Chapter One

The Strong Programme in the Sociology of Knowledge

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), DeGré (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, anti-scientific '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 a-causality 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 scientistic 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 Brahe's 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 non-rational 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 non-rational 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 second-hand 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 recognise 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.

Grünwald, 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) Grünwald 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). Grünwald 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 tustle 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 underly 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 researches which in turn may demand further 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.

particular case to the old particular cases. It is not mediated by any abstract entity called the meaning of the concept hat. The link is via the felt similarities and differences between the new object and the previous cases. Parental authority will soon cut across the child's natural extension of the concept and insist that really the object is not a hat but a lid. A socially sustained boundary is drawn across the flow of the psychological tendency. The child then sees a tea-cosy. Is it a lid or a hat? The choice, which may be quite obvious, spontaneous and unreflective, will be the outcome of the various response tendencies which converge on the case. The older and perhaps stronger habit will compete with the more novel restrictions. Should the tea-cosy bear an uncanny resemblance to Mother's hats then this will no doubt clinch the case, until, that is, the voice of authority draws another stern distinction.

In this simple learning situation it is not difficult to adopt a naturalistic stance and see the extension of the concepts emerging from the factors operating on the child. It is easy to feel how past experience can push this way and that. Nor is it hard to appreciate that extensions of usage are not drawn towards any alleged, real meaning of the concepts. Rather they are caused by diverse factors derived from past experience. It should be possible to transfer this perspective to the data in Lakatos's example. Of course that example did not bring out what caused the diversity of judgment about what counts as a polyhedron. This would be a matter of examining the professional commitments and backgrounds of the actors. What it does show is the scope for the operation of these factors. It is in this sense that appreciating the creative role of negotiation increases the need for a sociological perspective. It removes the myth that ideas lay down in advance the path which thinkers must follow. It removes the glib belief that the role of ideas in behaviour excludes social factors as causes as if the two were in competition.

Chapter Eight

Conclusion: Where Do We Stand?

The categories of philosophical thought form an intellectual landscape. Its great landmarks are named 'truth', 'objectivity', 'relativism', 'idealism', 'materialism', and so forth. I shall conclude by taking my bearings with respect to some of these landmarks and re-affirm which ones identify the position that I have advocated.

Throughout the argument I have taken for granted and endorsed what I think is the standpoint of most contemporary science. In the main science is causal, theoretical, value-neutral, often reductionist, to an extent empiricist, and ultimately materialistic like common sense. This means that it is opposed to teleology, anthropomorphism and what is transcendent. The overall strategy has been to link the social sciences as closely as possible with the methods of other empirical sciences. In a very orthodox way I have said: only proceed as the other sciences proceed and all will be well.

In delineating the strong programme in the sociology of knowledge I have tried to capture what I think sociologists actually do when they unselfconsciously adopt the naturalistic stance of their discipline. Danger derives from shrinking from its full implications, not from pressing forward. It is only a partial view that will be prey to inconsistencies. I have selected a number of arguments which appear to pose the central philosophical objections to the sociology of scientific knowledge. Always I have tried to respond not by retreat or compromise, but by elaborating the basic standpoint of the social sciences. Indeed the central themes of this book, that ideas of knowledge are based on social images, that logical necessity is a species of moral obligation, and that objectivity is a social phenomenon, have all the characteristics of straightforward scientific hypotheses.

The shortcomings of the views developed here are, no doubt, legion. The one that I feel most keenly is that, whilst I have stressed the materialist character of the sociological approach, still the materialism tends to be passive rather than active. It cannot, I hope, be said to be totally undialectical, but without doubt it represents knowledge as theory rather than practice. The possibility for discovering the right blend seems to me to be there, even if it has not been realised. Nothing that has been said denies the technical power and sheer practicality of much of our knowledge, but its precise relation to theory remains a worry. For example, how do our manual skills relate to our consciousness? How different are the laws which govern these two things? The most that can be said in defence is that the critics of the sociology of knowledge rarely do any better. Indeed they appear to have fewer resources for coping with the problem than those with a naturalistic approach. It is salutary to remember that Popper's philosophy makes science a matter of pure theory rather than reliable technique. He only provides an ideology for the purest scientist and leaves the engineer and craftsman without succour.

Unfortunately the process of takings one's bearings, of finding where one stands, has its snags. Like the landscape through which John Bunyan's pilgrim progressed, the topography of the intellect is not morally neutral. The high Peaks of Truth glitter invitingly, but the foul Pit of Relativism will trap the unwary. Rationality and Causation struggle with one another as if they were the forces of Good and Evil. These stock responses and conventional evaluations are as inappropriate to the sociology of knowledge as they are predictable by it. Take relativism, for example. Philosophers sometimes perplex themselves because moral relativism seems philosophically acceptable but cognitive relativism does not. Their feelings are different in the two cases so they look for reasons to justify them. Scientifically, the same attitude towards both morality and cognition is possible and desirable. Relativism is simply the opposite of absolutism, and is surely preferable. In some forms it can at least be held authentically in the light of our social experience.

