CHAPTER 2

What is science?

In this chapter we seek to ask what appears, at first glance, to be an obvious question. Yet its investigation reveals an ambiguity over definitions of science, its procedures and hence what it is capable of attaining. We are concerned to problematize these issues for two reasons. First, it will enable us to see how the aims and practices of science rest upon taken-for-granted assumptions that, when subjected to critical scrutiny, are often found wanting. Secondly, this will provide us with a basis for investigating, in the next chapter, the nature of the social sciences where there is a dispute as to whether they should replicate or replace the methods of the physical sciences; methods which, as will be noted, are themselves disparate when examined under a philosophical microscope.

Social research is a child of the scientific age. As an investigative discipline, its origins are to be found in a nineteenth century model of physical science. Previously, social thinkers had often confined themselves to general observations about human nature. The seventeenth century had witnessed an emergence of thinkers who, in their attempts to better understand the physical world, began to place at least some emphasis on theory testing. Francis Bacon (1561–1626) and later Isaac Newton (1642–1727), were claiming an empirical basis for their statements about how the world was and how it might be investigated. Following this, there was a growing realization, particularly after Newton, that the "language" of science was essentially mathematical. In its simplest sense, an investigation of the world was a search for the existence or non-existence of phenomena. Things either existed or they did not. If they did, the measurable relationship to other phenomena involved an encounter with number (Losee 1980:86–94).

If the "new" science had met with the level of success achieved by the alchemy or witchcraft of the middle ages, it would probably not have been emulated by those wishing to establish a method of investigating social life. As it was, the success of science lay in the workable technology that was derived from it. The inventions of the nineteenth century were the technological results of the successful scientific theories of that and the previous century. Given the status of the sciences at this time, it is not surprising that the founding figures of what were to become the social sciences were anxious to claim a legitimacy for their work by linking it to what they saw as the success of parallel research in the physical sciences. Thus, Sigmund Freud (1856–1939), an admirer of Newton, entitled an early manuscript of his, "A project for a Scientific Psychology" (Wollheim 1971). Similarly, Marx regarded his project as "scientific". Indeed, nineteenth century thinkers such as John Stuart Mill did not make any methodological distinction between investigations of physical and social phenomena. Mill believed such methods to be equally applicable to the investigation of diverse phenomena (Mill 1987), distinguishing between physical and social phenomena only by reference to the greater complexity of the latter.

The physical and social sciences thus share something of a common history. Although much of this is accounted for by the desire of the infant social sciences to emulate the methods of their successful physical counterparts, the two disciplines have a number of philosophical issues in common. There are, of course, crucial differences in the nature of some of the key problems encountered and we will point to these in the following chapter. For the present, a brief examination of the methods of the physical sciences is valuable for the two reasons we outlined above.

Science: a search for method

There are a number of ways a critical description of science could be presented. None would be entirely comprehensive and all would be controversial. Here, we have two aims. First, to present some prominent views of what scientific knowledge is, or ought to be, and secondly, to convey a sense of the controversy that exists in the philosophy of science. In furtherance of these aims, we will focus in this section on the idea of a search for scientific method.

For those previously unacquainted with such matters, the idea that there should be a search at all, that there can be anything other than a single scientific method, may seem rather surprising. However, what counts as scientific method has long been the subject of dispute. More specifically, controversy has centred upon how knowledge can be justified as scientific, which, in its turn, has been strongly linked to the question of how scientists actually discover things. Yet, why a concern with the attempt to identify the scientific method? Because most philosophers of science have argued that the method used is the only guarantee that the knowledge obtained is valid, reliable and thus scientific. By employing the correct method, the scientist may be sure that their findings are "true", "repeatable" and "generalizable". In this sense, science is method. It follows that if there is more than one method, then there is more than one science. For the majority of philosophers of science, that leads to trouble in terms of its knowledge status.

Let us first ask what is the difference between ordinary everyday knowledge and scientific knowledge? The popular view states that, "Scientific knowledge is reliable knowledge because it is objectively proven knowledge" (Chalmers 1982:1). In turn, this is dependent upon the formulation of scientific theories, which are:

> derived in some rigorous way from the facts of experience acquired by observation and experiment. Science is based on what we can see, hear and touch, etc. Personal opinion or preferences and speculative imaginings have no place in science. Science is objective (Chalmers 1982:1).

This view that scientific theories are derived from the facts of experience is controversial. Nevertheless, it has a long history as an explanation of how science discovers things. As such, we need to examine it in more detail.

Science and the role of experience

The view that scientific discovery is the result of our experience of the world, though traceable to the Ancient Greeks, has its modern origins in the work of the eighteenth century Scottish philosopher David Hume (1711–76). Hume's theory of knowledge (epistemology) is perhaps the best known example of the philosophical doctrine known as "empiricism".

Empiricism may be defined as the idea that all knowledge has its origins in experience that is derived through the senses. Broadly speaking, Hume made a distinction between "impressions" and "ideas". The former, he argued, have more influence upon our understanding. Although complex ideas do not necessarily resemble impressions—you can imagine a mermaid without necessarily having seen one—the parts that make up complex ideas are themselves derived from impressions and impressions are derived from experience. Anything else is rejected as metaphysical speculation. Thus:

> Those perceptions which enter with most force and violence we may name *impressions;* and under this name, I comprehend all our sensations, passions, and emotions, as they make their first appearance in the soul. By *ideas,* I mean the faint images of these in thinking and reasoning (Hume 1911:11. Original italics).

However:

> I observe that many of our complex ideas never had impressions that correspond to them, and that many of our complex impressions are never exactly copied in ideas. I can imagine to myself such a city as the New Jerusalem, whose pavement is gold and walls are rubies though I never saw any such. I have seen Paris; but shall I affirm I can form such an idea of that city, as will perfectly represent all its streets and houses in their real and just proportions? (Hume 1911:13).

On the face of it, the assertion that we discover things by seeing, hearing, touching, smelling or tasting them seems unremarkable. How else could we come to know the world? On the other hand, the claim that reliable knowledge is derived from sense impressions depends on the assumption that we all use our senses in the same way. In other words, if the information received via the sensory organs is the same for two people, each will then possess exactly the same knowledge. This seems, initially at least, plausible. After all, chaos on the roads would ensue if each driver saw something different in the same road sign! However, what we see depends on what we are looking for. Observation is not a straightforward affair for it contains two dimensions which interact in complex ways. They are the cognitive and social dimensions. Let us briefly considered each of these.

From a cognitive vantage point, we select phenomena from the world on the basis of a learned classificatory system. For example, we are able to recognize and classify many different sizes, shapes and varieties of trees on the basis of learned "tree like" characteristics. The more we know about trees, the more sophisticated our classificatory system becomes. The characteristics that differentiate the species must be selected and what we select will depend on our knowledge of the phenomenon. Although most people in Europe and North America can distinguish an oak from a palm tree, distinguishing specific varieties of palm or oak requires prior, systematically accumulated, knowledge. Therefore, though we are all able to select and classify, the process of selection comes from us, not the object as such. For this reason, two people looking at the same object may not see the same thing. When it comes to images on an X-ray, for instance, person A may see an object that person B does not.

We have spoken of the above characteristics as learned. Now although we can learn many things from direct experience of the world—fire is hot, ice is cold, lemons are bitter, etc.—many of the determining mechanisms of the classification and selection process are the result of the social nature of human beings. Although it is logically possible to learn most things about the world through direct perception, much of what we learn and subsequently formulate views upon are social products. A medical researcher investigating the causes of heart disease would begin from the basis of an enormous amount of knowledge and a sophisticated classificatory system. She would not have been employed unless she had undergone a rigorous programme of training and had already amassed considerable experience and data in the area of research. When the observations are made, her selection criteria are based upon the end product of this training and experience. Not only is it likely her observations will differ from those of the lay person, but they may differ from those of an equally experienced colleague employing slightly different criteria of selection.

