2 Science and its method

In the last chapter I chose a few episodes in the history of science to illustrate the complex interrelationship between science, society and philosophical world views. The nature, role and influence of science changed historically and this had implications not just for what was regarded as science, but also how science was done, i.e. the method of science. In this chapter I will focus on how science is done, or how it is said it is done, through its method.

When scientists talk of scientific method they are usually referring to an ensemble of practices and understandings which differ from discipline to discipline. For example a physicist will often stress the role of experiments and a geologist the importance of meticulous observation, but there are few 'experiments' in geology and increasingly observation in physics is through the proxy of instruments. From this it would seem that there is no single algorithm for obtaining scientific knowledge but simply an ensemble of practices and knowledge that make up 'method'. So what is it that makes it scientific? Let me be more specific. Is the ensemble to which I refer an approved list of things that are 'scientific', or is it the case that if scientists do it, it is 'scientific'? The latter is indeed a charge that has been levelled against science: that science is simply a social construction. In Chapter 4 I will consider this charge in some depth, but for the moment I want to concentrate on the question of what method is supposed to be.

If the method of science is the route to knowledge which can be called 'scientific' then this has implications for investigations of the social world. Specifically if it is held that the social world arises from the natural world, or is continuous with it, then a reliable scientific 'method' would appear to be the best way to reliable knowledge of the social world. On the other hand if there is no dependable methodological route to knowledge of the natural world, but it is still held that the relationship between the natural and social world is emergent, or continuous, then the social and natural sciences may share methodological problems and solutions. There are two other possibilities. That is the social world is not emergent from, or continuous with the natural world, and there is no methodological common interest, or that even if it is, its manifest properties are so different that they cannot be known in the same way. This last is a view commonly advanced in social science and I will return to it in Chapter 5.

The question of method in science is often regarded as one of a demarcation between what is and isn't science, and raises issues not just of technique but of contested philosophical assumptions about knowledge and the nature of things that exist. A consideration of scientific method is not simply a technical exercise, but must also be a philosophical one. The measurement of *X* implies agreement not just on what is a good and accurate measurement, but also on what counts as *X* in the first place. Science is underwritten by agreements about what is knowledge and the means to it, and by agreement about the nature of existence – epistemology and ontology respectively. In turn these matters of knowledge and existence can be shaped by the discoveries of science. As Paul Davies notes: 'True revolutions in science involve more than spectacular discoveries and rapid advances in understanding. They also change the concepts on which science is based' (Davies 1989: 1).

A change in concepts will result in a change in methods. The Davies quote above is from the introduction to a book written by one of the pioneers of quantum physics, Werner Heisenberg, and the concepts in this case concern the nature of matter itself. As I observed in Chapter 1, prior to the emergence of quantum physics, the building blocks of all matter were considered to be atoms, discrete entities occupying a particular physical space and behaving as miniature versions of objects we see in the world around us. However, the atom turned out to have constituent 'parts' that did not behave in this way. Indeed, to describe fundamental particles as constituent parts at all is misleading. Heisenberg showed that though these particles had properties, such as velocity or position, they could not be observed simultaneously and furthermore the very act of observing itself seemed to have an effect on what could be measured (Heisenberg 1989: 32–3). The upshot of this is that the quantum world can only be known probabilistically. The realisation that material existence has a probabilistic and not deterministic basis had direct methodological consequences for measurement (Rae 1986: 53–62), but also philosophical consequences. Specifically, if the world is 'probable' and not 'determined' (though nevertheless probabilistically predictable) at the quantum level, to what extent and in which ways can it be 'determined' at the non-quantum level?

The acceptance of Heisenberg's 'uncertainty principle' by the scientific community is an excellent example of how scientific discovery changes the philosophical principles of science and in turn these principles influence the methodological choices of science (for a discussion of theories of scientific change see Richards 1997). Discovery in science (and everyday life), then, does not take place in a vacuum, but is shaped by theories, themselves the product of earlier discoveries. To get some feel for what science is it is useful to look at this process of discovery, a task I will undertake in the next section of this chapter. But, as I will suggest, discovery in science is paralleled by discovery in everyday life and though a necessary condition for science, it is not a sufficient one. Often what is known as the hypothetico-deductive model, which specifies the relationship between observation and inference, has been cited as a necessary condition in science. In the following section I will take a brief critical look at the inductive and deductive assumptions arising from different versions of this. Finally I will attempt to show that the process of science, and the relationships between scientists, and between scientists and nature, is a heterogeneous one and that what we call scientific method will reflect that heterogeneity.

Science as discovery

In science, as in everyday life, things are discovered by accident as well as design and quite often accidental discoveries come about when we are looking for something else. In the case of both discovery by design and by accident there is nevertheless a pre-existing body of knowledge that allows us to make sense of our discovery. Even accidental discoveries that occur when we are not even looking for something else still presuppose a body of knowledge that makes them sensible. As Louis Pasteur is reputed to have said, ‘accidents favour the prepared mind’ (Langley *et al.* 1987: 305). Perhaps there is something of a tautology here? *X* would not be known to be a discovery if we were unable to describe and place it within an existing body of knowledge.