There is no denying that the strong programme in the sociology of knowledge rests on a form of relativism. It adopts what may be called 'methodological relativism', a position summarised in the symmetry and reflexivity requirements that were defined earlier. All beliefs are to be explained in the same general way regardless of how they are evaluated.

One way in which the sociology of knowledge might polemically justify itself in its relativism is to insist that it is neither more nor less guilty than other conceptions of knowledge which usually escape the charge. Who charges Popper's theory with relativism? Indeed, when this charge is pressed against the sociology of knowledge doesn't it frequently come from those who are impressed by that philosophy? And yet the sociology of knowledge can easily formulate the essentials of its own standpoint in the terms of that philosophy. All knowledge, the sociologist could say, is conjectural and theoretical. Nothing is absolute and final. Therefore all knowledge is relative to the local situation of the thinkers who produce it: the ideas and conjectures that they are capable of producing; the problems that bother them; the interplay of assumption and criticism in their milieu; their purposes and aims; the experiences they have and the standards and meanings they apply. What are all these factors other than naturalistic determinants of belief which can be studied sociologically and psychologically? Nor is the situation altered because explaining behaviour and belief sometimes involves making assumptions about the physical world which surrounds the actors. This only means that conjectures from say, physics or astronomy are used as subsidiary hypotheses. If Popper is right this knowledge is conjectural too. The whole of the explanation is a conjecture, albeit a conjecture about other conjectures.

Similarly a sociologist can embrace Popper's insistence that what makes knowledge scientific is not the truth of its conclusions but the procedural rules, standards and intellectual conventions to which it conforms. To say that knowledge is a question of standards and conventions is but to say it is a question of norms. A conventionalist theory of knowledge such as Popper's can be looked on as the abstract skeleton of a more realistic sociological account of knowledge.

To see all knowledge as conjectural and fallible is really the most extreme form of philosophical relativism. But Popper is surely right to believe that we can have knowledge, and scientific knowledge, that is nothing but conjecture. What constitutes the very existence of science is its status as an ongoing activity. It is ultimately a pattern of thought and behaviour, a style of going about things which has its characteristic norms and values. It does not need any ultimate metaphysical sanction to support it or make it possible. There need be no such thing as Truth, other than conjectural, relative truth, any more than there need be absolute moral standards rather than locally accepted ones. If we can live with moral relativism we can live with cognitive relativism.

Science may be able to work without absolute truth, but, such a thing might still exist. This residual feeling surely rests on a confusion between truth and the material world. It is the external, material world that really seems to be in mind when it is insisted that there must be some permanent truth. This instinct seems unassailable. But to believe in a material world does not justify the conclusion that there is any final or privileged state of adaptation to it which constitutes absolute knowledge or truth. As Kuhn has argued with great clarity scientific progress—which is real enough—is like Darwinian evolution. There is no goal for adaptation. No meaning can be given to the idea of perfect or final adaptation. We have reached the present position in the progress and evolution of our knowledge, as we have in the evolution of our species, with no beacon to guide us, nor any goal.

Just as the sociology of knowledge is accused of relativism, as if it were a crime rather than a necessity, so it will be accused of subjectivism. Where does the sociology of knowledge stand with regard to the Rock of Objectivity? Does it say that truly objective knowledge is impossible? Emphatically it does not. What was proposed in the discussion of Frege, for example, was a sociological theory of objectivity. If objectivity had been held to be non-existent there would have been no need to develop a theory to account for it. Nor is this a way of saying that objectivity is an illusion. It is real but its nature is totally different from what may have been expected. It is other theories of objectivity which are denied by a sociological account, not the phenomenon itself. Those who elect to be champions of scientific objectivity might reflect on the following: a sociological theory probably accords objectivity a more prominent role in human life than they do. On this theory moral knowledge can be objective too. Like many features of a landscape, knowledge looks different from different angles. Approach it from an unexpected route, glimpse it from an unusual vantage point, and at first it may not be recognisable.

