". This is an ad hoc modification. The modified theory cannot be tested in any way that was not also a test of the original theory. The consuming of any bread by any human constitutes a test of the original theory, whereas tests of the modified theory are restricted to the consuming of bread other than that batch of bread that led to such disastrous results in France. The modified hypothesis is less falsifiable than the original version. The falsificationist rejects such rearguard actions.

The next example is less gruesome and more entertaining. It is an example based on an interchange that actually took place in the seventeenth century between Galileo and an Aristotelian adversary. Having carefully observed the moon through his newly invented telescope, Galileo was able to report that the moon was not a smooth sphere but that its surface abounded in mountains and craters. His Aristotelian adversary had to admit that things did appear that way when he repeated the observations for himself. But the observations threatened a notion fundamental for many Aristotelians, namely that all celestial bodies are perfect spheres. Galileo's rival defended his theory in the face of the apparent falsification in a way that was blatantly ad hoc. He suggested that there was an invisible substance on the moon filling the craters and covering the mountains in such a way that the moon's shape was perfectly spherical. When Galileo inquired how the presence of the invisible substance might be detected, the reply was that there was no way in which it could be detected. There is no doubt, then, that the modified theory led to no new testable consequences and would be quite unacceptable to a falsificationist. An exasperated Galileo was able to show up the inadequacy of his rival's position in a characteristically witty way. He announced that he was prepared to admit that the invisible, undetectable substance existed on the moon, but insisted that it was not distributed in the way suggested by his rival but in fact was piled up on top of the mountains so that they were many times higher than they appeared through the telescope. Galileo was able to outmanoeuvre his rival in the fruitless game of the invention of ad hoc devices for the protection of theories.

One other example of a possibly ad hoc hypothesis from the history of science will be briefly mentioned. Prior to Lavoisier, the phlogiston theory was the standard theory of combustion. According to that theory, phlogiston is emitted from substances when they are burnt. This theory was threatened when it was discovered that many substances gain weight after combustion. One way of overcoming the apparent falsification was to suggest that phlogiston has negative weight. If this hypothesis could be tested only by weighing substances before and after combustion, then it was ad hoc. It led to no new tests.

Modifications of a theory in an attempt to overcome a difficulty need not be ad hoc. Here are some examples of modifications that are not ad hoc, and which consequently are acceptable from a falsificationist point of view.

Let us return to the falsification of the claim "Bread nourishes" to see how this could be modified in an acceptable way. An acceptable move would be to replace the original falsified theory by the claim "All bread nourishes except bread made from wheat contaminated by a particular kind of fungus" (followed by a specification of the fungus and some of its characteristics). This modified theory is not ad hoc because it leads to new tests. It is *independently testable*, to use Popper's (1972, p. 193) phrase. Possible tests would include testing the wheat from which the poisonous bread was made for the presence of the fungus, cultivating the fungus on some specially prepared wheat and testing the nourishing effect of the bread produced from it, chemically analysing the fungus for the presence of known poisons, and so on. All these tests, many of which do not constitute tests of the original hypothesis, could result in the falsification of the modified hypothesis. If the modified, more falsifiable, hypothesis resists falsification in the face of the new tests, then something new will have been learnt and progress will have been made.

Turning now to the history of science for a less artificial example, we might consider the train of events that led to the discovery of the planet Neptune. Nineteenth-century observations of the motion of the planet Uranus indicated that its orbit departed considerably from that predicted on the basis of Newton's gravitational theory, thus posing a problem for that theory. In an attempt to overcome the difficulty, it was suggested by Leverrier in France and by Adams in England that there existed a previously undetected planet in the vicinity of Uranus. The attraction between the conjectured planet and Uranus was to account for the latter's departure from its initially predicted orbit. This suggestion was not ad hoc, as events were to show. It was possible to estimate the approximate vicinity of the conjectural planet if it were to be of a reasonable size and to be responsible for the perturbation of Uranus' orbit. Once this had been done, it was possible to test the new proposal by inspecting the appropriate region of the sky through a telescope. It was in this way that Galle came to make the first sighting of the planet now known as Neptune. Far from being ad hoc, the move to save Newton's theory from falsification by Uranus's orbit led to a new kind of test of that theory, which it was able to pass in a dramatic and progressive way.