The discovery and utilisation of penicillin neatly illustrates the process of discovery. In 1929 a bacteriologist, Alexander Fleming, found that a culture plate seeded with the micro-organism *staphylococci* had become contaminated with a mould. On the culture plate the *staphylococci* had grown except in the immediate vicinity of the mould, suggesting that there was something about the mould (itself a micro-organism) that could inhibit the growth of other micro-organisms. Through further tests Fleming found that this was the case. However, after publishing a paper of his results, Fleming did nothing further and it was another nine years before Ernst Chain and Howard Florey isolated pure penicillin and demonstrated that it could kill lethal streptococci in mice (Macfarlane 1979). The popular myth is that Fleming accidentally discovered penicillin and this was the birth of effective antibiotics. Yes it is true that Fleming had grown the mould accidentally as a result of leaving culture plates lying around, but he was nevertheless seeking a solution to the problem of infections in wounds. Medical orthodoxy held that antiseptics killed bacteria and wounds were treated accordingly. However during the First World War it had been noticed that such treatments led to the festering of wounds, despite the fact that the same antiseptic would kill bacteria in test tubes. Prior to his ‘accidental’ discovery of penicillin Fleming had already identified the bacteria responsible for the wound infections and discovered that antiseptics killed both these and the white blood cells the body manufactures as its own defence, but left behind a small quantity of a harmful bacterium that could then reproduce.

Fleming had been searching for a solution to this problem, but the solution was unanticipated and of a different kind to that sought. The later work of Chain and Florey led to an intended discovery – that penicillin could be produced and could cure lethal bacteriological infections.

The lesson of this story might be that to find anything we must be looking for something and that a lot of the time we are. This is as true of everyday life as it is in science. Science, as I suggested in the last chapter, is in this sense curiosity, but curiosity is also problem-solving. Perhaps, as Karl Popper argued, all organisms are constantly engaged in problem-solving and moreover must do so in order to survive (Popper 1979: 242). The patterns of discovery do, however, suggest a history to the problem. The history of the problem may be simple or complex, it may be personal or social, it may be well documented or just folk knowledge. The history of any problem will be that of past resolved problems. For example, I have a problem of book storage space in my office, but this arises out of the resolution of previous problems such as getting the books, an office, or a job, in the first place! In other words the solution of past problems makes the proposition of new problems possible. Even the most trivial problem arises out of a mass of accumulated knowledge, even in everyday life. In this sense discovery in science follows a pattern similar to that in everyday life (Langley *et al.* 1987: 7). Despite this, what we call scientific knowledge comes to be codified in particular ways. I will say something briefly about these.

Laws, theories, observation and hypotheses

1 *Laws and theories.* Scientific knowledge is embodied in laws, theories and hypotheses. The first thing to say is that there is no sharp distinction between a law and a theory. We talk of Newton's laws and Einstein's theories as important and reliable scientific knowledge, although it is often held that a law is derived from axioms and holds for all times and places, whereas a theory is a more speculative statement. We speak of the 'laws of nature', but post-Einstein we know that Newton's laws offered only a partial explanation of the workings of the universe. Conversely Einstein's 'Special' and 'General' theories can account for those things explained by Newton's laws and other phenomena besides, and they seem to be just as well founded as the former. Nevertheless laws might be regarded as 'facts' that are indisputable, and theories as generalisations from what we know, to what we believe to be the case. Laws also express regularities in the universe. Despite the complexity and apparent randomness of much of nature, there is also much that is regular. Although in any drop of water there are billions of hydrogen and oxygen atoms, their ordering is entirely regular and

determined. Of course this determined character breaks down at subatomic level and is not present in many large scale structures. Thus science aims to extend laws, possibly themselves necessarily of greater and greater complexity, so as to seek the existence of ordered arrangements where none may have been formerly perceived. Of course it may be the case that deterministic laws of the kind under which we can describe molecular structure cannot in principle be found to explain some parts of nature. In which case the search may be for statistical laws that can explain and predict aggregate behaviour, but not that of the components of the aggregate. Thus any particular scientific theory will be built on an edifice of laws and other theories, with the latter consisting of a number of propositions about the world, some of which are more well founded than others. This is an important point and I will return to it later.

2 *Observation and perception.* The process of discovery has a number of components. First, there is the thing in the world which is to be discovered, but there is nothing in the thing itself that will commend its discovery. Subatomic particles, gravity and penicillin do not say 'Hi, I'm here'; something else is needed. That something has both a psychological and a social aspect. First the psychological.

I have suggested above that the process of discovery is that of human problem-solving (with perhaps science as a special case). Cognitive psychologists account for this by seeing the human brain as an information processing system able to hold interrelated symbolic structures. This ability arises from our biology, the result of an evolutionary process. In thinking we copy, re-organise symbols in memory and resolve problems by creating symbolic representations that allow us to conduct a search for solutions along the most promising paths (Langley *et al.* 1987: 8). All of this cognitive equipment requires data and these data come from the apprehension of the world via our senses. Some of this apprehension is of physical characteristics and processes and some is of all already processed information. The apprehension of physical characteristics such as quantity, shape, size, colour etc. must come either from an innate capacity to know these things, what Kant referred to as the 'synthetic *a priori*' (Körner 1955: chapter 4), or it must come from previously acquired knowledge of the world – or, of course, both. However, even if we are genetically predisposed to know number, shape etc., much of the knowledge we need for discovery is social knowledge. That is, knowledge that is held in common. In science, as in any discipline with a recorded history, much of this knowledge will be held in artefacts such as books, papers etc. – what Popper called the 'Third World of knowledge' (or World 3). World 1 is physical objects, and World 2, conscious experiences (Popper 1994: chapter 3).