No doubt I will be exposed to the further charge of 'scientism', that is, an over-optimistic belief in the power and progress of science. Amusingly this criticism will have to stand shoulder to shoulder with another charge, which has been examined at length: that this scientistic approach, when practised by the sociology of knowledge and applied to science itself, is a denigration of science. I have given reasons why this contradiction should be laid at the door of the critics rather than the strong programme. Nevertheless the charge of scientism is well aimed. I am more than happy to see sociology resting on the same foundations and assumptions as other sciences. This applies whatever their status and origin. Really sociology has no choice but to

rest on these foundations, nor any more appropriate model to adopt. For that foundation is our culture. Science is our form of knowledge. That the sociology of knowledge stands or falls with the other sciences seems to me both eminently desirable as a fate, and highly probable as a prediction.

Afterword: Attacks on the Strong Programme

Since its publication in 1976 'Knowledge and Social Imagery' has won few friends and many enemies. It has been denounced by sociologists as 'sociologically irrelevant' and a 'failure' (Ben-David (1981), p. 46, p. 54); by anthropologists as 'socio-centric' and incompatible with the 'unicity' of human nature (Archer (1987), pp. 235–36); by cognitive scientists as 'recidivist' and 'recycling classical . . . text-book mistakes' (Slezak (1989), p. 571); and by philosophers for being 'manifestly preposterous' and 'catastrophically obscurantist' (Flew (1982), p. 366). Behind these errors critics have seen the sinister hand of ideology and have identified it as Marxist, irrationalist, anti-scientific and behaviorist. Such polemics certainly enliven the dull routine of academic enquiry. I enjoy them as much as anyone, but there are dangers. The sociology of knowledge needs a cool head. We must avoid emotive stereotypes whether of science or of one another. Those who content themselves with stereotypes, rather than attending to the precise details of what sociologists of knowledge have written, will fail to grasp even the most central doctrines of the position they are attacking. As a salutary example, consider the arguments of Bartley (1987).

How Not to Attack the Strong Programme

W. W. Bartley lists this book, along with other work by Edinburgh colleagues, as representative of current approaches to the sociology of knowledge (p. 442, fn. 25). He says that his discussion 'can deal with it only in broad outlines'. He will not, he says, 'treat individual practitioners' (p. 443). The result is that he attacks a view that is the very opposite of that defended in the works cited. He thinks the sociology of knowledge is the study of how social processes *distort* knowledge. His complaint is that sociologists don't go far enough in the task of rooting out such distorting factors. Thus:

> If the problem that attracts sociologists of knowledge is distortion, then sociologists of knowledge need to take account of all kinds of distorting influences, those that attend all knowledge vehicles, and not only distortions of a social character (p. 446).

But this is not the problem that attracts the sociologist of knowledge. Indeed the picture that Bartley paints, depending as it does on an evaluative stance, is the very one that this book is devoted to rejecting. (See, for example, pp. 8–13.) The meaning of the symmetry postulate (to be discussed in detail later) is that our best and most cherished scientific achievements could not exist as they do without having the character of social institutions. They are therefore as socially influenced, and as sociologically problematic, as any other institution. Their social character is not a defect but part of their perfection.

There is much in Bartley's paper, as there is in the writings of other critics, that is interesting. What a pity that he missed the opportunity genuinely to engage with the sociologists of knowledge. He would have found, for example, that one of his own favoured positions, far from contradicting their position (as he thinks), is actually *shared* with it. The main positive thesis of Bartley's paper is introduced by his saying that he 'learnt from Popper that we never know what we are talking about' (p. 425). He means by this that we never arrive at a final grasp of the essence of things. Our knowledge is always provisional and conjectural, and even the meaning of our concepts is likely to change as new theories are advanced to cope with unexpected new facts. But this is not something that flies in the face of the sociology of knowledge at all. It is central to it, and is recognised under the name of 'finitism'. The idea comes from Mill and Wittgenstein, though the use of the label in this connection is taken over from Hesse. (See Hesse (1974); Barnes (1982), ch. 2.) We must think of the application of a concept moving from case to case, mediated by complex judgments of similarity and difference, and informed at all points by the local purposes of the concept users. Crudely, meaning is constructed as we go along. It is the residue of past applications, and its future applications are not fully determined by what has gone before. In this sense, therefore, the sociologists' 'finitism' conforms to Bartley's picture of our not knowing 'what we are talking about'. Of course

Bartley's own theory is not the one derived from Mill or Wittgenstein, but the fact remains that the phenomenon itself is common ground. Just as Bartley connects the unfathomability of our concepts with their objectivity, so do sociologists of knowledge, though for them, as I argued in my discussion of Frege in chapter 5, objectivity is social. Indeed finitism is probably the most important single idea in the sociological vision of knowledge. It shows the social character of that most basic of all cognitive processes: the move from one instance of concept application to the next. Failure to see this, along with the mistake of confusing the strong programme with the weak programme (i.e., the 'distortion' paradigm), vitiates Bartley's contribution.

