

From David Bloor, *The Strong Programme in the Sociology of Knowledge*

Chapter One

Can the sociology of knowledge investigate and explain the very content and nature of scientific knowledge? Many sociologists believe that it cannot. They say that knowledge as such, as distinct from the circumstances surrounding its production, is beyond their grasp. They voluntarily limit the scope of their own enquiries. I shall argue that this is a betrayal of their disciplinary standpoint. All knowledge, whether it be in the empirical sciences or even in mathematics, should be treated, through and through, as material for investigation. Such limitations as do exist for the sociologist consist in handing over material to allied sciences like psychology or in depending on the researches of specialists in other disciplines. There are no limitations which lie in the absolute or transcendent character of scientific knowledge itself, or in the special nature of rationality, validity, truth or objectivity.

It might be expected that the natural tendency of a discipline such as the sociology of knowledge would be to expand and generalise itself moving from studies of primitive cosmologies to that of our own culture. This is precisely the step that sociologists have been reluctant to take. Again, the sociology of knowledge might well have pressed more strongly into the area currently occupied by philosophers, who have been allowed to take upon themselves the task of defining the nature of knowledge. In fact sociologists have been only too eager to limit their concern with science to its institutional framework and external factors relating to its rate of growth or direction. This leaves untouched the nature of the knowledge thus created (cf. Ben-David (1971), DeGre (1967), Merton (1964) and Stark (1958)).

What is the cause for this hesitation and pessimism? Is it the enormous intellectual and practical difficulties which would attend such a programme? Certainly these must not be underestimated. A measure of their extent can be gained from the effort that has been expended on the more limited aims. But these are not the reasons that are in fact advanced. Is the sociologist at a loss for theories and methods with which to handle scientific knowledge? Surely not. His own discipline provides him with exemplary studies of the knowledge of other cultures which could be used as models and sources of inspiration. Durkheim's classic study 'The Elementary Forms of the Religious Life' shows how a sociologist can penetrate to the very depths of a form of knowledge. What is more Durkheim dropped a number of hints as to how his findings might relate to the study of scientific knowledge. The hints have fallen on deaf ears.

The cause of the hesitation to bring science within the scope of a thorough-going sociological scrutiny is lack of nerve and will. It is believed to be a foredoomed enterprise. Of course, the failure of nerve has deeper roots than this purely psychological characterisation suggests, and these will be investigated later. Whatever the cause of the malady, its symptoms take the form of

a priori and philosophical argumentation. By these means sociologists express their conviction that science is a special case, and that contradictions and absurdities would befall them if they ignored this fact. Naturally philosophers are only too eager to encourage this act of self-abnegation (e.g. Lakatos (1971), Popper (1966)).

It will be the purpose of this book to combat these arguments and inhibitions. For this reason the discussions which follow will sometimes though not always, have to be methodological rather than substantive. But I hope they will be positive in their effect. Their aim is to put weapons in the hands of those engaged in constructive work to help them attack critics, doubters and sceptics.

I shall first spell out what I call the strong programme in the sociology of knowledge. This will provide the framework within which detailed objections will then be considered. Since a priori arguments are always embedded in background assumptions and attitudes it will be necessary to bring these to the surface for examination as well. This will be the second major topic and it is here that substantial sociological hypotheses about our conception of science will begin to emerge. The third major topic will concern what is perhaps the most difficult of all the obstacles to the sociology of knowledge, namely mathematics and logic. It will transpire that the problems of principle involved are not, in fact, unduly technical. I shall indicate how these subjects can be studied sociologically.

The Strong Programme

The sociologist is concerned with knowledge, including scientific knowledge, purely as a natural phenomenon. The appropriate definition of knowledge will therefore be rather different from that of either the layman or the philosopher. Instead of defining it as true belief--or perhaps, justified true belief--knowledge for the sociologist is whatever people take to be knowledge. It consists of those beliefs which people confidently hold to and live by. In particular the sociologist will be concerned with beliefs which are taken for granted or institutionalised, or invested with authority by groups of people. Of course knowledge must be distinguished from mere belief. This can be done by reserving the word 'knowledge' for what is collectively endorsed, leaving the individual and idiosyncratic to count as mere belief.

Our ideas about the workings of the world have varied greatly. This has been true within science just as much as in other areas of culture. Such variation forms the starting point for the sociology of knowledge and constitutes its main problem. What are the causes of this variation, and how and why does it change? The sociology of knowledge focuses on the distribution of belief and the various factors which influence it. For example: how is knowledge transmitted; how stable is it; what processes go into its creation and maintenance; how is it organised and categorised into different disciplines or spheres?

For sociologists these topics call for investigation and explanation and they will try to characterise knowledge in a way which accords with this perspective. Their ideas therefore will be in the same causal idiom as those of any other scientist. Their concern will be to locate the regularities and general principles or processes which appear to be at work within the field of their data. The aim will be to build theories to explain these regularities. If these theories are to satisfy the requirement of maximum generality they will have to apply to both true and false beliefs, and as far as possible the same type of explanation will have to apply in both cases. The aim of physiology is to explain the organism in health and disease; the aim of mechanics is to understand machines which work and machines which fail; bridges which stand as well as those which fall. Similarly the sociologist seeks theories which explain the beliefs which are in fact found, regardless of how the investigator evaluates them.