In recent decades physics, in particular, has become concerned with objects that cannot be directly experienced by the senses. These phenomena can only be known through the means by which they are recorded or, in the case of black holes and sub-atomic particles, reasoned from indirect evidence. The things that are experienced, the presence of a particular radio wave, or the reading on an instrument, are not the things we wish to make ontological claims about. At best, they provide evidence for that which we wish to know and can never, in themselves, constitute direct sensory experience of the phenomenon itself. Indeed, it might be argued that their very nature is the product of theoretical description. From this point of view there is no "neutral" way of knowing them and the way we know them will inevitably be a product of the way that they are described: for example, in cosmology and mathematics (see Ferris 1988, Penrose 1989).

We are now left with the idea that science does not begin from observation, but presupposes a theory to render its observations intelligible. Observations are thus said to be "theory laden". The philosopher Karl Popper (1902–94) recounts an experiment conducted with physics students in Vienna in the early part of the century. He gave them the instruction to pick up a pencil and write down what they observed:

> They asked, of course, what I wanted them to observe. Clearly the instruction "observe" is absurd…observation is always selective. It needs a chosen object, a definite task, an interest, a point of view, a problem (1989:46).

All observations thus presuppose a theory of some kind. As Chalmers puts it in relation to a sentence uttered in commonsensical, everyday language:

> "Look out, the wind is blowing the baby's pram over the cliff edge!" Much low level theory is presupposed here. It is implied there is such a thing as wind, which has the property of being able to cause the motion of objects such as prams that stand in its path. The sense of urgency conveyed by the "look out" indicates the expectation that the pram, complete with baby, will fall over the cliff and perhaps be dashed on the rocks beneath and it is further assumed that this will be deleterious for the baby (Chalmers 1982:28–9).

"Low level" theories such as these are the outcome of a complex relationship between our physical ability to observe and a cognitive selection process shaped through socially obtained knowledge. They only differ from "higher level" theories of science in terms of the complexity of the knowledge obtained.

The problems associated with the acquisition of sense impressions of physical objects are compounded by the non-physical nature of some concepts. How can it be said, for example, that we have acquired concepts such as liberty, honesty or utility through sense impressions? However, we do seem to have these without any corresponding images of things in the world. Similarly, as Immanuel Kant (1724–1804), the German philosopher argued, arithmetic is itself an abstract concept. Our idea of number appears to be quite separate to the things we are counting. Indeed, although we can have a concept of the number 23,468,098 it would be impossible to have simultaneous experience of that many objects. Therefore, Kant maintained that although sense impressions provide the raw material for our empirical knowledge, it is our ability to reason that is responsible for ordering and organizing that knowledge (see Scruton 1982).

It is not our intention to suggest that observation plays anything but a large role in science, it is merely to show that assertions about the prior nature of observation, upon which science is thought to base itself, is highly problematic. Discovery in science is not just about observing the world, as passive receptors of sense data, but is dependant upon the process of active and purposive selection. Therefore, there is a constant relationship between theory and data. This still leaves the question as to how observed and theoretically constituted phenomena are actually presumed to be related to one other. To consider this issue, we need to examine the ideas of "causality" and "association".

Causality and association

Scientific theories are not only about the nature of objects, but the relationships that exist between them. Therefore, theories about objects on their own are usually accompanied by theories about how objects are related to other objects. In particular, what caused an object to be the way it is? For example, the cause of a particular chemical reaction, the cause for a collapsed bridge and the causes of heart disease. From this, it might be said that if we know the cause of an event on one occasion, we will know the cause of an event in the future where the circumstances of its occurrence remain the same. We stake an awful lot on this proposition. A great deal of effort is expended to establish the cause of an aeroplane crash, so that the defect might be rectified. The reasoning being that if the fault caused one plane to crash, it might well be the cause of further crashes. As such, cause is commonly held to be necessary for an event. No events occur without a cause and to explain an event is to know its cause. In these terms, science may be characterized as the search for causes.

Hume (1911) argued that if all we know of the world comes through our senses, then what we know of causes and effects must come to us in the same manner. This being the case, there is nothing in the events themselves to warrant us claiming a necessary connection between them. According to this view, if we observe, for example, that the striking of a match is followed by its bursting into flames, all we can say is that flames ensued after the match was struck. We cannot observe what, if any, connection exists between the two events. However, we can counter this by saying that if objects themselves are not always observable, then it is unsurprising that the relationships between them cannot be observed.

At one level Hume is correct to say that all we see in a cause is what is known as constant conjunction: that is, when event A occurs it is followed by event B. When one pool ball hits another, we see the second move and the only warrant we have for calling this a cause, is that in our experience one pool ball hitting another is followed by movement on the part of the second ball. However, suppose you make the statement "my watch broke because I dropped it". On the face of it, this seems a perfectly good example of cause and effect and in making such a statement, you are actually holding that a causal chain of events occurred. Thus, the watch hit the ground and this impact caused the displacement of a component A that, in turn, stopped component B from working, etc. Similarly, to talk of pool balls hitting each other in a causal sequence could entail the citing of a causal chain involving air pressure, friction, gravity and so on.

One philosophical solution to this question of cause and effect is to employ the concepts of sufficient and necessary conditions. By sufficient conditions, we simply mean that the occurrence of A was sufficient for the occurrence of B. Therefore, a sufficient condition for the breaking of a watch was dropping it. A necessary condition is when B could not have occurred without A. However, A might be necessary, but may not have actually caused B. To take another example. If a match is struck, oxygen can be said to be necessary for successful ignition, but it is not the cause of the match lighting. Matches will not combust simply due to the presence of oxygen. In the case of the watch, a necessary condition might be cited as the disturbance of a crucial part of the mechanism. Given this, to talk of one thing causing another in a straightforward manner is not always helpful for explanatory purposes. Within any cause-effect sequence, we can identify a whole series of relationships that are both necessary and sufficient.

If employing these concepts may be viewed as one philosophical solution, which is still open to dispute, it is not a methodological solution. In order to identify a cause, we need to identify all of the necessary and sufficient conditions. However, at this point we encounter the practical problem of such identification and the logical problem of never knowing that we have identified all of these conditions. If we wish to claim that something caused something else, we may pursue a broad strategy: that is, we can attempt to identify as many as possible of the antecedent conditions of an event. The more we identify, the more we will know. Detectives do this all of the time. It is not enough to say that a murder was the result of a gun-shot wound, nor that Joe Bloggs committed the murder. They also require to know who did it, how, why and when? This may involve pathologist's reports and the statements of witnesses. Thus, we can say that to know more about a cause, although accepting from a logical point of view that we may never known enough, is "sufficient" for the purpose at hand.

The problem with this pragmatic solution is that scientists are not always able to discover anywhere near the full range of antecedent conditions. In such cases, they must fall back on something like a Humean view of causality: that is, constant conjunction. For example, though the claim is made that people who smoke are more likely to develop lung cancer, a full causal description may not be possible even though more and more antecedent conditions are being identified on a daily basis. Although we can say that smoking appears to be sufficient condition for lung cancer, all we can actually claim is the strength of association between smoking and cancer. In itself such statistical associations might be powerful scientific tools, but they should not be confused with a causal description consisting of all necessary and sufficient conditions.