Covariance, Causality and Cognitive Science

The classic problems facing the sociology of knowledge are covariance and causality (Merton (1973)). Let S = society and K = knowledge: then if S is the cause of K, varying S ought to produce variation in K. Should we discover that S can vary while K stays the same, then S cannot be the cause of K. And that, it seems, is what we find. Ben-David (1981) surveyed some of the historical case studies cited in support of the strong programme, and declared that they failed the tests of covariance and causality. He asked

> whether the relationship between the social interests of scientists and their scientific ideas exists only in some or in all cases; and whether social interest or perspective initially associated with a theory . . . continues to exist over time, thus perpetuating ideological bias in the guise of scientific tradition (p. 51).

His answer was negative. Such studies show that 'ideological bias is not a general phenomenon in science' (p. 51).

Although objections may be raised against this way of posing the problem (e.g., it is formulated wholly within the 'distortion' stereotype), the general point seems right. We do not find, for example, that field theories in physics are associated exclusively with organic social forms, or atomic theories with individualistic societies. Such general connections would break down if only because theories created by one group are taken over by other groups as inherited cultural resources. This is not, however, fatal to the sociology of knowledge. It rules out one simple and implausible definition of the exercise, but leaves others intact. The lack of 'systematic relationships' between 'social location' and 'types of theory'—to use Ben-David's terms—

may depend on how broadly 'type' is defined. Ben-David's argument overlooked the possibility that sociologists may yet explain why an inherited body of ideas is modified in the way it is, even if the resulting theory is of the same general type. For example, one of the studies Ben-David cited showed how ancient atomism (in which matter was self-moving and self-organising) was taken over by Robert Boyle and modified by his insistence that matter was passive and that only force was active (see Jacob (1978) discussed in Bloor (1982)). Even though the modification was made to further an identifiable interest of a political kind, the fact that the theory was still of the same type (viz. an atomic theory) means that on Ben-David's perspective the covariance and causality passes unnoticed. This enabled him to treat the study as if it were evidence against the sociology of knowledge, instead of—as it really is—evidence for it.

This still leaves untouched Ben-David's predictable conclusion that only some, and not all, episodes in the history of science are found to be crucially dependent on particular, social interests. We must, of course, remember that not all interests are of the broad, political kind identified in the Boyle case mentioned above; some are narrow, professional interests. But still the point remains, and it is surely correct. It would, however, be fatal only to the claim that knowledge depended *exclusively* on social variables such as interests. Such a claim would be absurd, and has certainly not been defended in this book (see, for example, fig. 1, p. 32). No defensible picture of knowledge should rule out the scenario in which, for example, sensory experience impinges on a body of people and triggers a change in their culture. Such contingencies do not remove or trivialise the social component in knowledge; they merely put it in the background, and presuppose it, while the explanatory spotlight turns elsewhere. The only theory to be embarrassed by such possibilities would be a mono-causal story which denied a role for anything but social processes, i.e., the near meaningless claim that knowledge is 'purely social' or 'merely social'. By deploying his evidence in the way he did Ben-David tacitly foisted such a theory onto the sociologist of knowledge. But doesn't the strong programme say that knowledge is purely social? Isn't that what the epithet 'strong' means? No. The strong programme says that the social component is always present and always constitutive of knowledge. It does not say that it is the *only* component, or that it is the component that must necessarily be located as the trigger of any and every change: it can be a background condition. Apparent exceptions to covariance and causality may be merely the result of the operation of other natural causes apart from social ones.

What does this say about the search for 'laws' in the sociology of knowledge? It means that any such laws will exist, not on the surface of phenomena, but interwoven into a complex reality. In this respect they will be no different from the laws of physics. They will become more readily visible the more other contributory factors are held stable. Their surface manifestations are likely to be statistical tendencies whose strength will vary widely, not because they are themselves statistical, but because the conditions of their visibility are contingent. But what will such laws look like? Critics have taunted sociologists for not producing 'precisely specified and testable putative covering laws' (e.g., Newton-Smith (1981), p. 263). I would offer the following. Finitism itself, as described in the previous section, is a general truth about the social character of concept application to which there are no exceptions. Thus, *all* concept application is contestable and negotiable, and *all* accepted applications have the character of social institutions. Such laws are not what critics expect in answer to their challenge, but perhaps that reflects more on them than on the sociology of knowledge. Putative laws which are closer to what such critics may have in mind follow from Douglas's 'grid-group' theory, linking cosmological style to social structure. Such candidates are indeed putative rather than well confirmed or well tested, but they are a start. I have discussed them in connection with Lakatos's description of responses to mathematical anomalies in Bloor (1978), and with regard to the work of industrial scientists in Bloor & Bloor (1982).