Confirmation in the falsificationist account of science

When falsificationism was introduced as an alternative to inductivism in the previous chapter, falsifications (that is, the failures of theories to stand up to observational and

experimental tests) were portrayed as being of key importance. It was argued that the logical situation permits the establishment of the falsity but not of the truth of theories in the light of available observation statements. It was also urged that science should progress by the proposal of bold, highly falsifiable conjectures as attempts to solve problems, followed by ruthless attempts to falsify the new proposals. Along with this came the suggestion that significant advances in science come about when those bold conjectures are falsified. The self-avowed falsificationist Popper says as much in the passage quoted on pp. 66–7, where the italics are his. However, exclusive attention to falsifying instances amounts to a misrepresentation of the more sophisticated falsificationist's position. More than a hint of this is contained in the example with which the previous section concluded. The independently testable attempt to save Newton's theory by a speculative hypothesis was a success because that hypothesis was confirmed by the discovery of Neptune and not because it was falsified.

It is a mistake to regard the falsification of bold, highly falsifiable conjectures as the occasions of significant advance in science, and Popper needs to be corrected on this point. This becomes clear when we consider the various extreme possibilities. At one extreme we have theories that take the form of bold, risky conjectures, while at the other we have theories that are cautious conjectures, making claims that seem to involve no significant risks. If either kind of conjecture fails an observational or experimental test it will be falsified, and if it passes such a test we will say it is *confirmed*. Significant advances will be marked by the *confirmation* of *bold* conjectures or the *falsification* of *cautious* conjectures. Cases of the former kind will be informative, and constitute an important contribution to scientific knowledge, simply because they mark the discovery of something that was previously unheard of or considered unlikely. The discovery of Neptune and of radio waves and Eddington's confirmation of Einstein's risky prediction that light rays should bend in strong gravitational

CHAPTER 8

Theories as structures I: Kuhn's paradigms

Theories as structures

The sketch of the Copernican Revolution outlined in the previous chapter suggests that the inductivist and falsificationist accounts of science are too piecemeal. Concentrating on the relationship between theories and individual observation statements or sets of them, they seem to fail to grasp the complexity of the mode of development of major theories. Since the 1960s it has become common to conclude from this that a more adequate account of science must proceed from an understanding of the theoretical frameworks in which scientific activity takes place. The next three chapters are concerned with three influential accounts of science that have resulted from an adoption of this approach. (In chapter 13 we will have reason to question whether the "theory-dominated" view of science has gone too far.)

One reason why there is seen to be a need to view theories as structures stems from the history of science. Historical study reveals that the evolution and progress of major sciences exhibit a structure that is not captured by the inductivist and falsificationist accounts. The Copernican Revolution has already supplied us with an example. The notion can be further enhanced by reflecting on the fact that for a couple of centuries after Newton, physics was carried out in the Newtonian framework, until that framework was challenged by relativity and quantum theory at the beginning of the century. However, the historical argument is not the only reason why some have seen the need to concentrate on theoretical frameworks. A more general, philosophical argument is closely linked with the ways in which observation can be said to be theory-dependent. In chapter 1 it was stressed

that observation statements must be expressed in the language of some theory. Consequently, it is argued, the statements, and the concepts figuring in them, will be as precise and informative as the theory in whose language they are formed is precise and informative. For instance, I think it will be agreed that the Newtonian concept of mass has a more precise meaning than the concept of democracy, say. It is plausible to suggest that the reason for the relatively precise meaning of the former stems from the fact that the concept plays a specific, well-defined role in a precise, closely knit theory, Newtonian mechanics. By contrast, the social theories in which the concept "democracy" occurs are vague and multifarious. If this suggested close connection between precision of meaning of a term or statement and the role played by that term or statement in a theory is valid, then the need for coherently structured theories would seem to follow directly from it.

The dependence of the meaning of concepts on the structure of the theory in which they occur, and the dependence of the precision of the former on the precision and degree of coherence of the latter, can be made plausible by noting the limitations of some of the alternative ways in which a concept might be thought to acquire meaning. One such alternative is the view that concepts acquire their meaning by way of a definition. Definitions must be rejected as a fundamental way of establishing meanings because concepts can only be defined in terms of other concepts, the meanings of which are given. If the meanings of these latter concepts are themselves established by definition, it is clear that an infinite regress will result unless the meanings of some concepts are known by other means. A dictionary is useless unless we already know the meanings of many words. Newton could not define mass or force in terms of previously available concepts. It was necessary for him to transcend the limits of the old conceptual framework by developing a new one. A second alternative is the suggestion that concepts acquire their meaning by way of ostensive definition. We saw, in our discussion of a child learning the meaning of "apple" in chapter 1, that this is difficult to sustain even in the case of an elementary notion like "apple". It is even more implausible when it comes to the definition of something like "mass" in mechanics or "electric field" in electromagnetism.