Some typical problems in this area which have already yielded interesting findings may serve to illustrate this approach. First, there have been studies of the connections between the gross social structure of groups and the general form of the cosmologies to which they have subscribed. Anthropologists have found the social correlates, and the possible causes of our having anthropomorphic and magical world-views as distinct from impersonal and naturalistic one (Douglas (1966 and 1970)). Second, there have been studies which have traced the connections between economic, technical and industrial developments and the content of scientific theories. For example, the impact of practical developments in water and steam technology on the content of theories in thermodynamics has been studied in great detail. The causal link is beyond dispute (Kuhn (1959), Cardwell (1971)). Third, there is much evidence that features of culture which usually count as non-scientific greatly influence both the creation and the evaluation of scientific theories and findings. Thus Eugenic concerns have been shown to underlie and explain Francis Galton's creation of the concept of the coefficient of correlation in statistics. Again the general political, social and ideological standpoint of the geneticist Bateson has been used to explain his role of sceptic in the controversy over the gene theory of inheritance (Coleman (1970), Cowan (1972), Mackenzie (1981)). Fourth, the importance that processes of training and socialisation have in the conduct of science is becoming increasingly documented. Patterns of continuity and discontinuity, of reception and rejection, appear to be explicable by appeal to these processes. An interesting example of the way in which a background in the requirements of a scientific discipline influences the assessment of a piece of work is afforded by Lord Kelvin's criticisms of the theory of evolution. Kelvin calculated the age of the sun by treating it as an incandescent body cooling down. He found that it would have burnt itself out before evolution could have reached its currently observable state. The world is not old enough to have allowed evolution to have run its course, so the theory of evolution must be wrong. The assumption of geological uniformity, with its promise of vast stretches of time, had been rudely pulled from beneath the biologist's feet. Kelvin's arguments caused dismay. Their authority was immense and in the 1860's they were unanswerable; they followed with convincing rigour from convincing physical premises. By the last decade of the century the geologists had plucked up courage to tell Kelvin

that he must have made a mistake. This newfound courage was not because of any dramatic new discoveries, indeed, there had been no real change in the evidence available. What had happened in the interim was a general consolidation in geology as a discipline with a mounting quantity of detailed observation of the fossil record. It was this growth which caused a variation in the assessments of probability and plausibility: Kelvin simply must have left some vital but unknown factor out of consideration. It was only with the understanding of the sun's nuclear sources of energy that his physical argument could be faulted. Geologists and biologists had no foreknowledge of this, they simply had not waited for an answer (Rudwick (1972), Burchfield (1975)). This example also serves to make another point. It deals with social processes internal to science, so there is no question of sociological considerations being confined to the operation of external influences.

Finally, mention must be made of a fascinating and controversial study of the physicists of Weimar Germany. Forman (1971) uses their academic addresses to show them taking up the dominant antiscientific 'Lebensphilosophie' surrounding them. He argues 'that the movement to dispense with causality in physics which sprang up so suddenly and blossomed so luxuriantly in Germany after 1918, was primarily an effort by German physicists to adapt the content of their science to the values of their intellectual environment' (p. 7). The boldness and interest of this claim derives from the central place of acausality in modern quantum theory.

The approaches that have just been sketched suggest that the sociology of scientific knowledge should adhere to the following four tenets. In this way it will embody the same values which are taken for granted in other scientific disciplines. These are:

1 It would be causal: that is, concerned with the conditions which bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief.

2 It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation.

3 It would be symmetrical in its style of explanation. The same types of cause would explain say, true and false beliefs.

4 It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. Like the requirement of symmetry this is a response to the need to seek for general explanations. It is an obvious requirement of principle because otherwise sociology would be a standing refutation of its own theories.

These four tenets, of causality, impartiality, symmetry and reflexivity, define what will be called the strong programme in the sociology of knowledge. They are by no means new, but represent

an amalgam of the more optimistic and scientific strains to be found in Durkheim (1938), Mannheim (1936) and Znaniecki (1965).

In what follows I shall try to maintain the viability of these tenets against criticism and misunderstanding. What is at stake is whether the strong programme can be pursued in a consistent and plausible way. Let us therefore turn to the main objections to the sociology of knowledge to draw out the full significance of the tenets and to see how the strong programme stands up to criticism.

The Autonomy of Knowledge

One important set of objections to the sociology of knowledge derives from the conviction that some beliefs do not stand in need of any explanation, or do not stand in need of a causal explanation. This feeling is particularly strong when the beliefs in question are taken to be true, rational, scientific or objective.

When we behave rationally or logically it is tempting to say that our actions are governed by the requirements of reasonableness or logic. The explanation of why we draw the conclusion we do from a set of premises may appear to reside in the principles of logical inference themselves. Logic, it may seem, constitutes a set of connections between premises and conclusions and our minds can trace out these connections. As long as someone is being reasonable then the connections themselves would seem to provide the best explanation for the beliefs of the reasoner. Like an engine on rails, the rails themselves dictate where it will go. It is as if we can transcend the directionless push and pull of physical causality and harness it, or subordinate it, to quite other principles and let these determine our thoughts. If this is so then it is not the sociologist or the psychologist but the logician who will provide the most important part of the explanation of belief.