The difference between discovery in science and in everyday life lies in the nature of the social knowledge and of observation. The social knowledge of science is taken to be justified knowledge – that is facts – and these facts derive from structured and accurate observation. A rather good comparison between explanations deriving from non-scientific and scientific social knowledge lies in the perennially popular issue of life on Mars. In 1996 a meteorite of Martian origin was found in Antarctica. The meteorite contained, or appeared to contain, some primitive fossils, indicating that life may have, or may still exist on Mars (see *Nature,* 15 August 1997). This 'discovery' was taken seriously for it did not offend any of the 'facts' we already know about Mars, in that its chemical composition was recognisably Martian, and this we know this from probes which have been able to perform analyses on rocks *in situ* (see Ash *et al.* 1997). Conversely supermarket tabloid headlines like 'New NASA Photo Proves Humans Lived on Mars' do not have the same scientific status. Often at the centre of such a 'report' lies a grain of observational truth, but the 'knowledge' in which it is located has no empirical basis. Are the formations we can observe on Mars 'towers, columned temples, monumental statuary, immense frescoes? Or just rocks?' (Sagan 1996: 57). The scientist concludes the latter, for all her *experience,* both observational and that deriving from the social knowledge of science indicates this. This experience rests on a raft of other experiences, ultimately traceable to observational data – though observational data that accord with earlier experience.

Though of course anyone familiar with any refereed game such as football or baseball knows that what the referee sees, what the players see and what the crowd sees are often quite different. Thus it is in science. Russell Hanson asks us to consider two scientists observing the simple amoeba:

> One sees a one-celled animal, the other a non-celled animal. The first sees *Amoeba* in all its analogies with different types of single cells: liver cells, nerve cells, epithelium cells. ... The other, however, sees *Amoeba's* homology not with single cells, but with whole animals. Like all animals *Amoeba* ingests its food, digests and assimilates it. It excretes, reproduces and is mobile – more like a complete animal than an individual tissue cell.
>
> (Hanson 1965: 4)

Both scientists have seen the same thing, both can call upon the same 'Third World' of knowledge, yet they have interpreted what they have seen differently. Of course they may have actually *seen* something different, as in the celebrated 'duck–rabbit' picture (see for example Couvalis 1997: 12). Look at a picture one way, it's a duck, look at it another and it's a rabbit. But let us assume that their visual description is the same. Their explanation is, however, different because their description is 'theory laden', that is they depend on concepts the meaning of which is already known. For example, in cosmology the concept of 'redshift' describes the spectrum of a star moving away from an observer. This only makes sense if the scientist already knows that red light has a longer wavelength than blue light. A star moving away from an observer effectively ‘stretches’ the light emitted from it, rather like the sound from a police siren is ‘stretched’ as it moves away from the listener. Redshift itself was implicitly predicted by Einstein’s General Theory of Relativity (Gribben 1996: 343), namely that the universe had to be in motion (expanding, or contracting). In 1929 the astronomer, Edwin Hubble, established the relationship between redshift and the position of galaxies, showing that certain galaxies were moving away from us and concluding that the universe was expanding. But of course to do this Hubble had to both know about the characteristics of the light spectrum and be familiar with the predictions of the General Theory in order for his observations to make sense.

Observations are neither passive, nor neutral. They are directed and dependent on an existing conceptual framework of beliefs. The directedness of observation will often take place with the context of an experiment and might be seen as an attempt to isolate and interrogate one part of nature. It is a socially contrived form of observation which is carried out under artificially produced conditions, which are deliberately controlled and therefore capable of being reproduced. By holding other conditions constant it is possible to observe the effects of one variable on another. In doing this the scientist is often mimicking an unexplained sequence of events already observed in nature. It is then an articulation of a problem of what causes *X*. The crisis of bovine spongiform encephalopathy (BSE) in Britain, during the late 1980s and 1990s, illustrates such a scenario well. The first observations by vets and farmers were ‘passive’; though they had a knowledge of animal health and husbandry they were not looking for BSE. However, a large number of cattle had begun to develop symptoms of the disease in sheep known as scrapie (Lacey 1994). The development of BSE in British cattle coincided with the deregulation of animal feedstuffs by the Thatcher government, and permitted the feeding of preparations containing the processed remains of other animals, such as sheep. This much was strong circumstantial evidence, but the job of the scientists was to identify the transmission process. Evidence that contaminated feedstuffs were the agent of transmission from sheep to cattle was fairly readily established, but in order that the disease could be brought under control it was important to establish whether vertical transmission from cow to calf could take place. In 1988–9 the Ministry of Agriculture conducted an experiment in which 316 calves of BSE infected mothers were isolated as a control group and given foodstuffs that were screened for any BSE contamination. Any development of BSE in the calves would most likely be as a result of vertical transmission. Of the control group 19 succumbed to BSE showing that vertical transmission could take place (K. Taylor 1994). Nineteen cases in 316 was considered good evidence that vertical transmission could take place, but had this not been so it would have been possible to conduct a further experiment with a different control group comprising calves born of non-BSE mothers and protected from contaminated food. If a similar proportion (very unlikely) had developed BSE then doubt could be cast upon the hypothesis of a simple vertical transmission mechanism.

3 *Hypotheses*. Hypotheses are specific conjectures about the world that can be tested and are rather similar to the kind of 'low level' theories we have in everyday life. Often it will specify the mechanism by which an effect will be realised, such as in the BSE example above. In science hypotheses may not specify a particular effect in isolation, but instead will specify a measurement, or range of measurements. For example it may be hypothesised that interactions in a particular experiment will produce a temperature, or range of temperatures between *n* and *n*. Finally hypotheses can also be multiple. Any given theory may generate a number of hypotheses and some of these might be mutually exclusive of others.

Induction and falsification

The picture of scientific discovery I have tried to portray is one where the social-psychological processes of discovery in science parallel those in everyday life, but the form they take is specific to the practice of science. Scientific theories are built on an edifice of other theories and laws, which themselves are held to be 'true', but how do we know this is the case? Is it enough, for example, to say that observational experience can corrobrate a theory? As we have seen observations are themselves theory laden, yet the scientist has only theories and observations (and the relationships between these) at her disposal as a means to know the world. Stripped of the language of science this seems to be pretty much the case in everyday life, so what is it that separates science from non-science? For many it is the hypothetico-deductive (HD) model, said to be the golden thread running through science.

