

## *The Nature of Science*

### **Empiricism**

What is social science? This is a characteristically philosophical question, examining the assumptions and presuppositions of an area of human activity. It seems easy to give a list of would-be social sciences. Sociology and social anthropology would inevitably be on it, as would such subjects as politics and economics. History has a claim to be there too, although it is often not thought of as a social science. It certainly studies the interactions of humans in society. The main difference between it and the others is that it confines itself to the past. Psychology, even social psychology, should probably not be there as it concentrates on the individual rather than on his or her place in the wider group.

It is already obvious that the notion of social science is not as clear-cut as might be first imagined. Most people would accept psychology as a science, but would wonder about its qualifications as a *social* one. History deals with societies, as well as individuals, but perhaps it is often more like literature, and ought not to be classified as a science. Its imaginative reconstruction of individuals' motives may sometimes appear more like writing a novel than like the repeated experiments of a chemist in a laboratory.

All this assumes that 'social' qualifies 'science' in the same way that 'physical' or 'natural' do. A contrast is often drawn between physical and social sciences which takes it for granted that both sets of disciplines are sciences. Physicists study the natural world while sociologists study the social world. It would follow that social sciences are not different kinds of sciences from the physical ones. The same scientific method is being

used in a different area. This could not be the case for, say, theology, if that were classified as a science. It has sometimes been described as one and has on occasions even claimed to be 'the queen of the sciences'. Yet no one has ever suggested that it proceeds in the way that a physicist does. It may attempt to study the nature of God in a systematic way, but it is not an empirical science. Much of its function lies in studying what, if anything, transcends human experience.

Social sciences claim to be empirical. The enormous success of modern physical science in manipulating the world has made it appear that its methods provide the key for the extension of human knowledge in all areas. Any intellectual discipline is thus put under immense pressure to appear 'scientific' in precisely the manner in which physics is; otherwise it may be deemed not to produce any contributions to genuine knowledge.

The question whether the social sciences are 'proper' sciences is not a terminological quibble. Given that modern governments provide money for scientific research, there is a political question whether social scientists should be treated on a par with physicists and engineers. Some social scientists are very eager to show scientific credentials, while governments intent on controlling public expenditure will query how far social science is 'real' science. In 1983 the Social Science Research Council in the United Kingdom decided to change its name to the Economic and Social Research Council. This was apparently after pressure from government ministers, and they were presumably not interested merely in the philosophical issues involved. The public view of the social 'sciences' will determine decisions about public spending.

Nevertheless the pressure to make all disciplines conform to the one model begins to lessen as cynicism increases about the ability of science to further human welfare. The bright hopes for human progress which were once pinned on science already seem childishly optimistic. It is now easier to re-examine the presuppositions of a scientific world-view. All science may not be like physics, and physics itself may not provide a very good example of an empirical science, at least as understood by empiricism. Empiricism in fact has been put on trial and in many philosophers' opinion has to be discarded.

There has already been considerable disagreement over whether the social sciences should follow the methods of the natural sciences and share their assumptions. Are they to uncover the laws governing human behaviour and explain its causes? This is to assume that the social world is

indistinguishable from the natural world in important respects and may even be reducible to it. Many philosophers, particularly if they have been influenced by the European hermeneutical tradition, point out that the social world is constituted by the meanings and purposes of rational agents. The function of a social science is then to interpret and render intelligible rather than to invoke causes. People are different from physical objects and must be understood differently. This approach has been dubbed 'humanist', as opposed to the 'naturalist' approach of those taking natural science as a model. It has been alleged that each side focuses on part of the truth. For instance two writers about the social sciences say:

These sciences are *social*, which is to say that the phenomena they study are intentional phenomena, and so must be identified in terms of their meanings. Secondly, these sciences are *sciences*, in the sense that they try to develop systematic theories to explain the underlying causal interactions among phenomena of a widely divergent sort. Because they each fasten on only one of these features, humanism and naturalism fail to provide an adequate account of social science.<sup>1</sup>

Naturalism, in various guises, has had the most influence on the social sciences. Its assumptions may be increasingly questioned, but any survey of the social sciences must start with them. The scientific character of the social sciences is emphasized, and anything that cannot be subsumed under scientific laws is excluded. This can be part of an empiricist outlook, according to which human experience provides the standard by which anything can be tested. What is beyond experience is discarded. Perhaps the most notable group of philosophers expressing this position in this century was the Vienna Circle. Meeting in Vienna in the 1920s and the 1930s, they championed what they termed the 'scientific world-conception'. They claimed this was empiricist and positivist and explained these terms in a pamphlet by claiming: 'There is knowledge only from experience, which rests on what is immediately given. This sets the limits for the content of legitimate science.' They were empiricists because they rested knowledge on human experience, and positivists because they considered that scientific method was the only path to truth. They continued:

The aim of scientific effort is to reach the goal, unified science, by applying logical analysis to the empirical material. Since the meaning of

every statement of science must be statable by reduction to a statement about the given, likewise the meaning of any concept, whatever branch of science it may belong to, must be statable by step-wise reduction to other concepts, down to the concepts of the lowest level which refer directly to the given.<sup>2</sup>

The Vienna Circle explicitly dealt with the social sciences as branches of science and mentioned history and economics as examples.<sup>3</sup> They had a view of the unity of science, according to which all sciences fitted harmoniously together, and in which the social sciences were included. Science rested on the firm foundation of what was given in experience. 'Sense-data' is a term often used in this connection. Such data were supposed to be intersubjective, in that different people could grasp the same ones. This meant that science could apparently appeal to a fixed standard, and could repeat experiments to obtain the data according to which theories could be assessed. The data were independent of particular theories, since they were 'raw', in the sense that they contained no element of interpretation. Any theory had to fit them to be empirically adequate, and the meaning of observation terms was the same, whatever theory was adopted.