The false imputation that knowledge is 'purely social' also lies behind the claim that there is a fundamental incompatibility between the strong programme and recent work in cognitive science (cf. Slezak (1989)). Allegedly, the sociology of knowledge presupposes 'behaviorism' and is therefore contradicted by any work which furnishes an account of the internal machinery of our thinking. In particular, there are now computer models that can mimic the thought processes involved in scientific discovery. Equipped with a few general, heuristic principles, computers have been fed data from which they have been able to extract patterns having the form of natural laws. Stated dramatically, computers have shown that they can discover such regularities as Boyle's Law, Ohm's Law, Snell's Law, etc. (p. 569). Who needs the sociology of knowledge now? Psychology will suffice. Such work, the critic says, has vindicated the 'traditional epistemology' that was rejected in this book. In particular it vindicates the 'teleological' model that I was seeking to replace. The upshot is said to be 'as decisive a refutation of the strong program as one is likely to get' (p. 592).

It remains an open question whether the computer's way of extracting pattern from the data is the brain's way, but despite this, such work is surely to be welcomed. The only sociologists to be upset by it would be those foolish enough to deny the need for a background theory about individual cognitive processes. I take it as evident that you could have no social structures without neural structures. Cognitive science, of the type described, is a study of just that background of 'natural rationality' that advocates of the strong programme take for granted. See, for example, Barnes (1976) on our natural inductive propensities, and Bloor (1983, ch. 6) on our natural deductive propensities. The correct position for the sociologist to take is that, while a theory of our individual reasoning capacities is necessary to an account of knowledge, it is not sufficient.

To see why, let us grant that our brains have exactly the degree of information processing ability that the critics' cognitive models suppose. I will show that this neither removes nor trivialises the social aspects of knowledge. Let person A extract, say, Boyle's Law from a set of measurements, and let B, C, etc., possess the same cognitive powers and address similar data. We now have a set of individuals, each with their own personal technique for making sense of their experience. Each has their own personal version of Boyle's Law. We do not, however, have a group who know Boyle's Law as we know it, because we don't yet have a version of the scientific community with a shared body of knowledge. All we have is a computerised version of what philosophers used to call the 'state of nature', i.e., individuals in isolation from society.

The missing element is the interaction of A, B, C, etc., the interaction that would create a society. To supply this, let us now suppose that A, B and C try to coordinate their actions with one another. They will then confront the problem of social order, and to solve it they will discover that they also need to solve the problem of cognitive order. They must coordinate their personal techniques of cognition. Their problem will be to control and keep at bay the anarchy of private judgment. If it is said that this does not arise in cognitive science because the computers are identical and faultless and work on identical data, then this simply disqualifies the model as unrealistic. Realistically we must allow that often different individual brains or computers will be working with different sets of data, and that even those with identical sets will periodically get different results. There is therefore the problem of deciding who has the 'right' data, and who has drawn the 'right' conclusion from it. Indeed the notion of 'rightness' itself awaits construction. These problems are aggravated by the fact that any agreed law will soon encounter anomalies. The task of mobilising a consensus about the correct response to them will then confront the divergent goals and interests of the parties involved.

The sociologists thus have a subject matter that exists over and above that of the cognitive scientists whose work has been cited against them. The former, but not the latter, study how a collective representation of the world is constituted out of individual representations. This shared conception of the world as governed, for example, by Boyle's Law, will be held by the group as a convention, not as an atomised set of individual dispositions. Roughly, this means that one of the factors sustaining A's belief is that B and C, etc., hold it, and in holding it, take for granted that A holds it. This reciprocal understanding helps maintain the steadiness of the belief in the face of individual tendencies to diverge. The particular content of the shared belief, embodying as it does responses to anomaly and decisions relating it to the rest of the culture, will be the outcome of the interaction between A, B, C as they seek to negotiate a consensus. The negotiation is a social process whose outcome will be determined by all the natural contingencies that can impinge on it. For a study of the quite remarkable interests that historically impinged on the negotiations surrounding Boyle's original air-pump experiments, see Shapin and Schaffer (1985).

Before moving to further objections two residual points need to be clarified. First, Ben-David has argued that because a negotiation is a social process we should not infer that its outcome is socially determined. It could be that it is 'rationally determined' ((1981), p. 45). Given the traditional rationalist dichotomy between the rational and the social (i.e., the 'distortion' model), this caution is correct. But once the rationalist assumptions are put aside in favour of a naturalistic perspective, then the inference is good. What gives the objection some interest, even for a naturalist, is that assumptions about natural rationality may play a role in the negotiation of a convention. A and B naturally draw certain inferences and assume that C and D will do likewise, and that they will be entertaining the same expectations of them. Precisely because certain reasoning tendencies are natural they will have a salient position in the reciprocal reasoning that underlies our convention building. They will therefore enter into our conventions and even themselves become built up into conventions. None of this, however, destroys the qualitative difference between individual and collective or conventional representations.