The claim that concepts derive their meaning at least in part from the role they play in a theory can be given support by the following historical reflections. Contrary to popular myth, experiment was by no means the key to Galileo's innovations in mechanics. Many of the "experiments" he refers to in articulating his theory are thought experiments. This can appear paradoxical for those who see novel theories arising as a result of experiment, but it is quite comprehensible if it is accepted that precise experimentation can only be carried out if one has a precise theory capable of yielding predictions in the form of precise observation statements. Galileo, it might be argued, was in the process of making a major contribution to the building of a new mechanics that was to prove capable of supporting detailed experimentation at a later stage. It need not be surprising that his efforts involved thought experiments, analogies and illustrative metaphors rather than detailed experimentation. A case could be made to the effect that the typical history of a concept, whether it be "chemical element", "atom", "the unconscious" or whatever, involves the initial emergence of the concept as a vague idea, followed by its gradual clarification as the theory in which it plays a part takes a more precise and coherent form. The emergence of the concept of an electric field can be construed in a way that supports such a view. When the concept was first introduced by Faraday in the first half of the nineteenth century it was very vague, and was articulated with the aid of mechanical analogies involving such things as stretched strings and metaphorical uses of such terms as "tension", "power" and "force". The field concept became increasingly better defined as the relationship between the electric field and other electromagnetic quantities became more clearly specified. Once Maxwell had introduced his displacement current, again with the aid of mechanical analogies, it was possible to bring great coherence to the theory in the form of Maxwell's equations, which clearly specified the interrelationship between all the electromagnetic quantities. It was not long before the ether, which had been considered to be the mechanical seat of the fields, could be dispensed with, leaving the fields as clearly defined concepts in their own right.

In this section I have attempted to construct a rationale for approaching science by way of the theoretical frameworks within which scientific work and argumentation take place. In this and the following two chapters we look at the work of three important philosophers of science who have pursued this idea.

Introducing Thomas Kuhn

Inductivist and falsificationist accounts of science were challenged in a major way by Thomas Kuhn (1970a) in his book *The Structure of Scientific Revolutions*, first published in 1962, and then republished with a clarificatory PostScript eight years later. His views have reverberated in the philosophy of science ever since. Kuhn started his academic career as a physicist and then turned his attention to the history of science. On doing so, he found that his preconceptions about the nature of science were shattered. He came to believe that traditional accounts of science, whether inductivist or falsificationist, do not bear comparison with historical evidence. Kuhn's account of science was subsequently developed as an attempt to give a theory more in keeping with the historical situation as he saw it. A key feature of his theory is the emphasis placed on the revolutionary character of scientific progress, where a revolution involves the abandonment of one theoretical structure and its replacement by another, incompatible one. Another important feature is the important role played by the sociological characteristics of scientific communities.

Kuhn's picture of the way a science progresses can be summarised by the following open-ended scheme:

pre-science — normal science — crisis — revolution — new normal science — new crisis

The disorganised and diverse activity that precedes the formation of a science eventually becomes structured and directed when a single *paradigm* becomes adhered to by a scientific community. A paradigm is made up of the general theoretical assumptions and laws and the techniques for their application that the members of a particular scientific community adopt. Workers within a paradigm, whether it be Newtonian mechanics, wave optics, analytical chemistry or whatever, practise what Kuhn calls *normal science.* Normal scientists will articulate and develop the paradigm in their attempt to account for and accommodate the behaviour of some relevant aspects of the real world as revealed through the results of experimentation. In doing so, they will inevitably experience difficulties and encounter apparent falsifications. If difficulties of that kind get out of hand, a *crisis* state develops. A crisis is resolved when an entirely new paradigm emerges and attracts the allegiance of more and more scientists until eventually the original, problem-ridden paradigm is abandoned. The discontinuous change constitutes a *scientific revolution.* The new paradigm, full of promise and not beset by apparently insuperable difficulties, now guides new normal scientific activity until it too runs into serious trouble and a new crisis followed by a new revolution results.

With this resumé as a foretaste, let us look at the various components of Kuhn's scheme in more detail.

Paradigms and normal science

A mature science is governed by a single paradigm.[1] The paradigm sets the standards for legitimate work within the science it governs. It coordinates and directs the "puzzle-solving" activity of the groups of normal scientists who work

within it. The existence of a paradigm capable of supporting a normal science tradition is the characteristic that distinguishes science from non-science, according to Kuhn. Newtonian mechanics, wave optics and classical electromagnetism all constituted and perhaps constitute paradigms and qualify as sciences. Much of modern sociology lacks a paradigm and consequently fails to qualify as science.