The details of this approach have been modified, but the shadow of the Vienna Circle still falls over the philosophy of science, even when philosophers are reacting against their views. The emphasis is often still on the empirical content of a theory. An empiricist will make no distinction between the empirical information it gives and its ability to explain. The description and prediction of observable facts becomes the aim of science. *Why* observed regularities should be as they are is not thought to be the most fundamental question. The reality of theoretical entities, unobservable but posited by the demands of theory, becomes a problem for the empiricist. A consistent empiricism must maintain that they are not really there but are a convenient fiction which helps in the prediction of what is observable. This problem becomes particularly acute when electrons and other sub-atomic particles are being dealt with. What is quantum mechanics about, if there can in principle be no unobservable entities? An empiricist can only conclude that it is attempting to describe and predict observations and measurements made on sub-atomic systems, rather than refer to any intrinsic properties of the systems. There can be no intrinsic properties beyond the scope of measurement, since what cannot be observed cannot be there. As a result

the emphasis must move from the nature of reality to questions about scientists and their operations.<sup>4</sup> Sub-atomic particles can be talked of only in the terms of classical mechanics, and that means that the entity measured cannot in principle be separated from the act of measurement. The observation made provides the foundation for the theory, even though that involves a necessary reference to a human observer. What started by being a description of the basic constituents of matter apparently ends by being about an episode of human observation.

Its emphasis on experience lays empiricism open to the charge of being anthropocentric. It switches attention from what is or can be experienced to the mere fact of experience itself. By definition, what lies beyond human experience can be conveniently dismissed. The consistent empiricist cannot talk of the other side of the universe, the interior of a black hole, the position *and* the momentum of an electron, or a myriad other possible states of affairs which science may need to envisage. It is hardly surprising that realists who insist that what exists does so independently of human conceptions see idealist echoes in empiricism. Idealists make existence depend logically on its being perceived by a mind, usually a human one. Although empiricists may say that *what* is experienced does not depend on being experienced for its reality, they still limit reality to what can be experienced. Unobservable entities are thus contradictions in terms. This radically changes the character of scientific explanation. Theoretical entities cannot explain what we perceive. Perceptions themselves become the bedrock on which theory rests. Actual and possible observations become ends in themselves. Science may predict them but is left with no resources with which to explain them. Nothing else of a more fundamental nature can be invoked.

Many may object to a picture of the world in which reality is made to depend logically on the possibility of human observation. Another major criticism of empiricism has concentrated on the fact of observation. 'Raw' sense-data have been seen as the brute facts on which our knowledge of the world rested. For this reason empiricist epistemology is often termed a 'foundationalist' one. Such an epistemology is only as reliable as its foundations. Although much effort was expended by empiricists in showing how sense-data were experiences stripped of all interpretation, such as simple colour patches, there has been increasing scepticism about this. The view implied that the mind was a passive receptacle for experience, rather than an active searcher. Yet expectation

can govern what we see. So far from experience governing how we interpret it, our interpretation can affect what we experience. If we are waiting for a red bus, the fluttering of reddish leaves can be mistaken for the approach of the bus. When we recognize the leaves for what they are, we may even find that we see the colour differently. The leaves may look more orange than before.

One of the most influential philosophers of this century, Ludwig Wittgenstein, was much exercised by questions about this kind of change of aspect.<sup>5</sup> One of the most famous examples is the 'duck-rabbit', the drawing that can be seen as a duck or a rabbit. One can see the beak of a duck, but one can also interpret the same lines as the ears of a rabbit. Wittgenstein could not finally decide whether the visual impression altered along with our interpretation.<sup>6</sup> Yet the very fact the question could intelligibly be raised spelt the end of the sense-data theory. Passive experience, according to that, would always be the grounding for the mind's interpretation, so that the latter could not alter the former. Any theory about the empirical foundations of knowledge was in jeopardy once it was accepted that what we thought we were seeing could influence what we saw. 'Sense-data' were supposed to be given to us and not created or altered by us. Our knowledge of them was supposed to be infallible, with any errors coming in our interpretation. Once it was accepted that our interpretation could govern the nature of the data, however, any error in interpretation was seen to be liable to infect the data. The 'rock-solid' foundations of empirical knowledge are revealed as being in as much a quagmire as other parts of human reasoning.