Hume's account of causality does not simply rely upon the idea that we cannot observe any necessary connection between events. This is only one part of his argument. A second part concerns our habits of projecting our past experiences into present or future events. In other words, when we say that the movement of the black pool ball was the result of the white ball striking it, our claim for this is based on past experiences of observing the behaviour of one pool ball when struck by another. This kind of reasoning is called "inductive" and forms a central part of the idea of "science".

Induction

Induction concerns expectations about the uniformity of nature. Each time we drink a coffee, we have certain expectations of taste based on prior experiences. In the same way, we expect rain to be wet, sea to be salty and the sun to rise tomorrow. We have no reason to doubt these things because, in our experience, they have always occurred. Hume regarded this as a basic psychological characteristic of human beings.

Induction can be defined as the derivation of a general principle (or possibly a law in science), which is inferred from specific observations. As such, it can be seen as an important basis for many justifications of scientific knowledge. Scientific experiments are of little value unless they are able to tell us about the world in general. It is also an important claim for scientific method in that it enables prediction of future circumstances. For example, if a scientist establishes that the breaking strength of a particular type of steel bar is 1000kg, her experiment would have little point unless she can claim that a bar of the same composition and construction will have the same breaking strength under the same conditions in the future.

Long held scientific laws are actually based on inductive principles. Take two examples: acids turn litmus paper red and the larger a planetary body, the greater the gravitational pull and as bodies move further apart, the more the force of gravity diminishes. In both cases, we could take many examples of planets or litmus paper and each would produce the same effect if the principle holds. Indeed, most scientists will discover a single instance of a phenomenon via an experiment, or observation, and though they will repeat this under a variety of conditions to obtain confirmation of their hypothesis, what they are actually doing is reasoning from specific examples to general principles. How are these generalizations to be justified?

There are three conditions that must be satisfied in the process of induction. First, the number of observation statements forming the basis of the generalization must be sufficiently large. Secondly, the observation statements must be repeated under a wide variety of conditions and thirdly, no accepted observation statement should conflict with the derived universal law (Chalmers 1982). Once these conditions have been met and a "law" is said to be established, it is then possible for the scientist to both explain and predict phenomena. The explanation of a particular substance turning litmus red is that the liquid is an acid. Alternatively, if our scientist is given a sample of an acid she can predict it will turn litmus paper red.

Though Hume identified induction as a psychological process, he also pointed out its logical drawbacks. For Hume, all argument from experience was the attempt to create a "syllogism". A syllogism can be defined as a statement whereby something other than that which is stated necessarily follows. That such attempts must fail was vividly illustrated by Bertrand Russell in the story of the chicken who was fed every day of his life until the day he had his neck wrung (1980:35). The expectation of food, which had hitherto arrived every day, was dramatically unfulfilled! Such a foundation for science appears rather shaky when we consider that no inductive argument is "safe". Not even the sun rising tomorrow is a certainty.

It might well be objected that these issues are pointless if the sun does not put in an appearance tomorrow. Induction as a problem in the philosophy of science is rather arcane. Despite this, it remains a real problem for all kinds of researchers. Inductive evidence can appear rock solid. Yet the history of the physical sciences is replete with generalizations that are found to be wrong, sometimes after hundreds of years of being considered "right". Consider just two examples. Though it has been known since Copernicus that a central Ptolemic principle of the heavens turning around the earth is false, Ptolemy's geocentric model of the heavens still works perfectly well as a means of navigation:

> For a system that we now consider to be entirely "wrong", it was spectacularly accurate. Ptolemy, for example, calculated the distance of the moon from the earth as 29.5 times the earth's diameter. Our figure is 30.2 (Appleyard 1992:25).

Far from being "irrational", or "unscientific", belief in the geocentric model was backed up by good solid observation and even though an inaccurate representation, it accurately predicted phenomena; for instance, the earth's distance from the moon. Moreover, it had survived for over thirteen hundred years.

A more contemporary example concerns the role of stress in heart disease. Studies conducted in the late 1950s by Friedmann & Rosenman (cited in Golob & Bruce 1990) proposed that hurried, impatient, aggressive and hostile people were more likely to develop heart disease than those who were more relaxed, easy-going and co-operative. Though initially controversial, by 1981 the first kind of behaviour, known as "Type A" behaviour, was officially classified in the US as a risk factor for heart disease. However, studies conducted in the 1980s have concluded that there is no link between personality and heart disease (Shekelle cited in Golob & Bruce 1990). The variations in these findings were the result of different tests and procedures that overturned long and firmly held assumptions.

In the everyday world of science, on the other hand, the problem of induction is not usually confronted head on. Most researchers who make generalizations do so on the basis of the probability of their assertions being true:

> The fact that probabilities can in certain contexts be represented by numerical values led to the hope that one could assign values to the degree to which a body of evidence rendered a theory probable (Newton Smith 1981:216).

If we toss a coin 100 times, we can reasonably expect that heads will come up about 50 times. Simply because there are only two possibilities, this expectation is a reasonable one though, of course, it is perfectly possible that heads may appear more or less than half of the time. Probabilities are usually expressed on much more complex matters. For instance, we could express the probability of a group of 18 year olds becoming unemployed in the next ten years. What is important to note here and in virtually all cases where probability is used, is that we can only arrive at the odds of something happening on the basis of past experience. Just as a bookmaker will offer 10/1 on a particular horse based upon its past form, so researchers decide on probabilities on the basis of what they already know. The probability of our 18 year olds becoming unemployed can only be arrived at on the basis of the odds of a similar group becoming unemployed in the past. Sophisticated models may build in other factors to try to account for changing circumstances, but unfortunately we can never know what these are or the effects they will produce. The inability of economists to accurately forecast growth or shrinkage in any economy is sufficient testimony to this observation.

In employing probability we are faced with two problems. First, like any form of induction we have no guarantee that what is true now will remain so in the future. Secondly, because we cannot know the future, we cannot be *sure* of the probability to assign to particular circumstances. As Chalmers points out:

> Given standard probability theory, it is very difficult to construct an account of induction that avoids the consequence that the probability of any universal statement making claims about the world is zero, whatever the observational evidence…any observational evidence will consist of a finite number of observation statements, whereas a universal statement makes claims about an

> infinite number of possible situations. The probability of the universal generalization being true is thus a finite number divided by an infinite number, which remains zero however much the finite number of observation statements constituting the evidence is increased (Chalmers 1982:18).

That noted, let us not be too critical of probability. Logically and perhaps mathematically, probability may be flawed, but without it many sciences would be more difficult and some, such as quantum mechanics, impossible. That noted, alternative characterizations of scientific method still exist. One of these is the notion of deduction, as opposed to induction.

Deduction and logical positivism

Kant, as noted, held that there are ways of knowing the social and natural worlds other than through experience alone. At the heart of this argument is the distinction he makes between synthetic and analytic knowledge. Hume, in denying that there can be any necessary relations between propositions, overlooked what Kant described as analytic statements. In analytic statements the concept of the predicate is included in the concept of the subject. Thus, "All bodies (subject) are extended in space (predicate)" or, "All senators (subject) are citizens (predicate)." By definition, a body is something extended in space and by definition, a senator must be a citizen. Conversely, the statement, "Some bodies are heavy" though true, is not analytic because the idea of "heaviness" is not contained in the (subject) word "body". This kind of statement is described by Kant as synthetic.