Second, it should be clear that no (naturalistic) theory of our natural rationality, and therefore no computer model of thought, is really going to be acceptable to traditional epistemologists. It is simply wrong to assume—as my critic assumed—that these causal accounts can be equated with the teleological assumptions that I identified behind the rationalist attacks on the sociology of knowledge. (Examining what Flew (1987, p. 415) says on the subject of computers will give a sense of the difference. See also Geach (1977, p. 53).) The failure to appreciate the fundamental opposition between traditional, rationalist accounts of knowledge and naturalistic accounts is something we will meet again in the discussion of the symmetry postulate. For the moment the point to retain is that cognitive science and the sociology of knowledge are really on the same side. They are both naturalistic and their approaches are complementary.

The Ultimate Refutation of Interest Explanations

Numerous revealing historical studies of scientific disputes invoke the role of interests—cf. Shapin (1982), who lists some dozen titles alone under the narrow heading of 'vested professional interests'. The value of such studies is that they focus on events which bring into view the social substructure of science that is usually obscured in everyday practice. By seeing how disputes are resolved we come to see the conventional character of the forces that are lying dormant. This remains true even if the particular clashes of interest that provoked the dispute die away as the historical scene shifts. For example: in the 1820s Edinburgh was the scene of a sharp controversy over the anatomy of the brain. University anatomists, abetted by the local philosophers, saw the brain as relatively homogenous and unified. The followers of phrenology saw it as a republic of different faculties. Both sides fielded competent anatomists and conducted careful dissections, but could not reach agreement about, among other matters, the structure of various organs within the brain, or the pathways of the fibres connecting them to the brain stem. Shapin (1975, 1979a, 1979b) has argued that these disagreements can be made intelligible by relating the positions taken up to the interests of the disputing parties. The university people were an elite group whose esoteric knowledge embodied a subtle ideology of social hierarchy and unity. Their critics were drawn mainly from the mercantile middle classes of the city, who were looking for readily accessible, practical knowledge about people and their talents, to justify their calls for reform and their desire to create a more diversified and egalitarian social structure. Both sides, argues Shapin, can be seen to be putting nature to social use, making it underpin their vision of society and their role in it.

Arguments of this kind have met enormous resistance. Undeniably the terminology of interest explanations is intuitive, and much about them awaits clarification, but instead of seeing these as practical difficulties their critics see them as weaknesses of principle. Central to these complaints is the suggestion that appeals to interests involve the historian in an infinite regress. The premise is that interests must always be interpreted by the actors themselves. These interpretations, being loose and revisable, destroy the connection between the interest and the behaviour it is mean to explain.

Why, asks Brown (1989), are interests introduced in the first place? It is because, allegedly, scientific theories are undetermined by the data. Observations in the dissecting rooms didn't really prove the case for or against the phrenologists, so social interests must have tipped the balance. Insufficient evidence *seemed* sufficient to minds thus predisposed. Clearly it doesn't follow from underdetermination alone that what tips the balance is social, but even if we allow this step the account won't work because it raises exactly the same problems again. If observation won't determine, then neither will interests. Just as observation is compatible with many theoretical interpretations, so are interests. Brown says:

> A particular theory T may serve a scientist's interests, but more than one theory will do that. In fact, just as there are infinitely many different theories which do equal justice to any finite set of empirical data, so also are there infinitely many theories which will do equal justice to a scientist's interests (p. 55).

The idea that there is an 'infinity' of theories to choose from is not essential to the argument, but that may be passed over. The point is that if the sociologist postulates a further interest I_2 to explain why one theory is chosen from all the candidates that could express interest I_1, then we start an infinite regress. In historical rather than logical terms Brown is posing the question of why the Edinburgh middle classes chose *phrenology* when numerous other theories would serve their interests equally well (p. 55). Interest explanations are thus caught between underdetermination and infinite regress. This, says Brown, is the 'ultimate refutation' (p. 54).

I will begin with the historical problem, and then formulate the reply in more general terms. In the works cited, Shapin had anticipated Brown's question. It is true that other theories could have expressed middle-class interests as well as did phrenology. Indeed phrenology could be seen as a bad choice. A theory was needed to legitimate reform and change, and phrenology, as it was developed by its founders, was about people's inborn character traits. Its Edinburgh followers therefore modified it by saying that native endowment could be strengthened or weakened by exertion and use. All that really mattered, Shapin suggests, is that some theory could be found that could plausibly stand as a negation of the existing philosophy of 'common sense'. Perhaps anything materialistic, empiricist, and non-esoteric would have served as the not-X to the elite X. It was an historical contingency that phrenology was available, so it had to suffice (Shapin (1975), pp. 240–43).