One major tenet of empiricist thought was the total separation of fact from value. Sense-data are what they are, and facts are just what we know is the case as a result of them. Human evaluation is a totally distinct matter. The empiricist philosopher, David Hume, had, in the eighteenth century, sternly forbade sliding from assertions about what *is* the case to some about what *ought* to be so.<sup>7</sup> Descriptions and evaluations are still distinguished, as for instance in the moral philosophy of the Oxford philosopher, R. M. Hare. The result is that a privileged status is accorded to facts, which are thought, in a typically empiricist manner, to be true or false in a way that evaluations cannot be. It has become something of a challenge to empiricists to rescue evaluations from the charge of arbitrariness, and to demonstrate how there can still be rational constraints on what we may value. Having suggested that values cannot be empirically verified in the way that facts can be, they are faced with the

possibility that humans can logically value anything, without the possibility of error.

The importance of the idea of a 'value-free' science for the social sciences is clear. Social scientists want to be seen to establish 'facts' about society in the same way that they think that a physicist or a chemist uncovers 'facts'. They do not want to appear to be in the grip of an ideology or particular view of the world, which would make their results suspect to those who do not share it. The idea, for example, of a 'conservative' or 'liberal' social science seems to undermine the notion of a social science. Yet it is perhaps significant that Marxists find nothing surprising about the notion of Marxist social science. Most Marxists do not share the philosophical presuppositions of empiricists about the distinction between facts and values. This is a problem of major importance to which I shall return. For the moment, it is merely relevant to stress that any distinction between 'facts' and 'values' rests on empiricist assumptions, giving a privileged status to the idea of a scientifically verifiable fact.

### **The Role of Theories**

Empiricism laid great emphasis on the possibility of testing through experience, and this constituted its conception of objectivity. Carl Hempel, the American philosopher of science, says of science that 'it is concerned to develop a conception of the world that has a clear logical bearing on our experience and is thus capable of objective test'.<sup>8</sup> We have already seen that this approach depends on a notion of 'neutral' experience, which does not already presuppose the holding of a particular theory. Hempel developed a notion of scientific explanation which involves fitting an isolated phenomenon into an overall pattern. Scientific laws are then to be understood as mere observable regularities under which we can subsume whatever we wish to explain. This view is derived from Hume's analysis of causation as constant conjunction. Perceived regularities thus form the basis of our idea of cause, and explanation involves fitting our experiences into a pattern of such regularities. Hempel says: 'The explanation fits the phenomenon to be explained into a pattern of uniformities and shows that its occurrence was to be expected, given the specified laws and the pertinent particular circumstances.'<sup>9</sup>

Hempel proposes as a model what he terms 'deductive-nomological' explanation. This takes the form of a deductive argument, whose premisses include general laws as well as statements about particular observations. One example of a deductively valid inference which he gives is the following:

Any sodium salt, when put into the flame of a Bunsen burner, turns the flame yellow.

This piece of rock salt is a sodium salt.

Therefore, this piece of rock salt, when put into the flame of a Bunsen burner, will turn the flame yellow.<sup>10</sup>

One acknowledged difficulty of this form of explanation is that not all generalizations can have the status of general causal laws. It may well be true that the barometer falls when it gets windier, but it would not be right to say that the wind makes the barometer fall. The wind and the fall of the barometer each have the same underlying cause. In other cases there is no genuine connection at all. The sounding of a factory hooter in one city has no effect on workers in a factory a hundred miles away, even if the latter always stop work when the former sounds. The importation of counterfactuals (concerning what would happen if certain facts were different) can help to distinguish between accidental generalizations and causal laws. The issue is then whether the same apparent connection continues if one of the factories changes its hours of work.

Sometimes it is far from clear whether something is a coincidence or whether there is a causal connection. It used to be a joke in one English city afflicted with particularly bad traffic congestion that there was always a policeman on point duty in the middle of the worst traffic jams. It was a joke because everyone assumed that the situation would be worse if the police were not there. Yet, when they were withdrawn as an experiment, the traffic flowed more freely and there was less congestion. There proved to be a causal link in what appeared to be a mere empirical regularity.

Fitting particular instances into a network of regularities which are established empirically conforms to some ideal of explanation. Yet problems remain. The difficulty of sifting out 'real' empirical regularities from the coincidences arises only because empiricists wish to stop at the level of observation. Their ideal of explanation is the association of



different experiences on different occasions. Yet *why* should these be associated? Even the appeal to counterfactuals so as to discover genuine causal connections merely pushes the question back a stage. What makes a connection a genuine causal one? Many philosophers still follow the eighteenth-century philosopher, Hume, in wishing to deny the existence of hidden powers and necessary connections. It is impossible to *experience* the way two events are causally related. One can merely observe that one typically follows the other. Anything more requires a theory which may need to posit unobservable entities as the source of the relevant causal power. That seems irredeemably metaphysical, and therefore bad to genuine empiricists. They would not see the point of postulating mysterious causes behind observable effects but would be content to be successful in predicting observations. Yet this is done at a cost. Explanation becomes merely a summary of actual and possible observations, and the reason why events are linked as they are can never be given.