As will be explained below, it is of the nature of a paradigm to belie precise definition. Nevertheless, it is possible to describe some of the typical components that go to make up a paradigm. Among the components will be explicitly stated fundamental laws and theoretical assumptions. Thus Newton's laws of motion form part of the Newtonian paradigm and Maxwell's equations form part of the paradigm that constitutes classical electromagnetic theory. Paradigms will also include standard ways of applying the fundamental laws to a variety of types of situation. For instance, the Newtonian paradigm will include methods of applying Newton's laws to planetary motion, pendulums, billiard-ball collisions, and so on. Instrumentation and instrumental techniques necessary for bringing the laws of the paradigm to bear on the real world will also be included in the paradigm. The application of the Newtonian paradigm in astronomy involves the use of a variety of approved kinds of telescope, together with techniques for their use and a variety of techniques for the correction of the data collected with their aid. A further component of paradigms consists of some very general, metaphysical principles that guide work within a paradigm. Throughout the nineteenth century the Newtonian paradigm was governed by an assumption something like, "The whole of the physical world is to be explained as a mechanical system operating under the influence of various forces according to the dictates of Newton's laws of motion", and the Cartesian program in the seventeenth century involved the principle, "There is no void and the physical universe is a big clockwork in which all forces take the form of a push". Finally, all paradigms will contain some very general methodological prescriptions such as, "Make serious attempts to match your paradigm with nature", or "Treat failures in attempts to match a paradigm with nature as serious problems".

Normal science involves detailed attempts to articulate a paradigm with the aim of improving the match between it and nature. A paradigm will always be sufficiently imprecise and open-ended to leave plenty of that kind of work to be done. Kuhn portrays normal science as a puzzle-solving activity governed by the rules of a paradigm. The puzzles will be of both a theoretical and an experimental nature. Within the Newtonian paradigm, for instance, typical theoretical puzzles involve devising mathematical techniques for dealing with the motion of a planet subject to more than one attractive force, and developing assumptions suitable for applying Newton's laws to the motion of fluids. Experimental puzzles included the improvement of the accuracy of telescopic observations and the development of experimental techniques capable of yielding reliable measurements of the gravitational constant. Normal scientists must presuppose that a paradigm provides the means for the solution of the puzzles posed within it. A failure to solve a puzzle is seen as a failure of the scientist rather than as an inadequacy of the paradigm. Puzzles that resist solution are seen as *anomalies* rather than as falsifications of a paradigm. Kuhn recognises that all paradigms will contain some anomalies (for example the Copernican theory and the apparent size of Venus or the Newtonian paradigm and the orbit of Mercury) and rejects all brands of falsificationism.

Normal scientists must be uncritical of the paradigm in which they work. It is only by being so that they are able to concentrate their efforts on the detailed articulation of the paradigm and to perform the esoteric work necessary to probe nature in depth. It is the lack of disagreement over fundamentals that distinguishes mature, normal science from the relatively disorganised activity of immature *pre-science*. According to Kuhn, the latter is characterised by total disagreement and constant debate over fundamentals, so much so that

it is impossible to get down to detailed, esoteric work. There will be almost as many theories as there are workers in the field and each theoretician will be obliged to start afresh and justify his or her own particular approach. Kuhn offers optics before Newton as an example. There was a wide diversity of theories about the nature of light from the time of the ancients up to Newton. No general agreement was reached and no detailed, generally accepted theory emerged before Newton proposed and defended his particle theory. The rival theorists of the pre-science period disagreed not only over fundamental theoretical assumptions but also over the kinds of observational phenomena that were relevant to their theories. Insofar as Kuhn recognises the role played by a paradigm in guiding the search for and interpretation of observable phenomena, he accommodates the sense in which observation and experiment can be said to be theory-dependent.

Kuhn insists that there is more to a paradigm than what can be explicitly laid down in the form of explicit rules and directions. He invokes Wittgenstein's discussion of the notion of "game" to illustrate some of what he means. Wittgenstein argued that it is not possible to spell out necessary and sufficient conditions for an activity to be a game. When one tries, one invariably finds an activity that one's definition includes but that one would not want to count as a game, or an activity that the definition excludes but that one would want to count as a game. Kuhn claims that the same situation exists with respect to paradigms. If one tries to give a precise and explicit characterisation of some paradigm in the history of science or in present-day science, it always turns out that some work within the paradigm violates the characterisation. However, Kuhn insists that this state of affairs does not render the concept of paradigm untenable any more than the similar situation with respect to "game" rules out legitimate use of that concept. Even though there is no complete, explicit characterisation, individual scientists acquire knowledge of a paradigm through their scientific education. By solving standard problems, performing standard experiments and eventually by doing a piece of research under a supervisor who is already a skilled practitioner within the paradigm, an aspiring scientist becomes acquainted with the methods, the techniques and the standards of that paradigm. The aspiring scientist will be no more able to give an explicit account of the methods and skills he or she has acquired than a master-carpenter will be able to fully describe what lies behind his or her skills. Much of the normal scientist's knowledge will be *tacit*, in the sense developed by Michael Polanyi (1973).