This reply acknowledges the underdetermination on which the criticism depends, but solves the problem by reference to chance. Once chance favours one of the many possible candidates, then it can rapidly become the favoured vehicle for the expression of interest. Because a few people see how a theory might be used, and put it to work, others take up the cry. Its use by others becomes an added reason for using it. The mechanism implicit in this sketch is in fact quite precise, and there are even mathematical models of it developed by economists. These have been used to explain why markets produce stable, but often non-optimum, solutions to certain problems. They explain, for instance, how one of two competing technologies might come to dominate the other (even if it is not the superior technology); or how a particular geographical distribution of industry arises (even if it is not the best). Their leading idea is that stable solutions are achieved through positive feedback. The fact that some people use a technology becomes a reason for others to use it. The fact that an industry is already located at a place becomes a reason for others to be located there. Small but random advantages at the beginning of the process—or some chance initial distribution—become reinforced by positive feedback until the system achieves a highly stable but extreme solution, the total domination of one option (Arthur (1990)). Such mechanisms could explain how the Edinburgh middle classes could become locked onto phrenology in precisely the circumstances of underdetermination that the critic describes.

Isn't it still true that interests always have to be interpreted? That fact alone, it has been said, is sufficient to generate an infinite regress. In support Yearley (1982) cites work on rule-following which emphasises the interpretative character of their practical applications. He suggests that sociologists who appeal to interests will be in the position of citing rules for following rules, and so on *ad finitum* (p. 384). But, surely, the rule-following literature points in the opposite direction, and provides the answer to the regress objection. Wittgenstein pointed out that since we can properly be said to follow rules, there *must* be a way of following them that *doesn't* involve interpretation (Wittgenstein (1967), sect. 201). The analogy with interests that we have been invited to employ would thus lead us to reject the premise

of this attack. Interests *don't* have to work by our reflecting on them, choosing them, or interpreting them. Some of them, some of the time, just *cause* us to think and act in certain ways. The real basis of the objections to interest explanations is the fear of causal categories. It is the desire to celebrate freedom and indeterminacy, and the reluctance to construct explanations rather than simply describe.

These replies don't solve what I have called the 'practical' problems that attend the use of interest explanations. They do, however, answer the charge that such explanations are caught in a dilemma between underdetermination and infinite regress. They therefore show that the 'ultimate' refutation isn't a refutation at all.

The Charge of Idealism

Flew (1982) must speak for many when he says sociologists of knowledge aim, covertly,

> to disqualify, as possible causes of the beliefs which do happen to be true, all the effects upon the believer of the facts about which he comes truly to believe (p. 366).

The cause of the trouble, Flew believes, is the symmetry postulate. Reference to the facts has to be denied in order to put true beliefs on a par with false beliefs, so they can be said to have the same kind of cause (p. 366). Sometimes the accusation is expressed in terms of ignoring 'the causal influences of the subject matter of beliefs' (p. 368), or the efficacy of the 'objects actually perceived' (p. 367). So 'fact', 'object' and 'subject matter' are used interchangeably. But what are 'facts'? Unfortunately the term is taken to be well understood. In reality it is the source of much perplexity. Thus the dispute between Strawson and Austin on truth turned on the question of whether 'facts' are what true statements *state*, or whether they are what such statements are *about* (Strawson (1950); Austin (1961)). Flew's attack is not well defined with regard to this choice, but we shall see that it leads to two quite different questions for the sociology of knowledge. Fortunately, both can be given answers consistent with the materialism of the strong programme.

Take the facts-as-objects view. Here we must separate facts from their verbal formulations. In this case the result of the symmetry postulate is the opposite of what Flew says. Objects in the world will in general impinge equally on those who have true and those who have false beliefs about them. Consider Priestley and Lavoisier looking at some burning chemicals. They both see the same objects in the world; they both direct their attention and their remarks at the same things. But one says: 'In combustion a burning object releases phlogiston into the atmosphere', and the other says: 'In combustion a burning object takes oxygen from the atmosphere'. There is no question of disqualifying as possible causes the objects before them. Such causes do not however suffice to explain the verbal description that is given of them. This is so both for the versions we ourselves accept as true and for the versions we reject as false. (For an excellent discussion using an historical example, see Barnes (1984).)