Another source of worry for empiricism is shown in the example given by Hempel of a deductive inference. How can one observe that 'this piece of rock salt is a sodium salt'? We are already a long way from the level of simple sense-data. If the simplest colour patches cannot be disentangled from interpretation, the making of complicated observations in a scientific laboratory is certainly going to be 'theory-laden'. The recognition of a substance as 'rock salt', let alone its classification as a 'sodium salt', is in no sense a 'raw' experience, uncomplicated by interpretation. A trained chemist may well see at a glance what the substance is, but it is naïve to imagine that a schoolboy could immediately see a substance as 'rock salt' without being told. Reference to a 'sodium salt' goes further and presupposes a scientific theory.

Classification is the outcome of theory, even at fairly simple levels. It is not a question of neutral observations forming the basis for theory. The observations we make, the way we classify them, and even what counts as a relevant observation, may be governed by the theory we hold. Once this was understood, the emphasis inevitably swung away from sense-data to the role of theory or, more generally, of our conceptual scheme in making sense of the world around us. Of course, much depends on what is meant by 'theory', and it may be that, within our overall scheme, different theories will still compete with each other. Nevertheless, a theory seen as a whole claims priority, instead of each level in it being ultimately reducible to statements about what the Vienna Circle saw as

the given. This means that at every level, whether at that of the observation of colour or of the study of complex social phenomena, the 'facts' become secondary. They cannot be read off without a theory to guide classification and selection.

One writer on Marxism says, in a manner reminiscent of those attacking sense-data:

There are no 'brute' facts, descriptions which describe facts but independently of any interest in terms of which they are relevant. . . . The objects of knowledge are objects only in relation to some knowledge-generating inquiry. For 'objects', being 'facts *for*' some inquiry, are classes of events or states of affairs *under some description*, the descriptions in question being determined by the controlling interest of the inquiry.<sup>11</sup>

He mentions for illustration that there are many possible descriptions of the kind of society which Marx classified as capitalist:

Any society which is capitalist is also, necessarily, an 'industrial' society, and any modern industrial society which is capitalist is modelled, at any rate fundamentally, on market relations, and so can be called a 'market' society. Most market societies are politically 'liberal-democratic'.<sup>12</sup>

It appears that, instead of facts being discovered in the world, our descriptions are governed as much by our interests and purposes as by what is there. Human knowledge is then not just the passive reflection of reality but is itself partly constituted by human interests. This move in the social sciences was inevitable once the physical sciences were partially detached from empirical phenomena. Contemporary philosophers of science have themselves emphasized the crucial role of theories more and more, dismissing the notion that experience can be neutral. It seems that everything is 'theory-laden'. Even the contemporary American philosopher, W. V. Quine, who attended meetings of the Vienna Circle, and still stresses the importance of sensory stimuli as a starting-point for theory, shows how our most ordinary experiences already include an element of interpretation. To take one of his most famous examples, a linguist in a foreign country confronted by a native saying 'Gavagai' when a rabbit scurried by would have difficulties translating this unknown word.<sup>13</sup> Quine would assume that the native and the linguist had the same 'ocular irradiations', but these are physical states, far

removed from sense-data, which were supposed to be mental ones. On this sensory base, Quine thinks, we have to build our theories, but there can be no guarantee that the native and the linguist possess exactly the same theory. When we see a rabbit, the native may see 'rabbit stages' or 'rabbit parts'. Quine insists that there will always be a certain indeterminacy of translation, even given the same sensory input. Interpretations of the most basic experience may differ, and indeed the very notion of a basic experience is exposed as an empiricist prejudice. What we say about the world always goes beyond available data, so that all theories are underdetermined. Quine writes:

The truths that can be said even in common-sense terms about ordinary things are themselves, in turn, far in excess of any available data. The incompleteness of determination of molecular behavior of ordinary things is hence only incidental to this more basic indeterminacy: *both* sorts of events are less than determined by our surface irritations.<sup>14</sup>

The ghost of empiricism lives on in Quine, with his insistence on 'surface irritations'. They still provide the starting-point for all theory. Yet the very fact that even our ordinary experience of the world is itself the result of theory places, he says, ordinary physical objects on a par with the most theoretical of entities. Tables and chairs have no privileged status over electrons and protons. They are all posits of a theory. Quine says: 'Everything to which we concede existence is a posit from the standpoint of a description of the theory-building process.'<sup>15</sup> Yet, if theories are underdetermined by data, alternative theories can in their own terms be equally valid. One example he has given is seeing a procession of three lumps moving along at the surface of the water. We may posit a school of dolphins or a single Loch Ness monster. We choose our theory in such cases, he thinks, by preferring the simpler or more familiar explanation. Further observations may sometimes settle the matter, but, even if all possible observations have been made, Quine holds that alternative theories are always possible. Empirically equivalent theories may still be significantly different. In this and in his whole-hearted acceptance of theoretical entities, he shows himself as no empiricist.