The hypothetico-deductive model

The HD model is not the only model of science, but in its various forms it is seen by most scientists as offering the most rigorous route to knowledge. The model traces the path of discovery and justification and can be said to have its starting point in any of its phases. First it is an acceptance that hypotheses cannot be simply derived from observation (because observation is, as I noted above, contextual to begin with) and must arise out of an existing theory. Second, the hypothesis must be conjoined with the initial conditions that exist at the time. By this is meant all of those things in the environment that may have an effect on the hypothesis, or subsequent observations. Third, from the hypothesis and initial conditions a prediction is made which can be tested by observation. Our hypotheses become a clash with reality (Popper 1989: 117–18). In 'traditional' accounts if the observations are successful then the theory from which the hypotheses were derived is confirmed. A simple example would be that we hypothesise that water boils at 100° centigrade and an initial condition would be that this is at sea level. The prediction is that any given samples of water, if heated at sea level under normal atmospheric pressure, will boil at 100°. We then test the prediction by doing just that. The water boiling at 100° can be said to have been deduced from the hypothesis.

This sounds straightforward so far. Meticulous attention to the formulation of a hypothesis, derived from an existing body of theory, with due attention to initial conditions and rigorous experiment seem like an infallible recipe for success. There is, however, a major problem with the HD model, and although Karl Popper maintained he had resolved this, his resolution raised other serious problems.

The problem of induction

The problem is a logical one, that the confirmation of the hypothesis relies on the principle of induction, that is, from the observation of particular phenomena we can come to generalise about wider phenomena. To continue the simple example, if we heat many samples of water at sea level and they all boil at 100° it would seem that we can claim that all water boils at 100° at sea level. The problem is how many kettles of water would we need to boil to make such a claim? Certainly more than one. Five, maybe? Fifty? This problem has a long history in philosophy, but in the twentieth century its articulation became most famously associated with Popper (1959: 27–48). He too illustrated the problem with a simple example. For centuries Europeans believed that swans were white, and this knowledge was just about as firm as knowledge could be, but in the fullness of time (after Europeans first voyaged to Australia), black swans were discovered (Popper 1986: 43). It took only one black swan to ‘falsify’ such a long held theory. Popper goes on to propose a solution to this which keeps intact the HD model, but his solution raises as many problems as it solves.

Popper’s ‘demarcation’ criterion

Because of the logical problem of induction theories can never be shown to be true, never finally confirmed, but they can be shown to be false. Popper’s views on this matter derived from when he was a young man in Vienna. At this time Marxism and the psychoanalysis of Freud and Adler were highly regarded and claimed to be ‘scientific’. Yet as Popper recalls:

> These theories appeared to be able to explain practically everything that happened within the fields to which they referred

> ... [there were] confirming instances everywhere: the world was full of *verifications* of the theory. Whatever happened always confirmed it.
>
> (Popper 1989: 34–5; emphasis in original)

How could these theories ever be shown to be wrong? He contrasts these with Einstein's predictions from the General Theory of Relativity. This, he claims, led to predictions which were risky – they predicted something novel (in this case the bending of light by the gravitational effects of large bodies, such as the sun). This conjecture could be experimentally tested and risked being wrong (Popper 1989: 36). However, if theories and their derived hypotheses can never be finally confirmed, what counts as a good theory? First, it is easy to find confirmations if we look for them. What Popper proposes is that we set out with a different spirit, that of trying to show that we are wrong by proposing the most rigorous tests possible of our theories. If the theories stand up to such tests, they can for the time anyway be accepted. Second, a theory should forbid certain things to happen and the more it forbids, the better it is. Third, theories which do not have criteria of falsifiability are not scientific (Popper 1989: 36).

Popper's views have been controversial since they first appeared in English in 1959. The case against 'falsifiability' has been rehearsed from a number of angles (Lakatos 1970; Jeffrey 1975; Reichenbach 1978; Gemes 1989). Two principle objections can be picked out. First that his falsification (at least in its early guise) is 'naïve' (Lakatos 1970: 95–113), that if it was upheld through the history of science then theories which were initially 'falsified' would have been abandoned. As Alan Chalmers notes:

> An embarrassing historical fact for falsificationists is that if their methodology had been strictly adhered to by scientists then these theories generally regarded as being amongst the best examples of scientific theories would never have been developed because they would have been rejected in their infancy.
>
> (Chalmers 1982: 66–75)

Chalmers cites examples to illustrate this, one of which concerns the fact that Newton's gravitational theory was falsified by observations of the moon's orbit, just a few years after the theory's inception. It took nearly fifty years to show that the causes for this had nothing to do with the theory itself.

A second problem often cited concerns probability. Falsification requires a conjecture to be set out in terms of precise observations anticipated. Einstein's General Theory, for example, predicted the bending of the sun's rays during a solar eclipse. Thus had the rays not been bent, the conjecture would have been falsified. But in much of science results are probabilistic – in social science this is almost universally the case. Much of the justification for induction has traditionally rested on an enumerative principle expressed as the probability of a hypothesis being right (Gower 1997: 189–207), but Popper's falsification principle must lead him to reject this on the grounds that however much evidence is gathered the probability of any *universal* statement is zero. The reasoning for this is that in a closed system of possible outcomes (say the tossing of a coin) we can predict that the probability of heads coming up is 50 per cent, but in expressing the probability of an event where the number of possibilities is potentially infinite we must assume a principle of the uniformity of nature, i.e. that the phenomenon of which we make the inference in general is and will remain approximately the same as the specific phenomena we measured. According to Popper, probability (at least in its usual frequency form) requires justification through a principle of induction (Popper 1959: 29–30). However, apart from rendering a great deal of science unjustified, a rejection of probability seems to be counter-intuitive in everyday life. Bookmakers, as we know, do very nicely out of a reliance on probability – they at least have no problem with inductive inference! Actually, because it seems hard to deny that science uses probability successfully, Popper had to modify his theory in order to show how scientists adopt methodological rules in order to treat probability estimates as falsifiable (O'Hear 1980: 124–32).