Of course, when someone makes mistakes in their reasoning then logic itself is no explanation. A lapse or deviation may be due to the interference of a whole variety of factors. Perhaps the reasoning is too difficult for the limited intelligence of the reasoner, perhaps he or she is inattentive, or too emotionally involved in the subject under discussion. As when a train goes off the rails, a cause for the accident can surely be found. But we neither have, nor need, commissions of enquiry into why accidents do not happen.

Arguments such as these have become a commonplace in contemporary analytical philosophy. Thus in 'The Concept of Mind' (1949) Ryle says: 'Let the psychologist tell us why we are deceived; but we can tell ourselves and him why we are not deceived' (p 308). This approach may be summed up by the claim that nothing makes people do things that are correct but something does make, or cause, them to go wrong (cf. Hamlyn (1969), Peters (1958)).

The general structure of these explanations stands out clearly. They all divide behaviour or belief into two types: right and wrong, true or false, rational or irrational. They then invoke sociological or psychological causes to explain the negative side of the division. Such causes explain error, limitation and deviation. The positive side of the evaluative divide is quite different. Here logic, rationality and truth appear to be their own explanation. Here psycho-social causes do not need to be invoked.

Applied to the field of intellectual activity these views have the effect of making a body of knowledge an autonomous realm. Behaviour is to be explained by appeal to the procedures, results, methods and maxims of the activity itself. It makes successful and conventional intellectual activity appear self-explanatory and self-propelling. It becomes its own explanation. No expertise in sociology or psychology is required only expertise in the intellectual activity itself.

A currently fashionable version of this position is to be found in Lakatos's (1971) theory about how the history of science ought to be written. This theory was explicitly meant to have implications for the sociology of science as well. The first prerequisite, says Lakatos, is that a philosophy or methodology of science be chosen. These are accounts of what science ought to be, and of what steps in it are rational. The chosen philosophy of science becomes the framework on which hangs all the subsequent work of explanation. Guided by this philosophy it ought to be possible to display science as a process which exemplifies its principles and develops in accord with its teachings. In as far as this can be done then science has been shown to be rational in the light of that philosophy. This task, of showing that science embodies certain methodological principles, Lakatos calls either 'rational reconstruction' or 'internal history'. For example an inductivist methodology would perhaps stress the emergence of theories out of an accumulation of observations. It would therefore focus on events like Kepler's use of Tycho Brahes observations when formulating the laws of planetary motion.

It will never be possible, however, to capture all of the diversity of actual scientific practice by this means. Lakatos therefore insists that internal history will always need to be supplemented by an 'external history'. This looks after the irrational residue. It is a matter which the philosophical historian will hand over to the 'external historian' or the sociologist. Thus, from an inductivist standpoint the role of Kepler's mystical beliefs about the majesty of the sun would require a nonrational or external explanation.

The points to notice about this approach are first that internal history is self-sufficient and autonomous. To exhibit the rational character of a scientific development is sufficient explanation in itself of why the events took place. Second, not only are rational reconstructions autonomous; they also have an important priority over external history or sociology. The latter merely close the gap between rationality and actuality. This task is not even defined until internal

history has had its say. Thus: internal history is primary, external history only secondary, since the most important problems of external history are defined by internal history. External history either provides nonrational explanation of the speed, locality, selectiveness etc. of historical events as interpreted in terms of internal history; or when history differs from its rational reconstruction, it provides an empirical explanation of why it differs. But the rational aspect of scientific growth is fully accounted for by one's logic of scientific discovery (1971, p. 9).

Lakatos then answers the question of how to decide which philosophy should dictate the problems of external history or sociology. Alas for externalists the answer represents yet a further humiliation. Not only is their function derivative; it now transpires that the best philosophy of science, according to Lakatos, is one which minimises this role. Progress in philosophy of science is to be measured by the amount of actual history which can be exhibited as rational. The better the guiding methodology the more of actual science is rendered safe from the indignity of empirical explanation. The sociologist is allowed a crumb of comfort from the fact that Lakatos is only too pleased to grant that there will always be some irrational events in science that no philosophy will ever be able or willing to rescue. He instances here unsavoury episodes of Stalinist intervention in science like the Lysenko affair in biology.

These refinements however are less important than the general structure of the position. It does not matter how the central principles of rationality are chosen, or how they might change. The central point is that, once chosen, the rational aspects of science are held to be self-moving and self-explanatory. Empirical or sociological explanations are confined to the irrational.

What can it mean to say that nothing makes people do or believe things which are rational or correct? Why in that case does the behaviour take place at all? What prompts the internal and correct functioning of an intellectual activity if the search for psychological and sociological causes is only deemed appropriate in the case of irrationality or error? The theory that must tacitly underlie these ideas is a goal-directed or teleological vision of knowledge and rationality.