Deductive logic depends on analytic truths. A deductive statement is where the conclusion must follow from the premiss. In other words, just as in the first two examples above, the truth of the conclusion is contained in the premiss. This is generally not problematic in mathematics, but when we use linguistic expressions the truth of the conclusion is not a matter of logical agreement with the premiss, but depends on the truth of those premisses. For example, "All pigs can fly, Porky is a pig and so, Porky can fly." Now, if all pigs can fly then it must be the case that Porky can fly; if he chooses to do so! A deductive statement, though logically correct, is not necessarily a true statement. Conversely, if something is true it does not mean that it is logical. Take the following, "French is the official language of France. This woman is a French citizen. Therefore, she speaks French." The conclusion cannot be derived from the premiss. It does not follow that if the woman is a French citizen she necessarily speaks French. She may be a monoglot Breton, Corsican speaker, or even the daughter of emigré French parents. Although she may speak French, this cannot be deduced from the premisses.

The above examples may seem trivial, but they serve to establish an analytic-synthetic distinction in everyday life that, in science, is very difficult to maintain. It can be illustrated by the everyday example of the boiling point of water. It is commonly held that water boils at 100° centigrade. It should follow that, by definition, a pan of water that is boiling is doing so at 100° centigrade. Not so. Water will boil at a lower temperature at higher altitudes. In scientific method, as elsewhere, deductive arguments are no guarantee of truth.

The role for, or the emphasis placed upon, induction and deduction has important implications for the process of justification in science. Concern about how this should proceed has dominated the philosophy of science for much of the twentieth century. Though the empiricists and their challengers place different emphases on the role of observation and theory in the process of discovery and where inductive and deductive inference might be appropriate, something of a consensus evolved concerning a model of scientific procedure. In this model, hypotheses, as speculations based on what we believe we know, are tested against data. Our knowledge takes the form of existing scientific laws. Thus, if it is a law that acids turn litmus paper red, then it might be hypothesized that it is the acidic nature of a substance that is responsible for turning litmus paper red. Laws can thus be deduced from specific instances and specific instances deduced from those laws. The justification for this form of explanation works only if laws hold for all times and places. The problem is, of course, that the laws themselves, as noted, are the product of induction. In other words, the "law" that acids turn litmus red is simply obtained from the evidence that all acids tested so far on litmus paper have resulted in it turning red.

Given the above, it is not surprising that the problem of induction has plagued scientists since the time of Hume. As a problem in logic, it was considered insoluble, for all logic can do for us is to specify relations between concepts and must be silent on any "real" nature of these concepts. These things were recognized by the Logical Positivists in the 1920s and 1930s. Their attempt to ground scientific knowledge in principles as sound as those of mathematics, may be characterized as one of the heroic failures of modern philosophy.

Logical Positivism was born out of the work of a group of scientists and philosophers collectively known as the Vienna Circle (Von Mises 1951). The logical positivists were the most radical of empiricists, denying not only that we could identify any form of natural necessity in the world but that, in principle, we could never come to know the *real* world. We can never get behind what is apparent to the senses. It follows that all we can describe is that which we can know through our observations of the world. Anything else is regarded as speculative metaphysics. This is a view known as phenomenalism. This led the logical positivists to advocate a way of "doing" science that was based on the validation of theories by the use of "elementary observation statements": that is, simple statements about direct and basic observations. Whether something was the case or not could only be verified through the observation of phenomena. On this basis, if something is not observable and therefore verifiable, then we are not entitled to make claims about it:

> we have to proceed from that which is epistemically primary, that is to say from the "given" i.e. from experiences themselves in their totality and undivided unity…The elementary experiences are to be the basic elements of our constructional system. From this basis we wish to construct all other objects of pre scientific and scientific knowledge (Carnap 1969:108–9).

Logical Positivism did not so much ignore the problems of observation and induction. Instead, it recognized them and held them to be insoluble. If science cannot be based on observables, then what can it be based on? Moreover, verification through observation will give the most certain knowledge that it is possible to attain.

The expurgation of metaphysics from science was criticized most notably by Karl Popper. As he wrote of the logical positivists:

> in their anxiety to oust metaphysics, [they] failed to notice that they were throwing all scientific theories on the same scrap heap as the "meaningless" metaphysical theories (Popper 1989:259).

Because of their speculative nature, theories always contain an element of metaphysics. Arguably, they only stop being metaphysical when a system of testing them is constructed. Thus, theories about Black Holes, for example, are inevitably metaphysical because they remain, at present, unverifiable from an empirical point of view. However, if and when means of testing the theories are found, then they will cease to be metaphysical. A demarcation between metaphysics and meaningful propositions, therefore, is the ability to define methodical tests. The problem is if we cannot admit of theories, such as those about Black Holes, because they are metaphysical, then we will never test them simply because we rejected them in the first place!

This latter issue is closely connected to a further problem. This concerns the logical positivist need to separate the language of observation from the language of theory. The language of observation must be neutral and "uncorrupted" by theory, if the verification principle is to hold. This can be illustrated by a simple example. The theory to be tested is that substances S1, S2, S3 are acids. It was observed that when litmus paper was placed in substance S1 and S3, it turned red. Therefore, substances S1 and S3 are acids. The language of the observation in this case only makes sense if acids are defined as substances that turn litmus paper red in the first instance. The observation is necessarily theory laden. For this reason, the way that we describe observations cannot be separated from our theories about them. This example is an oversimplification, but even when logical positivism was at its philosophical height, the complexity of sciences, particularly physics, made the separation between observation statements and theoretical statements a practical impossibility.

Logical positivist attempts to build a justificatory framework for scientific knowledge were killed off by a close associate of the Vienna Circle—Karl Popper. Here is his confessional:

> Everyone knows nowadays that logical positivism is dead… "Who is responsible?" or, rather the question "Who has done it?". .. I fear I must admit responsibility (1986a: 88).

His murder weapon was "falsification" and his motive twofold. First, Popper was highly critical of verifiability and indeed any attempt at justification in science. Secondly, he wished to mark out that territory that belonged to science; a goal he shared with the logical positivists. For Popper these are interconnected:

> I understood why the mistaken theory of science which had ruled

> since Bacon—that the natural sciences were the *inductive* sciences and that induction was a process of establishing or justifying theories by *repeated* observations or experiments—was so deeply entrenched. The reason was that scientists had to *demarcate* their activities from pseudo science as well as from theology and metaphysics (1986:79. Original italics).

The need for justification was the need to show why science was special in that knowledge derived through scientific method was superior to other forms of knowledge. The actual problem of demarcation of science from pseudo science was the subject of Popper's first book in 1929, *Logik der Forschung* (published in English as *The Logic of Scientific Discovery* in 1959). Hume, it will be recalled, while noting that induction could not be logically justified, "explained away" our tendency to rely on it as a psychological pre-disposition. Popper claimed that this was mistaken (1979:85–90). His solution to the problem of demarcation rests on the need to logically solve the problem of induction. He achieves this by side-stepping it. The core of Popper's falsification can be stated in the following terms: although any number of observations can never conclusively prove a theory, one disconfirming observation is sufficient to refute it. This is no more than Hume had already argued. However, what was unique in Popper's formulations was his insistence upon characterizing science as a search for *disconfirming instances.*

A scientific theory, as opposed to a "pseudoscientific" theory, is one open to falsification. Here, it is stated in the specification of the theory what will count as crucial tests. If the theory fails these tests, it is falsified. Now, the logical positivists had allowed that theories may be falsified. The difference between them and Popper lies in their idea that if a theory passes the tests, it is confirmed, whereas Popper maintained that all this meant was that the theory was not falsified on that occasion. As such, Popper maintained that all theories remain conjectures and are open to refutation. Incidentally, this leads Popper to maintain there is no logical distinction between a theory and a hypothesis. A traditional view that hypotheses are unproven theories is turned on its head when theories are considered as conjectural (Popper 1986a: 81). It follows from this that laws in science are conjectures and that no part of science is safe, not even the tests themselves which can also be falsified. One question, however, remains: how can science ever make progress in the absence of laws upon which to build new knowledge?