A third and very important problem is what counts as a falsification anyway? This brings us back to the social-psychological status of theories. Popper's view was that a theory is a 'bold conjecture' and it actually didn't matter where it came from; what mattered was what you did with it when you had it. It could derive from painstaking years in the laboratory, or could have been dreamt up after an evening's over-indulgence (Williams and May 1996: 31), but once stated it possesses logical properties. On the face of it this seems okay, but in allowing that there may be a social-psychological element in the derivation of theories it is hard to resist the charge that the means of falsification itself may also be prone to social or psychological subjectivity. How can we be sure that these means are more valid than the theory itself? We cannot, of course. Popper's defence is that the observation statements that might falsify a theory are themselves intersubjectively testable within the scientific community, thus in principle also falsifiable (Popper 1959: 95–106). However, we shall see later in Chapter 6, intersubjectivity in science is not without its difficulties.

The aim of Popper's approach was primarily to produce a demarcation between science and non-science (or pseudo-science) (Popper 1959: 42). The unintended consequence of this was to focus attention on the matter of theory choice. Whilst it is logically correct that a singular negative statement can falsify many positive statements, what is important is the status of the claim to falsification and the status of the theory that is to be falsified. As Popper himself admits the decision about whether a theory is falsified is a matter of intersubjective agreement – it is then the outcome of a social process. But so too is the original theory. Although single hypotheses or 'hunches' about data may be born of sudden and individual inspiration, fully formed theories rarely are. Popper's method may be adequate to the testing of specific hypotheses, or parts of theories, but only rarely in the history of science has a crucial experiment falsified a whole body of theory. It may, however, be important that this remains a possibility.

What perhaps lies at the heart of Popper's philosophy is a credo of self-criticism in science, that of the critical attitude. Whilst I will argue later a credo of rigorous methodological criticism is the key characteristic of science, it does not amount to a 'method', nor does it provide a clear science/non-science demarcation.

Nature and the social practice of science

The logical strategy of showing something to be wrong turns out to not be much more helpful to the accretion of reliable knowledge as the one of showing something to be right. Inductive and falsificationist strategies end up failing on similar grounds, that is they depend on evaluation of evidence from the world in the court of human consciousness. In other words the only means we have to assess whether something is right or wrong are our senses and previous standards of corroboration or falsification. The truth about the world will be the truth mediated through human consciousness and whilst this may indeed be the truth, we cannot know that it is. As the philosopher William James pointed out: ' ... theories are a man made language, a conceptual shorthand in which we write our reports of nature' (James 1949: 57). This does not necessarily mean that we can't know reality, just that we can't know that we know reality! Even when we think we have good grounds for saying that we know a particular thing, we can't be sure that what we know is all there is to know about that thing, or even that what we know is correct.

For Popper deductive logic in the form of the falsifiability of statements is the only bastion against the subjectivism of ideology, dogma and caprice in science. Yet scientists, even when using the methodological device of falsification, can nevertheless fall prey to subjectivity in their choice of theory or tests. Rarely do they simply accept or reject theories on the basis of what he calls 'crucial experiments' (Popper 1979: 14), but nor do they arbitrarily choose one theory over another. Discovery and justification so often intermesh in a complex structure of theory choice, probabilistic reasoning and subjective or serendipitous factors. In the remainder of this chapter I will take a brief look at some of these factors.

Theory choice

Observations do not occur innocent of some conceptual framework and in science that is usually one of theory and hypothesis. Observations, then, have a history, but so do theories. Popper's 'falibilism' allows that theories can arise from anywhere and perhaps everywhere, but in actuality this would be rare. Science does not proceed by testing theories in isolation of other theories and moreover the connections between theories, and between theories and earlier observations, both constrain and license the predictions of any given theory. A theory itself has properties other than its predictive content. William Newton-Smith (1981: 226–32) proposes eight characteristics of good theory:

Observational nesting: A new theory should explain observed phenomena as well as its predecessor. Increasing observational success is a primary indicator that we are moving nearer to the truth of the way the world is. Indeed it might be added here that a theory that can explain more should be preferred to one which explains the same range of phenomena.

Fertility: A theory should be capable of being developed further to explain a range of phenomena. In itself this is not enough of a characteristic. Newton-Smith notes that psychoanalysis was a fertile theory, but ultimately did not bear fruit.

Track record: The longer a theory has been around, the more important its track record becomes. What have been its observational successes? A theory with a good track record of success is to be preferred to one with a poor track record.

Inter-theory support: A theory which is compatible with other theories is to be preferred over one that is not. Even if two theories are each successful, but they are incompatible each with the other, then one or other must be incorrect as they stand. Newton-Smith cites the success but incompatibility of Quantum Mechanics and General Relativity as an example of this.

Smoothness: Most theories will explain only part of a range of phenomena it is wished to explain and to provide explanations for the remainder scientists often introduce auxiliary hypotheses. A counter-example will serve here. In this respect Marxism has failed, for in order to explain its predictive failures many more than one auxiliary hypothesis must be introduced.