Suppose that it is assumed that truth, rationality and validity are our natural goals and the direction of certain natural tendencies with which we are endowed. We are rational animals and we naturally reason justly and cleave to the truth when it comes within our view. Beliefs that are true then clearly require no special comment. For them, their truth is all the explanation that is needed of why they are believed. On the other hand this self-propelling progress towards truth may be impeded or deflected and here natural causes must be located. These will account for ignorance, error, confused reasoning and any impediment to scientific progress.

Such a theory makes a great deal of sense of what is written in this area even if it seems implausible at first sight to impute it to contemporary thinkers. It even appears to have intruded itself into the thinking of Karl Mannheim. Despite his determination to set up causal and

symmetrical canons of explanation, his nerve failed him when it came to such apparently autonomous subjects as mathematics and natural science. This failure expressed itself in passages such as the following, from 'Ideology and Utopia':

The existential determination of thought may be regarded as a demonstrated fact in those realms of thought in which we can show . . . that the process of knowing does not actually develop historically in accordance with immanent laws, that it does not follow only for the 'nature of things' or from 'pure logical possibilities', and that it is not driven by an 'inner dialectic'. On the contrary, the emergence and the crystallization of actual thought is influenced in many decisive points by extra-theoretical factors of the most diverse sort (1936, p. 239).

Here social causes are being equated with 'extra-theoretical' factors. But where does this leave behaviour conducted in accord with the inner logic of a theory or governed by theoretical factors? Clearly it is in danger of being excluded from sociological explanation because it functions as the base-line for locating those things which do require explanation. It is as if Mannheim slipped into sharing the sentiments expressed in the quotations from Ryle and Lakatos and said to himself, 'When we do what is logical and proceed correctly, nothing more needs to be said.' But to see certain sorts of behaviour as unproblematic is to see them as natural. In this case what is natural is proceeding correctly, that is via or towards the truth. So here too the teleological model is probably at work.

How does this model of knowledge relate to the tenets of the strong programme? Clearly it violates them in a number of serious ways. It relinquishes a thorough-going causal orientation. Causes can only be located for error. Thus the sociology of knowledge is confined to the sociology of error. In addition it violates the requirements of symmetry and impartiality. A prior evaluation of the truth or rationality of a belief is called for before it can be decided whether it is to be counted as self-explanatory or whether a causal theory is needed. There is no doubt that if the teleological model is true then the strong programme is false.

The teleological and causal models, then, represent programmatic alternatives which quite exclude one another. Indeed, they are two opposed metaphysical standpoints. This may make it appear that it is necessary to decide at the outset which is true. Doesn't the sociology of knowledge depend on the teleological view being false? So doesn't this have to be established before the strong programme dare proceed? The answer is 'no'. It is more sensible to look at matters the other way round. It is unlikely that any decisive, independent grounds could be adduced 'a priori' to prove the truth or falsity of such major metaphysical alternatives. Where objections and arguments are proposed against one of the two theories it will be found that they depend on and presuppose the other, and so beg the question at issue. All that can be done is to check the internal consistency of the different theories and then see what happens when practical

research and theorising is based upon them. If their truth can be decided at all it will only be after they have been adopted and used, not before. So the sociology of knowledge is not bound to eliminate the rival standpoint. It only has to separate itself from it, reject it, and make sure that its own house is in logical order.

These objections to the strong programme are thus not based on the intrinsic nature of knowledge but only on knowledge viewed from the standpoint of the teleological model. Reject that model and all its associated distinctions, evaluations and asymmetries go with it. It is only if that model has a unique claim to attention that its corresponding patterns of explanation are binding upon us. Its mere existence, and the fact that some thinkers find it natural to use it, do not endow it with probative force.

In its own terms the teleological model is no doubt perfectly consistent and there are perhaps no logical reasons why anyone should prefer the causal approach to the goal-directed view. There are, however, methodological considerations which may influence the choice in favour of the strong programme.

If explanation is allowed to hinge on prior evaluations, then the causal processes that are thought to operate in the world will come to reflect the pattern of these evaluations. Causal processes will be made to etch out the pattern of perceived error, throwing into relief the shape of truth and rationality. Nature will take on a moral significance, endorsing and embodying truth and right. Those who indulge their tendencies to offer asymmetrical explanations will thus have every opportunity to represent as natural what they take for granted. It is an ideal recipe for turning one's gaze away from one's own society values and beliefs and attending only to deviations from them.

Care is needed not to overstate this point, for the strong programme does exactly the same thing in certain respects. It is also based on values, for example the desire for generality of a specific kind and for a conception of the natural world as morally empty and neutral. So it too insists on giving nature a certain role with respect to morality, albeit of a negative kind. This means that it too represents as natural what it takes for granted.

What may be said, however, is that the strong programme possesses a certain kind of moral neutrality, namely the same kind as we have learned to associate with all the other sciences. It also imposes on itself the need for the same kind of generality as other sciences. It would be a betrayal of these values, of the approach of empirical science, to choose to adopt the teleological view. Obviously these are not reasons which could compel anyone to adopt the causal view. For some they may be precisely the reasons that would incline them to reject causality and adopt asymmetrical, teleological conceptions. But these points do make clear the ramifications of the choice and expose those values that are going to inform the approach to knowledge. From this

type of confrontation, then, the sociology of knowledge can proceed, if it so chooses, without let or hinderance.