Quine's acceptance of theoretical entities as posits does not raise them to the level of familiar objects. Rather, he appears to question the objective reality of the latter. He would insist that 'to call a posit a

posit is not to patronize it'.<sup>16</sup> He says that 'we can never do better than occupy the standpoint of some theory or other, the best we can muster at the time'. It is perhaps fruitless to complain that theoretical entities are 'only' theoretical if all entities are. Insisting, though, on the complete priority of theory like this is bound to raise some misgivings. According to Quine, we can only talk about what our theory counts as real. He would deny that there is any sense in which we can further refer to reality. We can only conceive of what we can conceive.<sup>17</sup> The notion of objective reality thus becomes what our current theory says is real. Quine's view demands that 'world', 'reality' and all such concepts are theoretical, and that theories cannot be simply understood as attempts to describe an independently existing world. The world becomes the reflection of theory, rather than theories mirroring the world, more or less well. This is why translation becomes such a problem for him, since he cannot assume that different theories are *about* the same world. He has to attempt to correlate the words of one theory with those of another without assuming that each refers to the same entities. He cannot assume that the native sees a rabbit and he certainly cannot dogmatically assert that there just are rabbits and the native is wrong if he does not recognize the fact. That is only so from the standpoint of *our* current theory. For Quine the fundamental relationship has to be between word and word, and not between word and object. There are no such things as objects viewed outside the mediation of any particular theory. There is no possibility, therefore of a unique description of the world. Alternatives are always possible, because theories are underdetermined by data.

If theories are not merely means of predicting phenomena, nor are deducible from them, the importance of the empirical content of a theory has somehow been downgraded. It is not just that we are now free to posit unobservable entities to explain experience. The loosening of experience as a constraint on our choice of theory raises the question of what other influences there might be. The foundationalist epistemology of empiricism provided a curb on what could be rationally adopted. If the possibility of such an epistemology is an illusion, the status of rationality itself must be queried. There may seem something highly suspicious about asking for reasons for being rational, but we have to ask where our theories come from and on what they are based. Just how desperate the situation could be is illustrated by the way in which Richard Rorty can talk of 'the cultural space left by the demise of epistemology'.<sup>18</sup>

The same problem arises at a multiplicity of levels. Epistemology as a discipline flounders once the tight constraints of empiricism are loosened. It seems that we do not know where to look for justification, whether it is of our most general theory about the external world, for the practice of science, for the holding of particular theories within science, or for social theory. The same applies to the myriad other forms of belief which we may fondly imagine constitute knowledge.

The practice of physical science has come under particular scrutiny, because empiricism laid such store by observation, and modern science has appeared to be built on the results of observation and experiment. It was part of the received doctrine about science that the history of science was logically distinct from the philosophy of science. The rational justification of a theory may not coincide with the way the theory was actually arrived at. Philosophy laid bare the philosophical foundations of science, while history showed what actually happened. Such an outlook presupposed that science *had* epistemological foundations. Once, however, scientific theory is cut loose from its empirical base, it becomes unclear whether the philosopher can any longer stand aloof from history. Maybe there is no way of distinguishing between what ought to have happened in the development of science from what actually did happen. Insistent questions, however, remain to be answered. What is the purpose of science if it is no longer understood as the search for greater accuracy in empirical observation and prediction? Why should scientists ever change their theories? What, in fact, constitutes scientific progress?

### **The Views of Kuhn and Feyerabend**

One of the major influences in the cataclysmic changes that have affected modern philosophy of science has been the work of T. S. Kuhn. Significantly, he was a historian of science. He introduced the notion of a scientific 'paradigm' which governs a scientist's view of the world. A major change in scientific thinking involves a change in paradigm and the switch from classical to quantum mechanics would provide one example. Kuhn's thesis is that, when scientists work under a paradigm, they take part in what he calls 'normal science', trying to solve the problems thrown up by the theory they hold. Every so often, however, a scientific revolution occurs when the existing paradigm proves unable to cope with what appear to be anomalous situations. Kuhn's views are

particularly interesting as regards what happens when a scientific community shifts from one paradigm to another:

The historian of science may be tempted to exclaim that when paradigms change, the world itself changes with them. Led by a new paradigm, scientists adopt new instruments and look in new places. Even more important, during revolutions scientists see new and different things when looking with familiar instruments in places they have looked before.<sup>19</sup>

The inversion of the empiricist scheme of things is clear. The theory determines the observations, according to Kuhn, instead of observations determining the theory. Indeed, he seems to be going further than saying that a new theory shows us how to notice what we did not notice before. He appears to be asserting that the world is actually different for different theories. What counts as real under one paradigm does not under another. Scientists once believed there was a substance called phlogiston and now do not. They once believed the atom could not be split, whereas now they continue to search for new sub-atomic particles. There is a continuing ambiguity, though, in illustrations like this. Everyone accepts that scientific theories change our conceptions of reality, but the question is whether there is a sense in which they actually change reality. Perhaps the way the issue is posed begs the question, since it presupposes that we can sensibly talk of a reality which is independent of our conceptions of it. Yet this is what Quine denied, and Kuhn seems merely to be taking this a step further by saying that the world might itself change. The world as conceived by scientists does change, but does that mean the world itself changes? The distinction between these two positions is of major philosophical importance. Arguments rage over whether such a distinction can be made. Kuhn poses the question of what scientific theories are about. Can they be measured against anything external to themselves, and therefore at least in principle be judged correct or mistaken, true or false? The alternative is that we are left with a succession of different theories, or conceptions of the world, with no means of determining which is better than the others.