Internal consistency: Are the various statements in the theory logically compatible with each other? Does it contain internal *non sequiturs*?

Compatibility with well grounded metaphysical beliefs: Scientific theories rest on a foundation of metaphysical beliefs about the world. A theory can in principle deny one or more of these to be true, but mostly such beliefs are not testable. Newton-Smith offers as an example the rejection of the proposal 'that something in the physical world happened because the time was ripe for it to happen' in favour of 'something happened in time to explain the event' (1981: 229)

Simplicity: There has been a historical preference for parsimony in theories, of ontological economy – the principle of Ockham's Razor (see Chapter 1). In other words the more a theory explains in the fewest terms the better it is. Thus two theories may explain a set of observations, but the simpler of the two is preferred provided it is consistent with other known facts. Nevertheless as Newton-Smith observes, simplicity expressed in this way is not always possible or desirable (1981: 231). Quantum Mechanics looks more complex than classical mechanics, but we have good reason to suppose the former to be a closer approximation to the truth and therefore to be preferred.

Others offer slightly different criteria of a good theory, but whichever one adopts the message is simple, that there are several 'tests' of a theory and the more of these a theory passes the more likely it is that it will commend itself to the scientist. Furthermore scientists are not just passive observers of nature and will do their utmost to empirically discriminate between theories through testing the consequences predicted by each theory. Only very rarely will two theories each pass a range of tests made by the scientist. Of course the scientist may end up picking the wrong theory, or both theories may be ultimately wrong (as was the case for wave and particle theories of light). Often though the scientist has only one theory to work with and when the test (often an experiment) result contradicts the theory the scientist will want to know why. A culprit is very often the 'initial conditions' that were assumed, or the instruments used in the test. Finally, though there is no sharp distinction between a theory and a hypothesis, a theory will usually consist of several hypotheses. Obviously if tests failed to confirm any of these then it is likely the theory would be abandoned, or extensively modified. Quite often only one hypothesis fails to agree with test results. It then seems reasonable to conclude that at least some of the theory is right and the search is on to find the bit of the theory that was wrong, or what might be error in other assumptions underlying the 'failed' test.

Inductive inference

Whilst inductive arguments are not syllogistically valid (the conclusion is not entailed in the premises as in a deductive argument), inductive inference seems to be substantively unavoidable. At an everyday level our survival must depend on inductive assumptions – as indeed Hume himself insisted (Hume [1739] 1911). It might well be the case that a child can play in a busy road without injury or death, but it would be a very irresponsible adult that lets it do so. A scientist whose theory predicts certain phenomena, which are subsequently found, is more likely to be right than wrong. That is not to say that the scientist has obtained *true* knowledge, but instead knowledge that is probably true. Alternatively we could say that whilst something is not proven, it is proven beyond reasonable doubt. The inductive reasoning is not 'cold', rather it is located within a framework of facts which are not offended by the findings. For example a concentration of cases of childhood leukaemia near a nuclear power station would *suggest* an association between location and the likelihood of developing leukaemia, given our existing knowledge of the effects of radiation. If on further investigation it was discovered that in all, or virtually all cases, a parent had carried out work on or near the reactor core, and that decontamination procedures were lax, it would be a reasonable *assumption* to associate these prior circumstances with the cases of leukaemia. There may of course be other explanations, but given the known facts this is the most likely. Although, strictly speaking, this is still inductive reasoning because it is still logically possible for there to be other explanations, it is intuitively very like a deductive inference (Couvalis 1997: 53). Such a procedure as this is known as inference to the best explanation.

Probability

The statement that theories are 'probably true' has not satisfied all philosophers of science by any means, but scientists (like bookmakers) would claim to be vindicated by predictive success. Moreover, as this century has progressed science has become more 'probabilistic' in its methods, mainly as a result of the realisation that the world itself is, at the quantum level, intrinsically unpredictable and indeed systems in the non-quantum world may also in principle be non-deterministic (Feynman 1965: 127–48). Thus the idea that science is about simple mechanical cause and effect relationships is simply a persistent myth. As long ago as the second decade of this century Bertrand Russell (cited in Miller 1995) was moved to remark that in advanced sciences such as gravitational astronomy the word 'cause' never occurs. Much of science is probabilistic. Two examples illustrate this.

Brownian motion describes the irregular movement of minute particles of matter when suspended in a liquid. Whilst the movement of any given particle cannot be known, when the liquid is heated the particles move faster, when it is cooled they move more slowly. Aggregate movement can be known and the movement of any given particle could be described probabilistically. The second example is that of turbulence in liquids, such as the water flow in a river. A characteristic of this is aptly illustrated by the game of Pooh Sticks. Pooh and Piglet each throw a stick into the river one side of a bridge and rush to the other side to see which will emerge first. Neither Pooh, Piglet or the watching scientist can determine which will emerge first. Turbulence, like Brownian motion, can be predicted but not determined.

Yet of course assuming the sticks are thrown into the river at the same time and assuming they are of equal weight and the same shape (the initial conditions), then there is a 50 per cent chance of either stick emerging first. The odds can be known. Even if the weight of the sticks differed, or one was thrown into the water earlier than the other, it would still be possible to mathematically calculate the changed probability of each stick arriving first. This is, in scientific terms, an easy problem to calculate, but for Pooh Sticks substitute ecological systems, complex chemical reactions, or the trajectory of a comet – the principle remains the same. Mathematical axioms can be used to calculate the probability of systems, or parts thereof, behaving in particular ways. Though mathematics in general, nor probability theory in particular, cannot themselves tell us much about the truth of the way the world is, they can at least help us to understand and accurately predict relationships between parts of the world. As our mathematical abilities have developed, so has our ability to more accurately predict. The development of Aristotelian logic, the development of the mathematical calculus and of the computer have all significantly aided the process of discovery and justification of findings. And of course the existence of the former two were essential to the possibility of the third as a scientific tool.