Because of the way they are trained, and need to be trained if they are to work efficiently, typical normal scientists will be unaware of and unable to articulate the precise nature of the paradigm in which they work. However, it does not follow from this that a scientist will not be able to articulate the presuppositions involved in the paradigm should the need arise. Such a need will arise when a paradigm is threatened by a rival. In those circumstances, it will be necessary to attempt to spell out the general laws and metaphysical and methodological principles involved in a paradigm in order to defend them against the alternatives involved in the threatening new paradigm. The next section summarises Kuhn's account of how a paradigm can run into trouble and be replaced by a rival.

Crisis and revolution

Normal scientists work confidently within a well-defined area dictated by a paradigm. The paradigm presents them with a set of definite problems together with methods that they are confident will be adequate for the solution of the problems. If they blame the paradigm for any failure to solve a problem, they will be open to the same charges as the carpenter who blames his tools. Nevertheless, failures will be encountered and such failures can eventually attain a degree of seriousness that constitutes a serious crisis for the paradigm and may lead to the rejection of a paradigm and its replacement by an incompatible alternative.

The mere existence of unsolved puzzles within a paradigm does not constitute a crisis. Kuhn recognises that paradigms will always encounter difficulties. There will always be anomalies. It is only under special sets of conditions that the anomalies can develop in such a way as to undermine confidence in the paradigm. An anomaly will be regarded as particularly serious if it is seen as striking at the very fundamentals of a paradigm and yet persistently resists attempts by the members of the normal scientific community to remove it. Kuhn cites as an example problems associated with the ether and the earth's motion relative to it in Maxwell's electromagnetic theory, towards the end of the nineteenth century. A less-technical example would be the problems that comets posed for the ordered and full Aristotelian cosmos of interconnected crystalline spheres. Anomalies are also regarded as serious if they are important with respect to some pressing social need. The problems that beset Ptolemaic astronomy were pressing ones in the light of the need for calendar reform at the time of Copernicus. Also bearing on the seriousness of an anomaly will be the length of time that it resists attempts to remove it. The number of serious anomalies is a further factor influencing the onset of a crisis.

According to Kuhn, an analysis of the characteristics of a crisis period in science demands the competence of the psychologist as much as that of the historian. When anomalies come to be seen as posing serious problems for a paradigm, a period of "pronounced professional insecurity" sets in. Attempts to solve the problem become more and more radical and the rules set by the paradigm for the solution of problems become progressively more loosened. Normal scientists begin to engage in philosophical and metaphysical disputes and try to defend their innovations, of dubious status from the point of view of the paradigm, by philosophical arguments. Scientists even begin to express openly their discontent with and unease over the reigning paradigm. Kuhn (1970a, p. 84) quotes Wolfgang Pauli's response to what he saw as the growing crisis in physics around 1924. An exasperated Pauli confessed to a friend, "At the moment, physics is again terribly confused. In any case, it is too difficult for me, and I wish I had been a movie comedian or something of the sort and had never heard of physics". Once a paradigm has been weakened and undermined to such an extent that its proponents lose their confidence in it, the time is ripe for revolution.

The seriousness of a crisis deepens when a rival paradigm makes its appearance. According to Kuhn (1970a, p. 91), "the new paradigm, or a sufficient hint to permit later articulation, emerges all at once, sometimes in the middle of the night, in the mind of a man deeply immersed in crisis". The new paradigm will be very different from and incompatible with the old one. The radical differences will be of a variety of kinds.

Each paradigm will regard the world as being made up of different kinds of things. The Aristotelian paradigm saw the universe as divided into two distinct realms, the incorruptible and unchanging super-lunar region and the corruptible and changing earthly region. Later paradigms saw the entire universe as being made up of the same kinds of material substances. Pre-Lavoisier chemistry involved the claim that the world contained a substance called phlogiston, which is driven from materials when they are burnt. Lavoisier's new paradigm implied that there is no such thing as phlogiston, whereas the gas, oxygen, does exist and plays a quite different role in combustion. Maxwell's electromagnetic theory involved an ether occupying all space, whereas Einstein's radical recasting of it eliminated the ether.

Rival paradigms will regard different kinds of questions as legitimate or meaningful. Questions about the weight of phlogiston were important for phlogiston theorists and vacuous for Lavoisier. Questions about the mass of planets were fundamental for Newtonians and heretical for Aristotelians. The problem of the velocity of the earth relative to the ether, which was deeply significant for pre-Einsteinian physicists, was dissolved by Einstein. As well as posing different kinds of questions, paradigms will involve different and incompatible standards. Unexplained action at a distance was permitted by Newtonians but dismissed by Cartesians as metaphysical and even occult. Uncaused motion was nonsense for Aristotle and axiomatic for Newton. The transmutation of elements has an important place in modern nuclear physics (as it did in mediaeval alchemy and in seventeenth-century mechanical philosophy) but ran completely counter to the aims of Dalton's atomistic program. A number of kinds of events describable within modern microphysics involve an indeterminancy that had no place in the Newtonian program.