Popper answers this question in two ways. First, a theory that "survives" all possible tests may be considered better than the theories it replaces. Theories must always contain testable propositions and scientists who advance them should specify, in advance, what are to be the crucial tests. A competition between theories, or what Popper describes as "a kind of Darwinian struggle for survival" (1986a: 79), then ensues. This will produce the best theories available to science at a given time. Secondly, by eliminating "untruth" through the falsification process, science moves closer to the truth. Although "truth" may never be attained it follows, according to Popper, that the elimination of error must leave fewer candidates for this accolade. The aim of a good theory is "truth likeness", or "verisimilitude" and the elimination of error (1989:228–38). Moreover, unlike the logical positivists, Popper is a realist, which for him means that conjectural statements are about things in the real world. Yet can we know this?

> the procedure we adopt may lead…to success, in the sense that our conjectural theories tend progressively to come nearer to the truth; that is to true descriptions of certain facts, or aspects of reality (Popper 1979:40).

If things, therefore, were not "real" how could they confound us by showing our theories to be inaccurate? Falsification represents a head-on clash with reality and Popper maintains that progress in science must be through a process of learning by mistakes. A preparedness to do so marks out a scientific attitude, unlike inductivist approaches in which demarcation between science and pseudo science cannot be maintained on logical grounds.

Though himself never a Marxist, Popper moved in an intellectual circle in 1920s Vienna where Marxism and psychoanalysis (in the work of Freud and Adler) were regarded as scientific. Indeed, these theories appeared to be able to explain practically everything that had happened within the fields to which they referred. Nevertheless, a problem remained:

> The most characteristic element in this situation seemed to me the incessant stream of confirmations, of observations that verified the theories in question…A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history (Popper 1989:34–5).

For Popper, this resulted from a reliance on inductivist approaches to the accumulation of knowledge. It was mistaken. If Marxism and psychoanalysis were sciences then what marked them out from astrology or religion? Popper's answer was that from a logical point of view—nothing!

Popper thus shifted the focus of science from justification to discovery through error elimination. His contributions to the philosophy of science were far from being any kind of definitive, or final statement, but more the key that opened a Pandora's box. This Pandora's box was less a result of his falibilism and more a result of the introduction of personally subjective and social psychological criteria into the process of science. It will be recalled that Popper had taken issue with the empiricists, not just on the question of induction, but also on the grounds that observation is not neutral, but is theory laden. Despite this, his ideas are often and somewhat mistakenly regarded as variants of logical positivism, or at least as being very close to this body of thought. Yet he accepts that theories may have their origin in personal psychological factors, or that they might appear in a flash of inspiration, but what is important is what is then done with them. If theories are to be tested on the basis of "clashes" with the real world, then at some point scientists must rely on observational evidence. Surely, however, observation statements are themselves suspect.

Popper's response to this question is to introduce a "social psychological" element into his argument. For him, "basic statements" (observation statements) are intersubjectively testable. Though he insists that these observations are themselves open to refutation (Popper 1983:111), it remains that any decision on whether or not a theory is falsified is the product of agreement between scientists at a particular time. This, in turn, depends on their seeing the same thing, or at least agreeing that they saw the same thing. Popper always maintained that science was a discipline "without a subject". Nevertheless, by allowing that the theories of science may have been arrived at purely fortuitously he admitted of the existence of subjective criteria in the research process. Theories could have been derived after years of painstaking methodical work, but could just as easily have been dreamed up after the consumption of a large meal and several beers!

Aside from the entry of subjective criteria into his formulations, on closer examination falsification as a basis for science begins to look a little shaky. What can count as falsification is subject to the same problems as what can count as a verification. If a scientist proposes a critical test of a theory and the theory fails that test, can the theory be said to be falsified? The test itself and the observations are just as fallible here as in traditional inductivist accounts. The scientist arrived at the specification for the test via the same set of cognitive and social processes that dogged the selection criteria for the poor inductivist. Additionally, the observations themselves might be wrong for exactly the same reasons. To claim, as did Popper, that the tests themselves are open to falsification does not help because to "falsify" the test we would need a further test and so on. The result is a regress to infinity.

Finally, there remains a logical problem in Popper's ideas. If induction relies on an unwarranted move from particular instances to generalizations, then so does falsification. Why should something falsified at time *T1* remain falsified at time *Tn?* Chalmers offers the following example from the history of science:

> In the early years of its life, Newton's gravitational theory was falsified by observations of the moon's orbit. It took almost fifty years to deflect this falsification on to causes other than Newton's theory. Later in its life, the same theory was known to be inconsistent with the details of the orbit of the planet Mercury, although scientists did not abandon the theory for that reason. It turned out that it was never possible to explain away this falsification in a way that protected Newton's theory (1982:66).

Science is littered with examples of instances where a "falsification" was later overturned, while scientists have persisted to hold on to theories even after they appear to have been falsified, only to be proven correct in the long term.

Science: a psychological and social process

The challenge to falsification, though initially from within a deeply conservative philosophy of science establishment, led to the questioning of science as a *rational* discipline. If falsification and induction must rely on observation statements that appear to be rooted in some subjective criteria, then the question of how science actually proceeds and therefore what it is becomes broadened.

The most influential of these challenges came from Thomas Kuhn

(1970). Kuhn claimed that the history of science offers little comfort to either traditional or falsificationist accounts. Science, he maintains, is periodically driven by crises. This has been the case since the emergence of a first scientific consensus from a "pre-scientific period". Science consists of periods of "normal science", where scientists engage in "puzzle solving" within the confines of a particular "paradigm". The paradigm is the mark of a mature science. It comprises the intellectual standards and practices of a scientific community, but more than this it is based upon shared metaphysical and philosophical assumptions. Laws are held to be axiomatic and puzzle solving within a given theoretical structure. Dissidence from the key tenets of the paradigm are not tolerated, although most scientists remain uncritical of the paradigm they are working within. However, in the process of puzzle solving, anomalies will occur. Key theories will appear to be falsified. When this happens frequently and particularly when key scientists themselves begin to challenge the orthodoxy, crisis occurs. The crisis then spreads and becomes a revolution where a new paradigm becomes established with a new set of laws, theories, intellectual standards, etc.

For a thousand years, until Copernicus, the geocentric model of the universe prevailed. We have mentioned above that Ptolemy had devised a complex and workable navigational system based upon these assumptions. Navigation, over this period, became sophisticated and accurate, but the geocentric basis of Ptolemy's work was questioned by Copernicus in 1514. Copernicus suggested that the stars were very much further away than previously thought. More controversially, the apparent motion of the stars at night and that of the sun by day, was the result of the Earth rotating on its axis. So controversial were his views that even after his death his work was banned by the Church and remained so for over 200 years. It was not just the Church that was critical; the celebrated astronomer Tycho Brahe dismissed Copernicus's revolutionary theories. However, within the space of 50 years the "Ptolemaic paradigm" was replaced by the "Copernican" one.

The above example illustrates how *sociological* factors—religious orthodoxy in this example—can be important considerations in the determination of the legitimacy of scientific claims. In this case, the "sociological content" came from outside the community of science, though it must be said that in the sixteenth century a separation between science and religion was less pronounced. If, as claimed above, theories are determined not only by what there "is", but also by that which is prioritized, then we can see how various orthodoxies, whether they be religious or secular, can play a part in determining what the priorities of science will be. Until very recently, a challenge to the idea of mathematics as regular and deterministic would have been regarded as absurd; within a generation indeterminacy itself has become orthodoxy. To what extent the former "deterministic" view of science was the result of "rational" processes and to what extent it was a socially held convention, is now a matter of debate.