Mathematical modelling and simulations have become as important to the scientist as the laboratory experiment. The complexity of simulations, or the ability to deal with very large numbers, is the domain of the computer alone. Nature, it would seem, is too big and too complex to be known in its detail solely through human brain power.

Subjectivity and serendipity

The foregoing indicates that science and its method are much more complex than confirmationist or falsificationist accounts suggest. The HD model can be seen as an ideal type of reasoning, but perhaps more importantly the acceptance or rejection of a hypothesis must be seen in the context of the status of a whole web of theories and the nature of the connections between them. Moreover, findings are rarely ‘true’ or ‘false’, but usually assessed in terms of their probability, often within a hierarchy of knowledge, where the ‘higher’ one goes, the more ‘certain’ the knowledge is. Yet despite this complexity, particular researchers usually focus on just one problem at a time and they rarely have a knowledge of the hierarchy, or how their work affects its epistemological status (Sanitt 1996: 14). Particular standards are inherited, but the work of investigation is not determined beforehand. There will be many false trails, mistakes and reassessments of past work. This process of investigation itself combines several things: first it relies on a set of technical procedures, often particular to a discipline, or subdiscipline;

second, it relies on the provisional belief that certain assumptions are true; and third, it relies on testing both of these through observation, experiment or deduction. The interface between all of these things is constantly in flux and whilst some things are held as constants, others change, though rarely does everything change at once in a discipline. In the history of astronomy and cosmology, for example, the development of optics made observations of distant bodies possible and the observation in turn allowed the development of theory such that we can trace an observational and theoretical history from Copernicus to Hawking. In the last few years alone the concept of 'Black Holes' in space has moved from the 'fringe' science that Isaac Asimov talked of only in 1987 (Asimov 1995), simply theoretical objects, to objects for which there is a growing body of empirical evidence. Black Holes were predicted as a logical consequence of Einstein's General Theory, were theorised by Karl Schwarzschild in 1916 (Gribben 1996: 62), but convincing evidence for their existence was not forthcoming until the advent of powerful radio telescopes, and particularly after the launch of the Hubble Space Telescope in 1990.

The sociologist of science Bruno Latour neatly illustrates the dynamic and indeed serendipitous nature of science in a series of 'flashbacks' directly and indirectly concerning DNA research (Latour 1987). In his first flashback molecular biologist John Whittaker is admiring a three-dimensional picture of the DNA double helix on his computer screen. Whittaker, Latour tells us, is uncertain whether his research programme will yield results, or whether his fellowship at the *Institut Pasteur* will be renewed, but what he can be certain of is 'the double helix shape of DNA and his Data General computer' (Latour 1987: 2). In further episodes Latour tells the story of the difficult development programme of the computer, the elaborate de-bugging necessary and how it was nearly never finished and marketed at all. The narrative of these two stories is punctuated by that of the discovery of the structure of DNA, in 1951, by Jim Watson and Francis Crick. The discovery of the structure of DNA (deoxyribonucleic acid), sometimes called the 'blueprint' of life, has made possible a vast amount of research since in genetics, pharmacology and oncology, and has made possible the 'Human Genome Project' (described in Chapter 6). In 1951 this discovery (like penicillin) was sought and indeed heralded to the point where there was a race between Watson and Crick and the (then much better known) American chemist Linus Pauling. Shortly before their 'breakthrough' Watson and Crick were presented with a paper showing that Pauling had discovered the structure, but appeared to have made a basic error in his chemistry. This is how Latour summarises Watson and Crick's dilemma:

> To decide whether they are still in the game Watson and Crick have to evaluate simultaneously Linus Pauling's reputation, common chemistry, the tone of the paper, the level of Cal Tech's students [students who assisted Pauling]; they have to decide if

> a revolution is underway, in which case they have been beaten off, or if an enormous blunder has been committed, in which case they have to rush still faster because Pauling will not be long picking it up.
>
> (Latour 1987: 6)

Watson, Crick and Pauling each depended on a vast body of 'firm' knowledge, indeed the mistake referred to Pauling's apparent failure to recognise the known part of hydrogen atoms in the structure. Thirty-four years later Whittaker was able to do his work only because he could build upon the even greater body of 'firm' knowledge bequeathed to him by Watson and Crick and DNA research since. In 1951 researchers had no computers (as we understand them), therefore the success of Whittaker's work depends not just on firm knowledge, but on technology and the techniques made possible because of it. Yet in each of the flashbacks we are struck by the serendipity of what happens, or even the luck. It could have been otherwise and frequently in science it is, yet most scientific literature, both specialist and popular, reports only the success of science. Experiments go wrong, theories are misconceived and errors of interpretation and calculation occur.

Latour's narrative is an attempt to present a picture of how science gets done, the nature of contingent connections, of serendipity, of rivalry and of competition. His story reads like soap opera, portraying science as a very human activity, which of course it is and indeed the intention of his work is to deny any useful distinction between science and, for example, politics (Chalmers 1990: 80). Whether or not this view is correct it remains that science getting done is messy with the methods and procedures hard to disentangle from the social relations of science. Whittaker's training would incline him to the formal reasoning of science, but also towards seeing the tools he uses (his computer for example) as a 'black box' and whilst he would have a firm understanding of the biology and chemistry of DNA he may be unfamiliar with more fundamental physical theories. These too would be black boxes. The competitive nature of science drives him towards wanting firm results as much for the sake of his salary and career as simply a thirst for knowledge. Though separated by decades Watson, Crick and Pauling could be similarly described, as could most scientists engaged in research.