The way scientists view a particular aspect of the world will be guided by a paradigm in which they are working. Kuhn argues that there is a sense in which proponents of rival paradigms are "living in different worlds". He cites as evidence the fact that changes in the heavens were first noted, recorded and discussed by Western astronomers after the proposal of the Copernican theory. Before that, the Aristotelian paradigm had dictated that there could be no change in the super-lunar region and, accordingly, no change was observed. Those changes that were noticed were explained away as disturbances in the upper atmosphere.

The change of allegiance on the part of individual scientists from one paradigm to an incompatible alternative is likened by Kuhn to a "gestalt switch" or a "religious conversion". There will be no purely logical argument that demonstrates the superiority of one paradigm over another and that thereby compels a rational scientist to make the change. One reason why no such demonstration is possible is the fact that a variety of factors are involved in a scientist's judgment of the merits of a scientific theory. An individual scientist's decision will depend on the priority he or she gives to the various factors. The factors will include such things as simplicity, the connection with some pressing social need, the ability to solve some specified kind of problem, and so on. Thus one scientist might be attracted to the Copernican theory because of the simplicity of certain mathematical features of it. Another might be attracted to it because in it there is the possibility of calendar reform. A third might have been deterred from adopting the Copernican theory because of an involvement with terrestrial mechanics and an awareness of the problems that the Copernican theory posed for it. A fourth might reject Copernicanism for religious reasons.

A second reason why no logically compelling demonstration of the superiority of one paradigm over another exists stems from the fact that proponents of rival paradigms will subscribe to different sets of standards and metaphysical principles. Judged by its own standards, paradigm *A* may be judged superior to paradigm *B*, whereas if the standards of paradigm *B* are used as premises, the judgment may be reversed. The conclusion of an argument is compelling only if its premises are accepted. Supporters of rival paradigms will not accept each others' premises and so will not necessarily be convinced by each others' arguments. It is for this kind of reason that Kuhn (1970a, pp. 93–4) compares scientific revolutions with political revolutions. Just as "political revolutions aim to change politicial institutions in ways that those institutions themselves prohibit" and consequently "political recourse fails", so the choice "between competing paradigms proves to be a choice between incompatible modes of community life", and no argument can be "logically or even probabilistically compelling". This is not to say, however, that various arguments will not be among the important factors that influence the decisions of scientists. On Kuhn's view, the kinds of factors that do prove effective in causing scientists to change paradigms is a matter to be discovered by psychological and sociological investigation.

There are a number of interrelated reasons, then, why, when one paradigm competes with another, there is no logically compelling argument that dictates that a rational scientist should abandon one for the other. There is no single criterion by which a scientist must judge the merit or promise of a paradigm, and, further, proponents of competing programs will subscribe to different sets of standards and will even view the world in different ways and describe it in

different languages. The aim of arguments and discussions between supporters of rival paradigms should be persuasion rather than compulsion. I suggest that what I have summarised in this paragraph is what lies behind Kuhn's claim that rival paradigms are "incommensurable".

A scientific revolution corresponds to the abandonment of one paradigm and the adoption of a new one, not by an individual scientist only but by the relevant scientific community as a whole. As more and more individual scientists, for a variety of reasons, are converted to the new paradigm, there is an "increasing shift in the distribution of professional allegiances" (Kuhn, 1970a, p. 158). If the revolution is to be successful, this shift will spread so as to include the majority of the relevant scientific community, leaving only a few dissenters. These will be excluded from the new scientific community and will perhaps takes refuge in a philosophy department. In any case, they will eventually die.

The function of normal science and revolutions

Some aspects of Kuhn's writings might give the impression that his account of the nature of science is a purely *descriptive* one, that is, that he aims to do nothing more than to describe scientific theories or paradigms and the activity of scientists. Were this the case, then Kuhn's account of science would be of little value as a *theory* of science. Unless the descriptive account of science is shaped by some theory, no guidance is offered as to what kinds of activities and products of activities are to be described. In particular, the activities and productions of hack scientists would need to be documented in as much detail as the achievements of an Einstein or a Galileo.

However, it is a mistake to regard Kuhn's characterisation of science as arising solely from a description of the work of scientists. Kuhn insists that his account constitutes a theory of science because it includes an explanation of the *function* of its various components. According to Kuhn, normal science and revolutions serve necessary functions, so that science must either involve those characteristics or some others that would serve to perform the same functions. Let us see what those functions are, according to Kuhn.