The Argument from Empiricism

The premise underlying the teleological model was that causality is associated with error or limitation. This represents an extreme form of asymmetry and so stands as the most radical alternative to the strong programme with its insistence on symmetrical styles of explanation. It may be, however, that the strong programme can be criticised from a less extreme standpoint. Is it not plausible to say that some causes bring about erroneous belief whilst others bring about true belief? If it further transpires that certain types of causes are systematically correlated with true and false belief, respectively, then here is another basis for rejecting the symmetrical standpoint of the strong programme.

Consider the following theory: social influences produce distortions in our beliefs whilst the uninhibited use of our faculties of perception and our sensory-motor apparatus produce true beliefs. This praise for experience as a source of knowledge can be seen as encouraging individuals to rely on their own physical and psychological resources for getting to know the world. It is a statement of faith in the power of our animal capacities for knowledge. Give these full play and their natural, but causal, operation will yield knowledge tested and tried in practical interaction with the world. Depart from this path, rely on one's fellows, and one will be prey to superstitious stories, myth and speculation. At best these stories will be secondhand belief rather than first-hand knowledge. At worst the motives behind them will be corrupt, the product of liars and tyrants.

It is not difficult to recognize this picture. It is a version of Bacon's warning to avoid the Idols of the Market Place and the Theatre. Much of standard empiricism represents a refined and rarified statement of this approach to knowledge. Although the current fashion amongst empiricist philosophers is to avoid the psychological rendering of their theory the basic vision is not too dissimilar to that sketched above. I shall therefore refer to the above theory without more ado as empiricism.

If empiricism is correct then once again the sociology of knowledge is really the sociology of error, belief or opinion, but not knowledge as such. This conclusion is not as extreme as that derived from the teleological model of knowledge. It amounts to a division of labour between the psychologist and sociologist where the former would deal with real knowledge, the latter with error or something less than knowledge. The total enterprise would nevertheless be naturalistic and causal. There is therefore no question, as there was with the teleological model, of being confronted with a choice between a scientific perspective and a standpoint which embodies quite different values. Here the battle has to be fought entirely within science's own territory. Is the

boundary between truth and error correctly drawn by this empiricist conception of knowledge? There are two shortcomings in empiricism which suggest that it is not.

First, it would be wrong to assume that the natural working of our animal resources always produces knowledge. They produce a mixture of knowledge and error with equal naturalness, and through the operation of one and the same type of cause. For example, a medium level of anxiety will often increase the learning and successful performance of a task compared with a very low level, but the performance will then drop again if the anxiety level gets too high. As a laboratory phenomenon the point is fairly general. A certain level of hunger will facilitate an animal's retention of information about its environment, as in a rat's learning of a laboratory maze for food. A very high level of hunger may well produce urgent and successful learning of the whereabouts of food, but it will lower the natural ability to pick up cues which are irrelevant to the current, overriding concern. These examples suggest that different causal conditions may indeed be associated with different patterns of true and false belief. However, they do not show that different types of cause correlate simply with true and false belief. In particular they show that it is incorrect to put psychological causes all on one side of this divide, as naturally leading to truth.

No doubt this shortcoming could be corrected. Perhaps all that the counter-examples show is that psychological learning mechanisms have an optimum working arrangement and that they produce error when they are thrown out of focus. It may be insisted that when our perceptual apparatus is operating under normal conditions, and performing its functions properly, then it brings about true belief. This revision of the doctrine may be granted because there is a far more important objection to it to be considered.

The crucial point about empiricism is its individualistic character. Those aspects of knowledge which each of us can and has to furnish for himself may be adequately explained by this type of model. But how much of man's knowledge, and how much of his science is built up by the individual relying simply on the interaction of the world with his animal capacities? Probably very little. The important question is: what analysis is to be given to the remainder? It is plausible to say that the psychological approach leaves out of account the social component of knowledge.

Does not individual experience, as a matter of fact, take place within a framework of assumptions, standards purposes and meanings which are shared? Society furnishes the mind of the individual with these things and also provides the conditions whereby they can be sustained and reinforced. If the individual's grasp of them wavers there are agencies ready to remind him; if his view of the world begins to deviate there are mechanisms which encourage realignment. The necessities of communication help to sustain collective patterns of thought in the individual psyche. As well as the individual's sensory experience of the natural world, there is, then,

something that points beyond that experience, that provides a framework for it and gives it a wider significance. It fills out the individual's sense of what that overall Reality is, that his experience is experience of.

The knowledge of a society designates not so much the sensory experience of its individual members, or the sum of what may be called their animal knowledge. It is rather, their collective vision or visions of Reality. Thus the knowledge of our culture, as it is represented in our science, is not knowledge of a reality that any individual can experience or learn about for himself. It is what our best attested theories, and our most informed thoughts tell us is the case, despite what the appearances may say. It is a story woven out of the hints and glimpses that we believe our experiments offer us. Knowledge then, is better equated with Culture than Experience.