Paul Feyerabend went even further than Kuhn in claiming that historical, sociological and psychological factors determined how science progressed. His own reading of the history of science led him to conclude that the case studies examined

> speak *against* the universal validity of any rule. All methodologies have their limitations and the only *rule* that survives is *anything goes* (Feyerabend 1978:295–6. Original italics).

For Feyerabend, science is a kind of "playful learning" where new meanings are grasped, just as a child grasps new meanings that they play with for a while and then abandon. The "play" process itself uncovers new meanings and is the method of discovery. It follows that there is not, nor can there be, any "one" method of science.

Unlike the inductivists who believed in scientific progress through the gradual accumulation of knowledge as a hallmark of science, Kuhn maintained that "progress" comes through revolutions. To think of science as gradually evolving misses the importance of paradigms in determining what science looks for and the manner in which it proceeds. Nevertheless, in his later work Kuhn offers some universal characteristics of a good scientific theory. These include accuracy and consistency (within the standards of the paradigm), broad scope, simplicity and fruitfulness (Kuhn 1977:321–2). Feyerabend does not go this far and takes an anarchic view of knowledge. This is clearly of importance to the question, "what is science?"

Both Feyerabend and Kuhn have been accused of relativism. Relativism entails the view that there are no universal, ahistorical standards to which scientists might allude in justifying their methods and findings. In science, it means that no one theory is "better" than another. What is considered a true, or better, or worse theory, is the product of the community in which the theory is devised. Feyerabend gleefully accepted this accusation, whereas Kuhn responded with his definition of the characteristics of a good theory. Nevertheless, if as Kuhn claims, the certainties of an earlier paradigm cannot be carried over into a new paradigm, the implication is that the theories of each are not comparable.

Kuhn's conception of paradigmatic change has been criticized in a number of ways, not least for the inconsistencies in describing what comprises a paradigm and how paradigm shifts actually take place (Lakatos & Musgrave 1970). Space does not permit us to elaborate upon these issues. However, what is important here is that if relativism is right then any claim that science is superior to, say, astrology, witchcraft or voodoo, collapses. Science is just an historical manifestation of knowledge claims to be treated in exactly the same way as any others claims to knowledge, the result being that conceptions of science as a more accurate picture of the world are without foundation. In this respect the problems for "science" are twofold. There is, first, uncertainty about the status of what we observe and, relatedly, there appear to be not only difficulties in testing theories, but any number of theories can describe the same set of observations. In this sense, theories are said to be under determined. For example, Ptolemy and Copernicus both looked up to the same sky, apparently saw the same sun and stars, but arrived at quite opposite theories.

If it is the case that at different times incompatible theories are used to explain precisely the same empirical evidence, then we can never be sure that science is correct in its assertions. Scientific proof is a worthless currency that will be superseded by later scientific proofs. This is precisely the argument that one set of protagonists deploys in a controversy that has raged in rural England for several years. Since the mid 1980s, a series of "crop circles", patterns of intricate and sometimes beautiful designs, have been appearing in the ripening crops across several counties in the South of England. The "circles" invariably appear overnight and can be hundreds of metres in diameter.

Three explanations appear to be on offer for this phenomenon. First, a "scientific" inductive explanation whereby each new sighting is seen as evidence for an explanation of "plasma vortices"—a rather similar effect to that of "ball" lightning. The proponents of this explanation have needed to modify their theory many times to accommodate troublesome anomalies, to the point where the more recent theory is very different in form from the original. A second group has attempted to show that crop circles can be produced by hoaxers. Until a couple of years ago, this explanation was ruled out on the grounds that there were too many circles that were too complex to be "artificially" produced. Nevertheless, a group of academics from Southampton University did manage to convincingly fake some circles, fooling both the orthodox scientists and the "para scientists". This latter group favours some form of paranormal explanation that ranges from extra terrestrial visitations to various psychic powers. Not surprisingly, the orthodox scientists mock the latter explanations as "unprovable" or "unscientific". The para scientists' conjectures may well be unprovable, but then so are the orthodox ones. Indeed, in the gathering of data to support their own theories the para scientists have been equally as scientific as the orthodox ones. On the face of it, it is hard to see who is Ptolemy and who is Copernicus in this instance. Perhaps neither. The self styled "Wessex Sceptics" from Southampton University, despite the remaining objection from their opponents that not all the circles could be fake, may end up in the role of "Copernicus" in this dispute.

We end up with the implication of relativism being that one explanation is as good as any other. There is no one "truth", but many truths that are particular to time and place. On this basis, science becomes a social product that varies across time and is united in name only. We shall return to the question of relativism later. Relativism as a problem, or a solution for some, ends up being a question of what is regarded as truth. Therefore, we need to briefly digress and pursue this question.

In pursuit of the "truth"

We noted above that a statement that is logically correct is not necessarily "true". However, what do we mean by "true"? Any attempt to answer this question comes up against the problem that most things in the course of human history that have been regarded as true at one time, end up being seen as "false" at another. As such, it seems reasonable to suppose that those things we regard as true now will not be necessarily so in the future. This problem has led philosophers to arrive at different theories of truth. There have actually been many theories of truth, though we will confine ourselves to briefly considering three versions in order that some feel for the complexity of the topic might be obtained.

The first idea of truth is the most intuitively obvious. It is the *correspondence* theory. Quite simply, something is true if there is agreement with the facts. This definition has a resonance with logic. It requires an agreement between premiss and conclusion, yet it entails two important difficulties. First, there is the logical problem of sentences such as, "this statement is false". If the sentence is true then what it says must be true—that it is false, but if it is false then what it says must be false, so it must be true! The logician Alfred Tarski attempted to solve this problem by saying that the truth of a sentence can only be established in a further sentence(s) (Popper 1989:116). Nevertheless, this requirement for a "meta-language" appears counter intuitive and complex. Secondly, most of the really important questions are matters of dispute about the constitution of the facts. It is all very well saying truth is agreement with the facts when we can agree what the facts are. A government may claim that its citizens are richer now than in the past and the facts offered to demonstrate this may seem to support it. However, this claim will always be dependent upon what is meant by "richer" in terms of the use of relative or absolute measures.

Dissatisfaction with the correspondence theory of truth has led many to adopt various "intersubjective" versions of truth. The commonest of these is the view that coherence between propositions is itself a criterion of truth. This does not just mean statements must be consistent. If it is said "ripe strawberries are red" and "Paris is in France", there is no disagreement, but there is no coherence either. Coherence is a stronger relationship. However, there is intersubjective agreement that ripe strawberries are indeed red and Paris is in France. There have been numerous reports that have confirmed these things to be the case. Reports have been coherent with each other and there have been no reports to the contrary.

This view of truth is also problematic. First, statements might be coherent, but this does not make them "true". Truth lies only in the veracity of a number of statements, not in the thing itself. There are millions of coherent reports that koalas are bears. Therefore, it is true that koalas are bears. Yet this says nothing about the koala itself that actually turns out to be a marsupial. All those coherent reports were wrong. Secondly, despite the reliance on coherence between propositions, what is intended is not agreement between statements, but to make a statement about the "thing" itself. Coherence collapses into correspondence simply because there has to be at least a perceived agreement with the facts. Thus, the thousands of reports of koalas as bears were coherent, but wrong, whereas a single report of koalas as marsupials was correct because of a better agreement with the facts.