Periods of normal science provide the opportunity for scientists to develop the esoteric details of a theory. Working within a paradigm, the fundamentals of which they take for granted, they are able to perform the exacting experimental and theoretical work necessary to improve the match between the paradigm and nature to an ever-greater degree. It is through their confidence in the adequacy of a paradigm that scientists are able to devote their energies to attempts to solve the detailed puzzles presented to them within the paradigm, rather than engage in disputes about the legitimacy of their fundamental assumptions and methods. It is necessary for normal science to be to a large extent uncritical. If all scientists were critical of all parts of the framework in which they worked all of the time then no detailed work would ever get done.

If all scientists were and remained normal scientists, a particular science would become trapped in a single paradigm and would never progress beyond it. This would be a serious fault, from the Kuhnian point of view. A paradigm embodies a particular conceptual framework through which the world is viewed and in which it is described, and a particular set of experimental and theoretical techniques for matching the paradigm with nature. But there is no *a priori* reason to expect that any one paradigm is perfect or even the best available. There are no inductive procedures for arriving at perfectly adequate paradigms. Consequently, science should contain within it a means of breaking out of one paradigm into a better one. This is the function of revolutions. All paradigms will be inadequate to some extent as far as their match with nature is concerned. When the mismatch becomes serious, that is, when a crisis develops, the revolutionary step of replacing the entire paradigm with another becomes essential for the effective progress of science.

Progress through revolutions is Kuhn's alternative to the

cumulative progress characteristic of inductivist accounts of science. According to the latter view, scientific knowledge grows continuously as more numerous and more various observations are made, enabling new concepts to be formed, old ones to be refined, and new lawful relationships between them to be discovered. From Kuhn's particular point of view, this is mistaken, because it ignores the role played by paradigms in guiding observation and experiment. It is just because paradigms have such a pervasive influence on the science practised within them that the replacement of one by another must be a revolutionary one.

One other function catered for in Kuhn's account is worth mentioning. Kuhn's paradigms are not so precise that they can be replaced by an explicit set of rules, as was mentioned above. Different scientists or groups of scientists may well interpret and apply the paradigm in a somewhat different way. Faced with the same situation, not all scientists will reach the same decision or adopt the same strategy. This has the advantage that the number of strategies attempted will be multiplied. Risks are thus distributed through the scientific community, and the chances of some long-term success are increased. "How else", asks Kuhn (1970c, p. 241), "could the group as a whole hedge its bets?"

The merits of Kuhn's account of science

There is surely something descriptively correct about Kuhn's idea that scientific work involves solving problems within a framework that is, in the main, unquestioned. A discipline in which fundamentals are constantly brought into question, as characterised in Popper's method of "conjectures and refutations", is unlikely to make significant progress simply because principles do not remain unchallenged long enough for esoteric work to be done. It is all very well painting a heroic picture of Einstein as making a major advance by having the originality and courage to challenge some of the fundamental principles of physics, but we should not lose sight of the fact up the world and the kinds of evidence and modes of explanation that are deemed appropriate. What is more, once this is acknowledged, then any adequate account of scientific progress must include an account of how the changes made in the course of a revolution can be construed as progressive. Indeed, we can draw on Kuhn's characterisation of science and pose the problem in a particularly acute way. Kuhn insisted that what counts as a problem can change from paradigm to paradigm, and also that the standards of adequacy that are brought to bear on proposed solutions to problems also vary from paradigm to paradigm. But if it is the case that standards vary from paradigm to paradigm, then what standards can be appealed to in order to judge that a paradigm in better than, and so constitutes progress over, the paradigm it replaces? In precisely what sense can science be said to progress through revolutions?

Kuhn's ambivalence on progress through revolutions

Kuhn is notoriously ambiguous on the basic question we have posed and which his own work serves to highlight. After the publication of *The Structure of Scientific Revolutions* Kuhn was charged with having put forward a "relativist" view of scientific progress. I take this to mean that Kuhn proposed an account of progress according to which the question of whether a paradigm is better or not than one that it challenges does not have a definitive, neutral answer, but depends on the values of the individual, group or culture that makes the judgment. Kuhn clearly was not comfortable with that charge and, in the PostScript that he added to the second edition of his book he attempted to distance himself from relativism. He wrote (1970a, p. 206), "later scientific theories are better than earlier ones for solving puzzles in the often quite different environments to which they are applied. That is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress". This criterion is problematic insofar as Kuhn himself stresses that what

counts as a puzzle and a solution to it is paradigm-dependent and also insofar as Kuhn (1970a, p. 154) elsewhere offers different criteria such as 'simplicity, scope and compatibility with other specialties'. But even more problematic is the clash between the non-relativist claim about progress and the numerous passages in Kuhn's book that read as an explicit advocacy of the relativist position, and even as a denial that there is a rational criterion of scientific progress at all.