Kuhn uses the 'duck-rabbit' drawing as an analogy to illustrate the way in which a scientist's vision can change after a switch of paradigms. He draws attention to the way in which theories can govern what we see. A student, he says, may only see confused and broken lines when

looking at a bubble-chamber photograph, whereas a trained physicist will actually *see* a record of sub-nuclear events. He says:

It is as elementary prototypes for these transformations of the scientist's world that the familiar demonstrations of a switch in visual gestalt prove so suggestive. What were ducks in the scientific world before the revolution are rabbits afterwards.<sup>20</sup>

One consequence of these radical shifts in a scientist's vision is that scientists working under different paradigms possess different concepts and make different observations. They cannot appeal to any theory-neutral observation, and Kuhn removes the possibility of referring to the same objective world underpinning all paradigms. Since 'the world' is seen only through a paradigm, there is a problem as to how those with allegiance to different paradigms can discuss their theories with each other. Empiricists could argue with those they disagreed with, by appealing to the experience they all shared. Kuhn cannot do that and considers the introduction of a neutral language of observation 'hopeless'.<sup>21</sup> He has to talk instead of the 'incommensurability' of different scientific theories. At a time of revolution a scientist will find the world of his research here and there 'incommensurable with the one he had inhabited before'.<sup>22</sup> This has an important and worrying consequence. Since scientists will each see a world constituted by their own paradigm, those with allegiance to different paradigms will be talking at cross-purposes to each other. They will actually be referring to different entities and their theories will not rest on a common base, nor apparently even be about the same world. Kuhn does not even use the physical impact of the world on our senses as a starting-point, as Quine does. There is instead a competition between different visions of the world, some of which may overlap, but which cannot formulate their disagreements in any common language.

Why, then, should scientists change their paradigms? The history of science shows that this has often happened and Kuhn attempts to describe that process. At what point do the strains of an existing theory become unbearable? Kuhn has left himself with few resources to answer that. It cannot be because of an appeal to experience, since that is governed by a paradigm. It cannot be because we discover basic errors, since the paradigm controls what is to count as correct. Discovering basic 'errors' is, in fact, tantamount to changing the way we choose to see the world. Kuhn

points out that individual scientists embrace a new paradigm ‘for all sorts of reasons and usually for several at once’. He continues:

Some of these reasons – for example the sun worship that helped make Kepler a Copernican – lie outside the apparent sphere of science entirely. Others must depend upon idiosyncrasies of autobiography and personality. Even the nationality or the prior reputation of the innovator and his teachers can sometimes play a significant role.<sup>23</sup>

This is not a very encouraging summary of the rational processes of scientists, and Kuhn goes on to stress that his interest is mainly in the sort of community that re-emerges after a time of scientific crisis. He mentions that those who resist the new paradigm cannot be said to be wrong, and indeed he could have no criterion by which they could be judged wrong. He says of the historian: ‘At most he may wish to say that the man who continues to resist after his whole profession has been converted has *ipso facto* ceased to be a scientist.’<sup>24</sup>

The philosophy of science can thus no longer provide a rational reconstruction of the way theories logically depend on each other and can justify each other. There is nothing left but the question how science has actually developed. The philosophy of science has to become the history of science, since there is no way left in which what has happened can be rationally criticized. Science is what particular communities happen to do, rather than being an impressive rational creation of the human mind. It is hardly surprising that Kuhn’s views have paved the way to a growing interest in the sociology of science. The account he gives of paradigm shifts cries out for a sociological explanation for them. Sociologists must feel confident that they can contribute to our understanding of a sudden change in the behaviour of a community, particularly if there is no possibility of its having occurred on rational grounds. By talking of the interaction of scientists with each other, Kuhn focuses on the idea of a community, and he explores the idea of standards enforced by the community. The very fact that a particular social group can accept one solution to a problem but not another, or can see one argument as a justification but not another, cries out for sociological analysis. So at least a sociologist of science would argue. The conventional character of scientific judgement has seemingly been exposed, and it is the sociologist’s concern to show us how the conventions operate and change. One such sociologist writes:



---

Scientific standards themselves are a part of a specific form of culture; authority and control are essential to maintain a sense of the reasonableness of the specific form. Thus, if Kuhn is correct, science should be amenable to sociological study in fundamentally the same way as any other form of knowledge or culture.<sup>25</sup>

Kuhn's attack on the rational foundations of science (for that is in effect what it is) has not been a solitary one. Its anti-empiricism has been in tune with the mood of the times and has found a ready following. There is no doubt, though, that at times the reaction from the idea that knowledge has firm foundations in experience has been very extreme. It is one thing to say that theories can govern how we see and experience the world, but quite another to make it impossible any longer to refer to the world. It is one thing to question whether experience is the only source of human knowledge, but quite another to say that knowledge is conventional. The latter is to say that societies or traditions determine what we count as knowledge. Our grasp on reality becomes exceedingly tenuous, if that is so. What we believe or claim to know is merely the product of social forces of which we may be utterly ignorant.