If this designation of the word 'knowledge' is accepted then the distinction between truth and error is not the same as the distinction between (optimum) individual experience and social influence. Rather it becomes a distinction within the amalgam of experiences and socially mediated beliefs that make up the content of a culture. It is a discrimination between rival mixtures of experience and belief. The same two ingredients occur in true and false beliefs and so the way is open for symmetrical styles of explanation which invoke the same types of cause.

One way of putting this point which may assist its recognition and acceptance is to say that what we count as scientific knowledge is largely 'theoretical'. It is largely a theoretical vision of the world that, at any given time, scientists may be said to know. It is largely to their theories that scientists must repair when asked what they can tell us about the world. But theories and theoretical knowledge are not things which are given in our experience. They are what give meaning to experience by offering a story about what underlies, connects and accounts for it. This does not mean that theory is unresponsive to experience. It is, but it is not given along with the experience it explains, nor is it uniquely supported by it. Another agency apart from the physical world is required to guide and support this component of knowledge. The theoretical component of knowledge is a social component, and it is a necessary part of truth, not a sign of mere error.

Two major sources of opposition to the sociology of knowledge have now been discussed and both have been rejected. The teleological model was indeed a radical alternative to the strong programme but there is not the slightest compulsion to accept it. The empiricist theory is implausible as a description of what we in fact count as our knowledge. It provides some of the bricks but is silent on the designs of the varying edifices that we build with them. The next step will be to relate these two positions to what is perhaps the most typical of all objections to the sociology of knowledge. This is the claim that it is a self-refuting form of relativism.

The Argument from Self-Refutation

If someone's beliefs are totally caused and if there is necessarily within them a component provided by society then it has seemed to many critics that these beliefs are bound to be false or unjustified. Any thorough-going sociological theory of belief then appears to be caught in a trap. For are not sociologists bound to admit that their own thoughts are determined, and in part even socially determined? Must they not therefore admit that their own claims are false in proportion to the strength of this determination? The result appears to be that no sociological theory can be general in its scope otherwise it would reflexively enmesh itself in error and destroy its own credibility. The sociology of knowledge is thus itself unworthy of belief or it must make exceptions for scientific or objective investigations and hence confine itself to the sociology of error. There can be no self-consistent, causal and general sociology of knowledge, especially not scientific knowledge.

It can be seen at once that this argument depends on one or the other of the two conceptions of knowledge discussed above, namely the teleological model or a form of individualistic empiricism. The conclusion follows, and it only follows if these theories are first granted. This is because the argument takes as its premise their central idea that causation implies error, deviation or limitation. This premise may be in the extreme form that any causation destroys credibility or in the weaker form that only social causation has this effect. One or the other is crucial for the argument.

These premises have been responsible for a plethora of feeble and badly argued attacks on the sociology of knowledge. Mostly the attacks have failed to make explicit the premises on which they rest. If they had, their weakness would have been more easily exposed. Their apparent strength has derived from the fact that their real basis was hidden or simply unknown. Here is an example of one of the much better forms of this argument which does make quite clear the standpoint from which it derives.

Grunwald, an early critic of Mannheim, is explicit in his statement of the assumption that social determination is bound to enmesh a thinker in error. In the introduction to Mannheim's 'Essays on the Sociology of Knowledge' (1952) Grunwald is quoted as saying: 'it is impossible to make any meaningful statement about the existential determination of ideas without having any Archimedean point beyond all existential determination . . . ' (p. 29). Grunwald goes on to draw the conclusion that any theory, such as Mannheim's, which suggests that all thought is subject to social determination must refute itself. Thus: 'No long argument is needed to show beyond doubt that this version of sociologism, too, is a form of scepticism and therefore refutes itself. For the thesis that all thinking is existentially determined and cannot claim to be true claims itself to be true' (p. 29).

This would be a cogent objection against any theory that did indeed assert that existential determination implied falsity. But its premise should be challenged for what it is a gratuitous assumption and an unrealistic demand. If knowledge does depend on a vantage point outside society and if truth does depend on stepping above the causal nexus of social relations, then we may give them up as lost.

There are a variety of other forms of this argument. One typical version is to observe that research into the causation of belief is itself offered to the world as being correct and objective. Therefore, the argument goes, the sociologist assumes that objective knowledge is possible, so not everybody's beliefs can be socially determined. As the historian Lovejoy (1940) put it: 'Even they, then, necessarily presuppose possible limitations or exceptions to their generalisations in the act of defending them' (p. 18). The limitations the 'sociological relativists' are said necessarily to presuppose are designed to make room for criteria of factual truth and valid inference. So this objection, too, depends on the premise that factual truth and valid inference would be violated by beliefs that are determined, or at least socially determined.

Because these arguments have become so taken for granted their formulation has become abbreviated and routine. They can now be given in such condensed versions as the following, provided by Bottomore (1956): 'For if all propositions are existentially determined and no proposition is absolutely true, then this proposition itself, if true, is not absolutely true, but is existentially determined' (p. 52).