Finally, the American philosopher William James argued for a *pragmatic* theory of truth, whereby something was true if it was useful and of benefit for it to be true. This was not just a matter of expediency:

> Grant an idea or belief to be true…what concrete difference will its being true make in anyone's actual life? How will the truth be realized? What experiences will be different from those which would obtain if the belief were false? What in short is the truth's cash value in experiential terms? (James quoted in Ayer 1982:79–80).

In this version of truth, the focus shifts from the property of a thing to how we think about a thing. In the correspondence theory of truth something is true for all time. Although coherence definitions are still concerned with the truth of a thing, though this may change, truth for the pragmatists is not fixed or immutable, but something that happens to an idea itself, not to the thing to which it refers. It becomes true if it can be assimilated or validated in a community of experiences.

James suggests that our desire to eliminate error rather than seek truth leads us not to choose between propositions, whereas if we choose one or the other we have an even chance of being right. Many objections have been raised to this definition of truth. In particular, it is often not a question of deciding whether something is true or false, but a matter of assigning degrees of belief to the matter. For example, suppose you see a man in a crowd who resembles the picture you have seen of a dangerous criminal wanted by the police. The person you have seen may or may not be the criminal but, all things being equal, it is more likely he is not. Further, imagine now that your decision to believe must rest not on the degree of probability you assign to the likelihood of the man being the wanted criminal, but instead to the effects of your belief either way! If you are right, society is safer and you reap a reward. On the other hand, if you are wrong an innocent man is arrested. Your decision depends on what you see as the "best" outcome. With a little imagination, it can be seen that the moral dilemmas involved in adopting such a criterion of truth become both complex and problematic.

The outcome of this short discussion of truth is not a happy one for either the philosopher or scientist. Even what counts as truth is without a consensus! Adherents of the correspondence and coherence view are ultimately concerned with "reality", whereas the pragmatists are equally concerned with how we view reality. Although the correspondence theory of truth claims to refer to reality it simply ends up being about semantics—logical rules between statements. There is also a blur in the pragmatists' argument between describing the production of truth as taking place within a community of experiences and an advocacy of the notion of truth as expediency.

Social interests and scientific practices

At every stage subjective criteria enter into the scientific process. Indeed, even what counts as truth is not beyond dispute. Our choice of problem may be attributed to psychological or social factors as is choosing what is to count as a test. What holds as a solution to a scientific problem can likewise be socially determined. That social factors determine not just the subject matter of science, but also how science itself is done, has been the thesis of a particular group of sociologists of science in the last two decades or so. This view is associated with the work of Barry Barnes (1972, 1974,1977), Harry Collins (1975), David Bloor (1976) and Steve Woolgar and Bruno Latour (1979). Much of their work was inspired by the emergence of psychological and social concerns to the debate about method in the 1960s (for example, see Lakatos & Musgrave 1970) and was much influenced by the work of Kuhn and Feyerabend.

For this group of scholars our understanding of science has been flawed by our reliance on the "internal accounts" of science that are themselves used to explain science. In other words, scientific rationality is itself a product of science and is just as suspect as those aspects of science that are seen as rational or irrational. To explain science, they argue, we need a sociology of scientific knowledge:

> The sociologist must ask what it is that guides the research of a scientific speciality, what makes it a coherent social phenomenon, and what makes its rapid rate of cultural change feasible. He must seek a description of normality and change within the speciality (Barnes 1974:48).

Like Kuhn and Feyerabend, this work is located in the study of specific episodes in the history of science or, in the case of Woolgar & Latour (1979), studies of what actually goes on in the laboratory. These studies point to socially determined reasons for both the substantive and intellectual content of science. It is not just the agendas of what are interesting problems for science, or how results are seen, but also the actual process or method itself that is socially determined. Let us take some examples to illustrate this focus.

Foreman (1971:109) maintains that the willingness of scientists in the German Weimar Republic to accommodate themselves to fashionable thought was such that they were prepared to abandon any principle of causality—one of the cornerstones of physics at that time. Now, although causality in the sense understood since Hume was to be later challenged by quantum physics, it was still used as a justification for the scientists' decision to "abandon" causality. Therefore, in this process science was reconstructed to appear as if the decision was scientific, but it arose from a desire among the physicists to win public acclaim in a climate where "spiritual values" and the "mystery of things" were fashionable (Brown 1989:13). Similarly, Shapin (1975) claims that the enthusiasm for phrenology in the nineteenth century was not the result of the power of the scientific explanations of its "founder", the Viennese doctor, Franz Joseph Gall, but is explained through its adoption by influential people in the social reform movement, such as George Combe (Shapin 1975:232). Given such examples, there are those who have argued that not only is "respectable" science wholly shaped by social concerns, but that the only difference between science and the "para" science of "ufology", for example, is that the former has attained its prominence only because it is favoured by intellectual elites (Blake 1979).

The implication of these forms of approaches to science is to "debunk" it as a form of enquiry superior to any other. Thus, an account of social science that rests upon claims to scientific legitimacy is appealing to a specious concept. However, it is not this simple (things rarely are in philosophy). Although on the face of it a sociological account of science may be appealing to social scientists in a quest for intellectual justification, the account has not gone uncriticized, even from those sympathetic to what is known as a "post-Popperian" philosophy of science. For instance, Chalmers has criticized a number of the accounts of these particular sociologists of science as being based on a misrepresentation of science, which emphasizes bad science and the extreme empiricism and rationalism of (what they term) the standard account of science (Chalmers 1990:83). This is exemplified by the claim that scientific theories are underdetermined by the evidence available and that there is never enough evidence to make rational decisions (Brown 1989:7). As a result, any number of theories can be used to explain the evidence:

Therefore extra-scientific social factors enter into the processes that

> lead to the selection of one among the perhaps many possible theories compatible with the evidence (Chalmers 1990:84).

However, in practice, "scientists often struggle to find *any* workable theory compatible with some problematic evidence" (Chalmers 1990:85). What is occurring here is that the chosen episodes are being made to fit the sociologist's own theory and are not representative of what actually happens in the day-to-day business of scientific practice.

In addition, if it is argued that scientific beliefs are socially derived, then so are all beliefs:

> if all beliefs are socially caused, rather than rationally well founded, then the beliefs of the cognitive sociologist himself have no relevant rational credentials and hence no special claim to acceptability (Laudan 1977:201).

Thus, the sociologists of science are selective in their description of scientific practice and, according to Laudan, guilty of the same kind of universal justification for knowledge that they accuse science of possessing.

We are now left with a further issue to contend with in answering our question: is the production of scientific knowledge the result of rational scientific procedure, or is it socially and/or psychologically determined? If it is the latter, then scientific knowledge, as superior to any other, appears to be undermined. On the other hand, if the philosophy of science, or sociology of science, cannot explain science, why it is that we appear to know considerably more now than five hundred years ago? Quite clearly, there is something going on in the laboratory and something which has had and will have, an enormous effect on our lives. Moreover, it might also be said that to know science is to know of its dangers and potential excesses, thus allowing us to use it as a tool for the betterment of humankind.

The above question has two concerns at its heart. First, what is "rational belief" and is this a characteristic of science? Secondly, what is the ontological status of the things that science investigates? We shall return to the first of these questions later, for the tackling of this problem from a social scientific viewpoint offers insights that are of value in understanding the physical sciences.

The question of the nature of things that exist is a very old one in philosophy and the debate above is merely a new angle on an old problem.

The problem is a metaphysical one at heart that can be reduced to a simple question: do the things that we know of in the world really exist? On the face of it this may seem to be a trivial question and for most of the time it is not really an issue. However, consider the problem of the quantum physicist. The objects she deals in are not just very small, they simply cannot be observed and the only knowledge she will have of them is the "effects" they produce, which are only known through very sophisticated means of measurement. Do these objects exist, or are they a product of the theories we have about them?