Conclusion

At the beginning of this chapter I referred to scientific method as an ensemble of practices and knowledge. The question of this chapter has been what makes scientific method scientific? Popper's falsificationism illustrated the difficulty of pursuing a simple demarcation criterion between science and non-science. I have instead suggested that science is a heterogeneous system of methodological checks and balances involving testing, theory choice, and logical and mathematical reasoning. However, to this we must add the ‘human’ element of science. At a philosophical level what we ‘know’ of the world we know only through our knowledge as participating agents in the world. As Thomas Nagel observed, there is no ‘view from nowhere’ (Nagel 1986). At the level of scientific practice the activity is a social one and it would therefore be surprising if science did not take on at least some of the character of other social activities.

Scientific discovery, though usually directed toward more complex phenomena, nevertheless follows the same kinds of cognitive patterns as everyday discovery. Indeed as Jacob Bronowski (1960) noted much of what was once ground-breaking knowledge often becomes commonplace later. The discoveries of Galileo or Newton are now the basis of common sense knowledge. Yet what has been discovered is to a great extent cumulative and at least partially determines the discovery agenda of the future. The cumulative nature of knowledge and the refinement of the technical means to discovery are enough to account for the complexity and counter-intuitiveness of many of those discoveries Wolpert calls ‘unnatural’. Any activity which has refined its practices and the means to its goals will be unnatural (to a greater or lesser degree) to the outsider. We can comprehend the outcomes of science, but not understand how scientists got there, just as we can comprehend great music without understanding the intricacies of its production.

Though science is in the business of discovery, this is shaped by and shapes theories. But theories come and go. Scientists insist at one point that *X* is right, but later that it isn’t and *Y* is. For example at different times the scientific orthodoxy has supported both wave and particle theories of light, but nowadays neither are seen to be wholly true (Nagel 1979: 143–5). This surely must cause us to doubt all of their findings? There are three things to say here. First, a lot of science remains ‘right’ even after a very long time. Though many of the findings of Galileo or Copernicus are now part of the history of science, they are not wrong. In the few decades since the Watson and Crick discovery we have learnt considerably more about DNA, but their findings remain fundamentally correct. Even though it is commonplace to say Newtonian physics was superseded by Einsteinian physics, it remains the former is still ‘right’, but is limited in what it can explain. Since the advent of quantum physics the aforementioned theories of light have been ‘incorporated’ into a new theory, whereby light travels in discrete ‘quanta’, appearing as waves or particles depending on how it is measured.

Second, any old theory will not do. Scientists hold a concept of good theories and bad theories and as Newton-Smith shows, we can distinguish these on a number of criteria. A scientist’s defence of ‘getting it wrong’ is that, as in everyday life, science learns by its mistakes and in doing so moves closer to the truth. Third, justification in science is complex. Theories do not exist in isolation but as part of what W.V. Quine called 'a web of belief (Quine [1951] 1961). Though he was referring just to theories, we must also include accumulated techniques and standards of testing and inference.

Each of these components of 'good theory' or of justification through testing or inference is in itself neither a necessary or sufficient condition for scientific method. Yet the whole is greater than the sum of the parts, but not all of the parts need to be in place at the same time. Moreover at different times and in different circumstances particular aspects of method will be emphasised. This plurality in method is inevitable because scientists are part of that which they study. The final court of appeal is human consciousness, thus science in general and its method in particular are an attempt to render the workings of the natural world manifest to human consciousness. Yet it does not follow from this that method is subjective, or even arbitrary, but it is necessarily intersubjective. That is, within the scientific community certain procedures and certain knowledge will be taken to be scientific. What counts as method rests on the intersubjective *values* of the scientific community.

At this point controversy arises. Those whom we might loosely term 'realists' will claim that the methodological values of the scientific community arise out of the fact that science discovers objective facts about the world. That is, that the world exists independently of us and a successful method is marked by its ability to reveal the world to us; the 'realist' can be more specific here. She can cite particular values which are general to all science. The first is verisimilitude, or 'truth likeness'. Science aims for increasing our stock of truths about the world. Second, science is a fallible enterprise, that is, scientists can be wrong. Now this is not quite the same as Popperian falsification (though Popper would have claimed it should be), but simply a willingness to take seriously contrary evidence. The third value is that science is logical, being based upon sound reasoning. Although these may be consensual values held by the scientific community they do not simply derive from the social structure of science but instead from their efficacy as a means to explain reality.

Those who we might (again loosely) term social constructionists deny that science is an objective encounter with the world, as suggested by the 'realists'. The values of science do not arise out of any privileged access to nature, but are simply contingent social constructions. In this view science is just one story of many about the world and the privileged knowledge that scientists claim is just a manifestation of their ideological success in convincing us of this.

This controversy is important to social scientists, for if a version of social constructionism is right then any description of studies of the social world as 'scientific' would amount to no more than the claim that social science holds the same set of socially constructed values as natural science. In Chapter 4 I will return to this debate about the social character of science and in Chapter 6 the question of objectivity and social context, but in the next chapter I want to consider the case for the social sciences as ‘scientific’ in the narrower sense of whether or not its investigations can proceed in the same or similar ways to those of the physical world.

Suggested further reading

Gower, B. (1997) *Scientific Method: An Historical and Philosophical Introduction,* London: Routledge.

Newton-Smith, W. (1981) *The Rationality of Science,* London: Routledge & Kegan Paul.

Sanitt, N. (1996) *Science as a Questioning Process,* Bristol: Institute of Physics.