The premise, that causation implies error, on which all these arguments depend has been exposed and rejected. The arguments can therefore be disposed of along with them. Whether a belief is to be judged true or false has nothing to do with whether it has a cause.

The Argument from Future Knowledge

Social determinism and historical determinism are closely related ideas. Those who believe there are laws governing social processes and societies will wonder if there are also laws governing their historical succession and development. To believe that ideas are determined by social milieu is but one form of believing that they are, in some sense, relative to the actor's historical position. It is therefore not surprising that the sociology of knowledge has been criticised by those who believe that the very idea of historical laws is based on error and confusion. One such critic is Karl Popper (1960). It will be the purpose of this section to refute these criticisms as far as they may be applied to the sociology of knowledge.

The reason why the search for laws is held to be wrong is that if they could be found they would imply the possibility of prediction. A sociology which furnished laws could permit the prediction of future beliefs. In principle it would seem to be possible, to know what the physics of the future would be like just as it is possible to predict future states of a mechanical system. If the laws of

the mechanism are known along with a knowledge of its initial position, and the masses and forces on its parts, then all the future positions may be predicted.

Popper's objection to this ambition is partly informal and partly formal. He informally observes that human behaviour and society just do not furnish the same spectacle of repeated cycles of events as do some limited portions of the natural world. So long-term predictions are hardly realistic. This much may be certainly granted.

The nub of the argument, however, is a logical point about the nature of knowledge. It is impossible, says Popper, to predict future knowledge. The reason is that any such prediction would itself amount to the discovery of that knowledge. The way we behave depends on what we know so behaviour in the future will depend on this unpredictable knowledge and this too will be unpredictable. This argument appears to depend on a peculiar property of knowledge and to result in a gulf between the natural sciences and the social sciences in as far as they dare to touch humans as knowers. It suggests that the aspirations of the strong programme with its search for causes and laws is misguided and that something more modestly empirical is called for. Perhaps sociology should again restrict itself to no more than a chronicle of errors or a catalogue of external circumstances which help or hinder science.

In fact the point which Popper makes is a correct though trite one which, properly understood, merely serves to emphasise the similarities rather than the differences between the social and the natural sciences. Consider the following argument which moves along exactly the same steps as Popper's but would, if correct, prove that the physical world is unpredictable. This will jerk our critical faculties into action. The argument is this: It is impossible to make predictions in physics which utilise or refer to physical processes of which we have no knowledge. But the course of the physical world will depend in part on the operation of these unknown factors. Therefore the physical world is unpredictable.

Naturally the objection will be raised that all that this proves is that our predictions will often be wrong, not that nature is unpredictable. Our predictions will be falsified in as far as they fail to take into account relevant facts that we did not know were involved. Exactly the same rejoinder can be made to the argument against historical laws. Really Popper is offering an inductive argument based on our record of ignorance and failure. All that it points to is that our historical and sociological predictions will usually be false. The reason for this is correctly located by Popper. It is that people's future actions will often be contingent on things which they will know, but which we do not know now, and of which we therefore take no account when we make the prediction. The correct conclusion to be drawn for the social sciences is that we are unlikely to make much headway predicting the behaviour and beliefs of others unless we know at least as much as they do about their situation. There is nothing in the argument which need discourage the sociologist of knowledge from developing conjectural theories on the basis of empirical and

historical case studies and testing them by further studies. Limited knowledge and the vast scope for error will ensure that these predictions will mostly be false. On the other hand the fact that social life depends on regularity and order gives grounds for hope that some progress will be possible. It is worth remembering that Popper himself sees science as an endless vista of refuted conjectures. Since this vision was not intended to intimidate natural scientists there is no reason why it should appear in this light when it is applied to the social sciences--despite the fact that this is how Popper has chosen to present it.

But still the objection must be met: doesn't the social world present us with mere trends and tendencies and not the genuine law-like regularity of the natural world? Trends, of course, are merely contingent and superficial drifts rather than reliable necessities within phenomena. The answer is that this distinction is spurious. Take the orbiting planets, which are the usual symbols of law rather than trend. In fact the solar system is a mere physical tendency. It endures because nothing disturbs it. There was a time when it did not exist and it is easy to imagine how it might be disrupted: a large gravitating body could pass close by it, or the sun could explode. Nor do the basic laws of nature even require the planets to move in ellipses. They only happen to orbit round the sun because of their conditions of origin and formation. Whilst obeying the same law of attraction their trajectories could be very different. No: the empirical surface of the natural world is dominated by tendencies. These tendencies wax and wane because of an underlying tussle of laws, conditions and contingencies. Our scientific understanding seeks to tease out those laws which, as we are prone to say, are 'behind' observable states of affairs. The contrast between the natural and social worlds on which the objection depends fails to compare like with like. It compares the laws found to underlie physical tendencies with the purely empirical surface of social tendencies.