Kuhn likens scientific revolutions to gestalt switches, to religious conversions and to political revolutions. Kuhn uses these comparisons to stress the extent to which the change of allegiance on the part of a scientist from one paradigm to another cannot be brought about by rational argument appealing to generally accepted criteria. The way in which the diagram on p. 6 changes from a staircase viewed from above to a staircase viewed from below is a modest example of a gestalt switch, but it serves to emphasise the extent to which such a switch is the very antithesis of a reasoned choice, and religious conversions are typically considered to be an analogous kind of change. As far as the analogy with political revolutions is concerned, Kuhn (1970a, pp. 93–4) insists that those revolutions "aim to change political institutions in ways that those institutions themselves prohibit" so that "political recourse fails". By analogy, the choice "between competing paradigms proves to be a choice between incompatible modes of community life" so that no argument can be "logically or even probabilistically compelling". Kuhn's insistence (1970a, p. 238) that the way in which we are to discover the nature of science is "intrinsically sociological" and is to be accomplished by "examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains", also leads to relativism if it transpires that different groups value, tolerate and disdain different things. This, indeed, is how proponents of the sociology of science currently in vogue commonly interpret Kuhn, developing his views into an explicit relativism.

In my view, Kuhn's account of scientific progress as it appears in the second edition of his book, complete with PostScript, contains two incompatible strands, one relativist and one not. This opens up two possibilities. The first is to follow the path taken by the sociologists mentioned in the previous paragraph and to embrace and develop the relativist strand in Kuhn's thought, which among other things involves carrying out the sociological investigation of science the need for which Kuhn alluded but never responded to. The second alternative is to ignore the relativism and rewrite Kuhn in a way that is compatible with some overarching sense of progress in science. This alternative will require an answer to the question of the sense in which a paradigm can be said to constitute progress over the one it replaces. I hope it will be clear by the end of the book which option I regard as the most fruitful.

Objective knowledge

"The transition between competing paradigms … must occur all at once (though not necessarily in an instant) or not at all." I am not the only one to have found this sentence from Kuhn (1970a, p.150) puzzling. How can a paradigm change take place all at once, but not necessarily in an instant? I do not think it is difficult to find the source of the confusion embodied in the problematic sentence. On the one hand, Kuhn is aware of the fact that a scientific revolution extends over a considerable period of time involving much theoretical and experimental work. Kuhn's own classic study of the Copernican Revolution (1959) documents the centuries of work involved. On the other hand, Kuhn's comparisons between paradigm change and gestalt switches or religious conversions make immediate sense of the idea that the change takes place "all at once". I suggest that Kuhn is, in effect, confusing two kinds of knowledge here, and it is important and helpful to spell out the distinction.

If I say "I know the date on which I wrote this particular paragraph and you do not", I am referring to knowledge that

I am aquainted with and that resides in my mind or brain, but which you are not aquainted with and is absent from your mind or brain. I know Newton's first law of motion but I do not know how to biologically classify a crayfish. Again, this is a question about what resides in my mind or brain. The claims that Maxwell was unaware that his electromagnetic theory predicted radio waves and that Einstein was aware of the results of the Michelson-Morley experiment involve this same usage of "know" in the sense of "being aware of". Knowledge is a state of mind. Closely connected with this usage, in the sense that it is also to do with the states of mind of individuals, is the issue of whether or not, and the degree to which, an individual accepts or believes a claim or set of claims. I believe that Galileo made a convinving case for the validity of the use of his telescope, but Feyerabend did not. Ludwig Boltzmann accepted the kinetic theory of gases but his compatriot Ernst Mach did not. All these ways of talking about knowledge and claims to knowledge are about the states of mind or attitudes of individuals. It is a common and perfectly legitimate way of talking. For want of a better term I will call what is talked of here knowledge in the *subjective* sense. I will distinguish it from a different usage which I refer to as knowledge in the *objective* sense.

The sentence "my cat lives in a house that no animals inhabit" has the property of being contradictory, while the sentences "I have a cat" and "today a guinea pig died" have the property of being consequences of the statement "today my white cat killed someone's pet guinea pig". In these examples, the fact that the sentences have the properties I attribute to them, in some common sense, is obvious, but this need not be so. For example, a lawyer in a murder trial may, after much painstaking analysis, discover the fact that one witness's report has consequences that contradict those of a second witness. If that is indeed the case, then it is the case whether the witnesses in question were aware of it or believed it or not. What is more, if the lawyer had not discovered the inconsistency, it may have remained undiscovered, so that no