There are two views on these matters. The first is idealism, which is the view that the external world is the product of mind. This can take the form that all material objects consist of nothing but ideas, or those things that we perceive in the world are just appearances and have no independent existence outside our thoughts. The former view is associated with the eighteenth century Irish bishop, George Berkeley, and the latter (sometimes called transcendental idealism) with Kant. The second set of views are known as realism. As with idealism, they take different forms but can be summed up as claiming that things in the world have a real existence, independent of our thoughts about them.

Whether one adopts an idealist or realist view, will make a huge difference to what we can say about the social and natural worlds. The methods advocated often point to a view on the "nature" of things. For example, whereas empiricism is neutral on whether things exist or not, the view that all we can know of the world is that which we perceive through our senses is, by default, an idealist one. Contrast that with Popper's principle of verisimilitude as implicitly realist in its postulation of getting closer to the "truth"—to what is real—through the elimination of error.

In the debate over the status of scientific knowledge we must classify Kuhn, Feyerabend and the "sociologists" of science as idealists. Why? Because they are committed to the view that scientific theories are the product of minds, but more importantly, what counts as their verification, or falsification, is also a product of mind(s). Now this does not mean that such decisions about theories are produced by individual minds in isolation but, as is especially stressed in the above sociological accounts, social products. In other words, the scientist adopts or rejects a theory on the basis of a decision not made with regard to a "real" state of affairs pertaining to that which is being investigated, but on the basis of social and psychological criteria. Moreover, there is no "real" state of affairs to appeal to, but just so many theories that are themselves the product of mind or, in this case, collective minds.

The plausibility of this view has been questioned by a number of philosophers who point to important differences in the classes of things we want to make knowledge claims about. As Laudan puts it:

> There is an enormous amount of evidence that shows that certain doctrines and ideas bear no straightforward relation to the exigencies of social circumstance: to cite but two examples, the principle that "2+2=4" or the idea that "most heavy bodies fall downwards when released" are beliefs to which persons from a wide variety of cultural and social situations subscribe. Anyone who would suggest that such beliefs were socially determined or conditioned would betray a remarkable ignorance of the ways in which such beliefs were generated and established (1977:199–200).

Nevertheless, Laudan goes on to point out that it does not follow from this that science may be characterized as wholly rational in its decisions and formulations. It may be bad science and there may be a lot of it, but it still remains the case that the studies produced by Feyerabend, Barnes, Bloor, Shapin, etc. were records of how science was performed. Here, Laudan proposes what he calls the "arationality assumption" (1977:201). Briefly put, if we can explain a belief as being the result of the rational examination of the evidence, we should assume it to be the correct explanation. If, on the other hand, no such explanation is to hand, then we must look for social or other forms of explanation. To use a concept favoured by the sociologist Robert Merton, there are both "internal" and "external" accounts of science (1968:516) and both appear to be necessary to its practice. On the face of it, Laudan's concept of arationality seems attractive, but it is open to a fairly obvious criticism: that is, what is going to count as the rational thing to do and is this the same for all times and places?

In the foregoing, we have characterized the "social" view of the derivation of scientific knowledge as idealist, but the problem does not end there. Even if we could divide the methodological decisions of science into the rational and the social, we would be left with the problem of how "real" some phenomena actually are. For example, the kinetic theory of gases involves the claim that gases are made up of molecules in random motion colliding with each other. Yet, no one has ever seen a molecule with their own eyes, so in what sense are they real? A version of empiricism, known as instrumentalism, attempts to get around the problem by saying that theories are just useful devices for connecting one set of observables with another. Gases can be "observed"—they can be seen, or smelt, or weighed. These observable properties need to be explained and it is the role of theories to achieve this end. In the instrumentalist view, if the theory adequately explains the behaviour of a particular gas, for example, then it is a useful theory, but does not necessarily describe any "real" world. Molecules are convenient theoretical fictions. Apart from the implicitness of a "pragmatic theory of truth" and its attendant problems, instrumentalism runs into the problem of the "theory laden" nature of observation.

Contrast this with the realist view that our theories actually describe things that exist. The kinetic theory of gases, for instance, is taken to refer to the behaviour of molecules as real things in the world. One principle often evoked in support of this view is that things can be taken to be "real" if they have real effects (Bhaskar 1975,1989). This gets over the problem of the theory laden nature of observations by classing any "imprint" as evidence of reality. Thus, our direct observations of the world are real, as are those that result from the effects of phenomena on complex instruments. This does not necessarily mean the theories are "correct" and that an observation is always "correct", but that our theories and observations are records of real phenomena and not just illusions.

Most forms of realism depend on a correspondence theory of truth. Science, however, as Bhaskar (1975) notes, is often dependent upon experiments for its results. Experiments are themselves closed systems constructed by humans as appropriate to the test of their theories. The "footprints" of phenomena are in this sense brought about by the experimenter. In principle, she could never have known what would have happened had the experiment not taken place. The correspondence between the theory and that which it seeks to explain is the result of the scientist's intervention. A distinction can thus be made between the results of experiments and the way the world is. Although evidence of the latter might have been found and this may come to constitute a "law", this does not exhaust the characteristics of a phenomenon. As Chalmers points out:

> In general, systems in the world will possess other characteristics in addition to those picked out by a particular law…For instance a falling leaf is at once a mechanical, chemical, biological, optical and thermal system (1982:155).

If the experiment intervened in only one of those systems how can we be sure the "truth" it tells us is applicable to all of the other systems? After all, the systems themselves are not recognized by nature as distinct. If things are true, then they are true under all conditions not just those contrived by the experimenter working within one theoretical framework.

Summary

The whole *raison d'être* of the philosophy of science can be said to be the quest for a method of doing science and of defining its nature in the process. We have seen that, in their day, the logical positivists thought they had found this holy grail, as did Popper after them. Recent forays into questions of method have been more circumspect. Though the extreme relativism of some of the sociologists of science may not be any more desirable than the narrow prescriptions of logical positivism, it remains the case that "post-Popperian" philosophy of science has opened up possibilities and we should be wary of prematurely closing these down. New philosophies of science abound. For instance, in Chapter 4 we go on to discuss, in relation to social science, the "network" model of science of Mary Hesse (1974) and the "research programmes" identified by Imre Lakatos (1987). The debate is far from over and continues to produce new ideas and insights, many of which remain controversial (for example, see Brown 1994).

What is more certain is that any question about the scientific nature of social enquiry is parasitic upon what counts as scientific. Yet, if this is a difficult question to answer, it demonstrates that all forms of systematic enquiry are plagued by philosophical problems. Whether we call something "science" or not, it remains that there are questions that will always be present in the systematic pursuit of knowledge. These are philosophical problems concerned with what kinds of things exist and how we can know them. Moreover, many of these are shared by both social research and investigations of the physical world. For this reason, in the next chapter, we will examine the nature of social research and in so doing we will refer back to many of the philosophical issues that we have raised in this chapter.

Questions for discussion

1. What is the role of observation in science? Can there be a neutral observation language?
2. How sustainable is a falsificationist account of science?
3. Can science distance itself from social interests?
4. How well does Kuhn's theory of paradigmatic change account for the history of science?

Suggested reading

Chalmers, A. 1982. *What is this thing called science?* Milton Keynes: Open University Press.

Hospers, J. 1967. *An introduction to philosophical analysis.* London: Routledge.

Lakatos, I. & A. Musgrave (eds) 1970. *Criticism and the growth of knowledge.* Cambridge: Cambridge University Press.

Law, J. & P.Lodge 1984. *Science for social scientists.* London: Macmillan.

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