Interestingly, the word 'planet' originally meant 'wanderer'. Planets attracted attention precisely because they did not conform to the general tendencies visible in the night sky. Kuhn's historical study of astronomy, 'The Copernican Revolution' (1957), is a record of just how difficult it was to find regularities beneath the tendencies. Whether there are any underlying social laws is a matter for empirical enquiry, not philosophical debate. Who knows what wandering aimless, social phenomena will turn into symbols of law-like regularity? The laws that do emerge may well not govern massive historical tendencies, for these are probably complex blends like the rest of nature. The law-like aspects of the social world will deal with the factors and processes which combine to produce empirically observable effects. Professor Mary Douglas's brilliant anthropological study 'Natural Symbols' (1973) shows what such laws may look like. The data is incomplete, her theories are still evolving, like all scientific works it is provisional, but patterns can be glimpsed.

In order to bring the discussion of laws and predictions down to earth it may be useful to conclude with an example. This will show the sort of law the sociologist of science actually

looks for. It will also help to clarify the abstract terminology of 'law', and 'theory' which has little practical currency in the conduct of either the sociology or history of science.

The search for laws and theories in the sociology of science is absolutely identical in its procedure with that of any other science. This means that the following steps are to be found. Empirical investigation will locate typical and recurrent events. Such investigation might itself have been prompted by some prior theory, the violation of a tacit expectation or practical needs. A theory must then be invented to explain the empirical regularity. This will formulate a general principle or invoke a model to account for the facts. In doing so it will provide a language with which to talk about them and may sharpen perception of the facts themselves. The scope of the regularity may be seen more clearly once an explanation of its first vague formulation has been attempted. The theory or model may, for example, explain not only why the empirical regularity occurs but also why, sometimes, it does not occur. It may act as a guide to the conditions on which the regularity depends and hence the causes for deviation and variation. The theory, therefore, may prompt more refined empirical theoretical work: the rejection of the earlier theory or its modification and elaboration.

All of these steps may be seen in the following case. It has often been noted that priority disputes about discoveries are a common feature of science. There was a famous dispute between Newton and Leibniz over the invention of the calculus; there was bitterness over the discovery of the conservation of energy; Cavendish, Watt and Lavoisier were involved in the dispute over the chemical composition of water; biologists like Pasteur, medical men like Lister, mathematicians like Gauss, physicists like Faraday and Davy all became embroiled in priority disputes. The approximately true generalisation can thus be formulated: discoveries prompt priority disputes.

It is quite possible to sweep this empirical observation aside and declare it to be irrelevant to the true nature of science. Science as such, it may be said, develops according to the inner logic of scientific enquiry and these disputes are mere lapses, mere psychological intrusions into rational procedures. However a more naturalistic approach would simply take the facts as they are and invent a theory to explain them. One theory which has been proposed to explain priority disputes sees science as working by an exchange system. 'Contributions' are exchanged for 'recognition' and status--hence all those eponymous laws like Boyle's Law and Ohm's Law. Because recognition is important and scarce there will be struggles for it, hence priority disputes, (Merton (1957), Storer (1966)). The question then arises of why it is not obvious who has made a certain contribution: why is it possible for the matter to become one of dispute at all? Part of the answer is that because science depends so much on published and shared knowledge, a number of scientists are often in a position to make similar steps. The race will be a close one between near equals. But second, and more important, is the fact that discoveries involve more than empirical findings. They involve questions of theoretical interpretation and reinterpretation. The changing

meaning of empirical results provides rich opportunities for misunderstanding and misdescription.

The discovery of oxygen will illustrate these complexities (Toulmin (1957)). Priestley is frequently credited with the discovery of oxygen, but this is not how he saw the matter. For him the new gas that he isolated was dephlogisticated air. It was a substance intimately connected with combustion processes as conceived in terms of the phlogiston theory. It required the rejection of that theory and its replacement by Lavoisier's account of combustion before scientists saw themselves as dealing with a gas called oxygen. It is the theoretical components of science which give scientists the terms in which they see their own and other's actions. Hence those descriptions of actions which are involved in the imputation of a discovery are precisely the ones which become problematic when important discoveries are taking place.

Now it should be possible to offer an account of why some discoveries are less prone to create priority disputes than others. The original empirical generalisation can be refined. This refinement however, will not be a simple or arbitrary limitation on the scope of the generalisation. Rather, it will take the form of a discrimination between different types of discovery prompted by the above reflections on the exchange theory. This allows for an improved statement of the empirical law: discoveries at times of theoretical change prompt priority disputes; those at times of theoretical stability do not.

Naturally the matter does not rest here. First, the refined version of the law has to be checked to see if it is empirically plausible. This of course, means checking a prediction about the beliefs and behaviour of scientists. Second, another theory needs to be developed to make sense of the new law. There is no need to go into more detail although the point may be made that a theory has been formulated which performs this task. It is provided by T S. Kuhn in his paper *The Historical Structure of Scientific Discovery* (1962a) and his book *'The Structure of Scientific Revolutions'* (1962). More will be said about this view of science in a subsequent chapter.

It does not matter for the present whether the exchange model or Kuhn's account of science, is correct. What is at issue is the general way in which empirical findings and theoretical models relate interact and develop. The point is that they work here in exactly the same way as they do in any other science.