The philosopher of science, P. Feyerabend makes it explicit that reality in some sense actually depends on our choice. He says flatly: 'We decide to regard those things as real which play an important role in the kind of life we prefer.'<sup>26</sup> He considers that there is no more ultimate way of referring to reality than through the particular tradition we belong to. Instead of reality controlling our beliefs, at least to some extent, it seems as if the beliefs of a tradition determine what is to count as real. There are countless traditions, each of which has firm teaching on what is real. Within any one of them, we can gain the illusion that knowledge can be and has been attained. Yet, once we see that many conflicting traditions have the same conceit, we realize, it is alleged, that judgements of truth only have relative validity. They hold for our colleagues in the tradition to which we are attached, but not for those outside.

Feyerabend's unsettling view is that this kind of argument can be applied in any sphere. The practice of science, and even the exercise of reason, can be revealed as themselves historical traditions. He says:

Scientific practice, even the practice of the natural sciences, is a tightly woven net of historical traditions. . . . This means that general statements

*about science, statements of logic included, cannot without further ado be taken to agree with scientific practice.*<sup>27</sup>

The notion of a philosophy of science with an ideal of rational justification is again under attack. What is left is merely what scientists happened to have done, or are doing, and that forms suitable material for sociological study. Scientific standards are 'not imposed upon science from the outside'.<sup>28</sup> Instead Feyerabend insists that they are produced by scientists in the course of their research. He particularly stresses the importance of being able to choose between competing alternatives, and advocates the principle (or anti-principle) of 'anything goes' as a basis for scientific method. Because he sees Western science as one tradition among many, he wants freedom of choice between that and its alternatives. He also talks of the exercise of reason itself (or 'rationalism', as he puts it) as a tradition:

Each tradition, each form of life, has its own standards of judging human behaviour and these standards change in accordance with the problems that the tradition is constrained to solve. Rationalism is not a boundary condition for traditions: it is itself a tradition, and not always a successful one. There exists, therefore, a plurality of standards just as there is a plurality of individuals. In a free society, however, a citizen will use the standards to which (s)he belongs: Hopi standards, if he is a Hopi: fundamentalist Protestant standards, if he is a fundamentalist.<sup>29</sup>

This is a classic statement of relativism. There are different, self-contained traditions and ways of life. Each generates its own standards. None can be understood, let alone judged, by means of criteria rooted in a different tradition. Many have found this an attractive doctrine when applied to human customs or even morality. It is perhaps a philosophical version of the saying, 'When in Rome, do as the Romans do'. Different societies have different ways of doing things, and sometimes at least there is mere difference, without one set of practices being better or worse than another. An obvious example is which side of the road we drive on. No one can say that driving on the left is better than driving on the right, or vice versa. We naturally tend to prefer what we are used to, but as long as everyone in one country conforms to the same convention that is enough. There is a story of a newly independent nation which decided to change from driving on the left to driving on the right, but to do so

gradually. That is clearly the speedy route to chaos. So it is with many social conventions. Conformity to particular ones in a given society is necessary for the functioning of that society, but a different society can conform to totally different ones.

Questions about this kind of relativity in the case of moral standards become much more controversial. Feyerabend, however, goes even further. So far from accepting scientific method as the very model of rationality, or indeed considering rationality itself as a universal ideal, he belittles each as being one tradition among many. He says of scientists that they are 'salesmen of ideas and gadgets, they are not judges of truth and falsehood'.<sup>30</sup> This is not the raving of a crank but is the inevitable outcome of the attacks on empiricism which removed the foundations of our empirical knowledge without putting anything in its place. As a result, questions of knowledge, truth, reality and reason are all discarded. We step back from knowledge to belief, from what is true to what is held true, from reality to people's beliefs about it, and from questions of rational standards to all the peculiar ways in which humans do actually purport to reason. We step from justification to description, and from philosophy, or, at least, epistemology, to sociology. The emphasis laid on the incommensurability of theories by Feyerabend, as well as Kuhn, develops into a position where theories or traditions set their own standards of rationality and are immune to criticism from outside.

Feyerabend eagerly seizes on Wittgenstein's view that the meaning of concepts is given by their use in a practice.<sup>31</sup> Concepts are not labels for things but are grounded in ways of acting. I shall return to this later, but Feyerabend draws out one very important consequence. If we understand what a word means by using it in connection with a particular activity, the only way to learn it is by participating in that activity. Instead of words referring to an objective world or an experience which can be shared intersubjectively, they are rooted in what Wittgenstein refers to as a 'language-game' or a 'form of life'. The purpose of both expressions is to draw attention to the fact that language is intimately connected with activities. It cannot, it is suggested, be peeled off and understood apart from the life of which it is a part. Yet this means that non-participants cannot understand what is being said in any practice. This doctrine, if true, has tremendous consequences for the social sciences, and the problem of *Verstehen* (understanding) is indeed one of the most venerable topics for the philosophy of social science. Clearly, though, on Feyerabend's view, all the sciences face the same difficulty.

So far from the social sciences finding it difficult to live up to the exacting standards of the natural sciences, the latter seem to be dissolving into social groupings posing problems of understanding and interpretation for the social scientist.

This might seem an argument against Feyerabend's position, but unfortunately it is a consequence that he embraces. He has rejected the idea that philosophy can uncover basic rational principles according to which science proceeds. He rejects the idea of an objective, extra-theoretical reality, so that as a result everything is valid only within the confines of a particular theory. The practices of a science are valid only for the practitioners of the science. Feyerabend explicitly mentions the position of traditional methodologists of science who have held 'that a historian studies a distant culture by trying to "understand" it while a physicist who deals with explicit abstract notions "explains"'.<sup>32</sup>

He will have none of this dichotomy between the interpretation of a social scientist and the explanation of a natural scientist. In fact, he has left the latter without any materials with which to explain in a manner that can be either intelligible or relevant to a non-physicist. Explanation can only satisfy those who are physicists. By insisting that physics is itself a social practice or tradition, Feyerabend has removed its claims to have any ultimate answers, or to be in a privileged position compared with other disciplines. He explicitly follows in Kuhn's footsteps and says with approval: 'Kuhn makes the highly interesting and revolutionary suggestion that *physics is a historical tradition and therefore as much in need of Verstehen as history proper*'.<sup>33</sup> The theories of a contemporary physicist, therefore, may stand in as much need of interpretation and understanding as those of an ancient Greek. Physics is no longer seen as the base to which other sciences can be reduced, but as one tradition out of many; it can be investigated only by means of the methods of the social sciences.

We have arrived at a bewildering conclusion. According to a dominant trend in contemporary philosophy of science, science is not a path to truth at all, let alone the only one. It sometimes looks as if some philosophers are saying that there is no such thing as truth. The incoherence of that is revealed if we realize that they are asserting *as true* the proposition that there is no such thing as truth. Feyerabend himself realizes that he may appear to be giving reasons for his view that reason 'is a tradition in its own right with as much (or as little) claim to the centre of the stage as any other tradition'.<sup>34</sup> There are times when it looks as if he views the opportunity of choice between different tradi-

tions and theories as a good stratagem in the pursuit of truth. Refusal to be bound by a rigid methodology may itself be the last of all methodologies. At other times, it seems as if his advocacy of relativism is leading us perilously close to denying that any view is better than any other, or any reason more valid than any other. The problem that remains is whether it is possible to dethrone the natural sciences without undermining the possibility of human reason. Can the social sciences step in with new modes of explanation?

### Notes

- 1 B. Fay and J. D. Moon, 'What Would an Adequate Philosophy of Social Science Look Like?', *Philosophy of the Social Sciences*, 7, 1977, p. 227.
- 2 'The Scientific Conception of the World: The Vienna Circle', in M. Neurath and R. S. Cohen (eds), *Otto Neurath: Empiricism and Sociology*, 1973, p. 309.
- 3 *Ibid.*, p. 315.
- 4 For a further discussion of quantum mechanics and reality, see my *Reality at Risk*, 1980, ch. 6.
- 5 Ludwig Wittgenstein, *Philosophical Investigations*, 1958, II, xi.
- 6 See my *Pain and Emotion*, 1970, pp. 81ff.
- 7 David Hume, *A Treatise of Human Nature*, Book III, reprinted in *Hume's Ethical Writings*, ed. A. MacIntyre, 1965, p. 196.
- 8 Carl Hempel, *Philosophy of Natural Science*, 1968, p. 47.
- 9 *Ibid.*, p. 50.
- 10 *Ibid.*, p. 10.
- 11 D. Turner, *Marxism and Christianity*, 1983, p. 104.
- 12 *Ibid.*, p. 104.
- 13 W. V. Quine, *Word and Object*, 1964, p. 29.
- 14 *Ibid.*, p. 22.
- 15 *Ibid.*
- 16 *Ibid.*
- 17 For a longer discussion of Quine, see my *Reality at Risk*.
- 18 Richard Rorty, *Philosophy and the Mirror of Nature*, 1980, p. 315.
- 19 T. S. Kuhn, *The Structure of Scientific Revolutions*, 1962, p. 110.
- 20 *Ibid.*, p. 110.
- 21 *Ibid.*, p. 125.
- 22 *Ibid.*, p. 111.
- 23 *Ibid.*, p. 151.
- 24 *Ibid.*, p. 150.

- 25 Barry Barnes, *T. S. Kuhn and Social Science*, 1982, p. 10.
- 26 P. Feyerabend, *Philosophical Papers*, vol. I, *Realism, Rationalism and Scientific Method*, 1981, p. xiii.
- 27 *Ibid.*, p. 4.
- 28 Feyerabend, *Philosophical Papers*, vol. II, *Problems of Empiricism*, 1981, p. 27.
- 29 *Ibid.*
- 30 Feyerabend, *Philosophical Papers*, vol. II, p. 31.
- 31 *Ibid.*, p. 129.
- 32 *Ibid.*, p. 237.
- 33 *Ibid.*
- 34 Feyerabend, *Science in a Free Society*, 1978, p. 8.