Now consider facts as what-statements-state rather than what they are about. Here facts fall on the side of the 'content' of propositional attitudes rather than on that of their 'objects'. We are dealing, however, with a subclass of such contents, that is: beliefs picked out by their truth and thus standing in a privileged relation to reality. What is the class thus picked out? Is it a natural kind of belief, or something analogous to a natural kind? Chemists discovered that there are two oxides of copper; have philosophers discovered that there are two kinds of belief, distinguished by whether they possess or lack the property of corresponding to reality? Such a claim, however, could never be made good. We can't play God and compare our understanding of reality with reality as it is in itself, and not as it is understood by us. (See pp. 37–40.) But if truths don't form a natural kind, what manner of class do they form? The alternative to their forming a natural kind is that they form a social kind. They form a class like the class of valid banknotes, or the class of holders of the Victoria Cross, or the class of husbands. Their membership in this class is the result of how they are treated by other people, though we must never forget that the reason for that treatment will be practical, complicated, and itself part of reality.

There *are* interesting attempts to argue that true statements form a genuine natural kind, e.g., by treating them as entities that sustain a determinate biological and functional relationship with reality (cf. Millikan (1984)). Such explorations are naturalistic, and have shed much light on semantic issues. Nevertheless they tacitly substitute another relationship—such as 'being adapted'—for 'being true'. Here the reaction of the sociologist is similar to that of the traditional epistemologist: something has been left out. A full analysis of truth must do justice to our sense of its special and elevated character, that which raises it above mere nature and generates the obligation that we feel toward it. The last thing that a sociological account of truth can afford is an insensitivity to, of all things, its *status*. Our response here must be modeled on Durkheim's response to pragmatism: to welcome all naturalistic accounts, but to correct them insofar as they fail to account for the special authority that truth exerts over us (Durkheim (1972)).

But isn't this idealism after all? Surely this is all a disguised way of saying that truth is all in the mind of the believer, or that it is just a projection of our collective attitudes? If this *is* a species of idealism, it is at most an idealism of certain aspects of things or an idealism of things under some description or in some role. It would thus be a form of 'idealism' that is compatible with an underlying materialism. It would be, at most, an idealism about the semantic dimension of current forms of realism, but not an attack on its ontological dimension. It would also be strictly limited in its scope. For notice: a banknote is ultimately a banknote because we collectively deem it to be so. For all that, it is a real thing with weight and substance and location. None of this materiality is denied by what has been said about its social status as a banknote. The same applies to the people who occupy a social role. They are flesh and blood. That material reality is not denied but presupposed by their social status.

Where does this leave the charge that the sociological approach neglects the part played by the facts as causes of our beliefs about them? On the first meaning of this ambiguous accusation, where facts are objects, I have shown this is false. On the second meaning, where facts are the content of beliefs, the charge is, in a way, correct. Leaving aside certain subtleties, the content of a belief is not to be treated as the cause of the belief. But that is because it *is* the belief. Nevertheless, critics may feel, as Flew does (p. 370), that they are getting contradictory signals from the sociologist about the causal role of facts. They are not. They are getting consistent answers to two quite distinct questions—one about the role of reality, the other about the status of reports of reality. They are just mistaking these answers for inconsistent responses to the same question.

Symmetry Lost and Symmetry Regained

The symmetry postulate, which enjoins us to seek the same kind of causes for both true and false, rational and irrational beliefs, seems to fly in the face of common sense. Our everyday attitudes are practical and evaluative, and evaluations are by their nature asymmetrical. Similarly with our curiosity. Typically things which are unusual or threatening attract our attention. Ultimately this is rooted in the physiology of habituation, the process by which our brains rapidly adapt to background conditions and preserve their information processing capacity for whatever breaks the local routine. Because much of our background consists of social regularities, this alone is sufficient to ensure that our curiosity is socially structured. The symmetry requirement is the call to overcome these tendencies, and to restructure our curiosity. Fortunately, it doesn't require us to transcend the physiological laws of our own nervous tissue, but it does require us to reconstruct the local social background to which our curiosity is adapted. We can do this by creating new, specialist groups with their own taken-for-granted, professional perspective.

Two residual forms of asymmetry will be left intact by these new structures of curiosity. I will call them 'psychological asymmetry' and 'logical asymmetry'. Neither is inconsistent with the original requirement, which, to differentiate it, may be called 'methodological asymmetry'. I will look at each in turn. When anthropologists study, say, a witchcraft culture they are implicitly asking what circumstances would permit a rational person to embrace such beliefs. This question can be addressed and answered without becoming a believer. It is consistent with a residual evaluation that such beliefs are false. This is the psychological asymmetry referred to above. It is consistent with methodological *symmetry* because the character of the desired explanation is independent of the evaluation. It is the same kind of explanation as would be appropriate if the institutionalised belief under study happened to be one the anthropologist could accept. The assumption here is that no institutionalised body of belief depends on its adherents having defective brains or lacking natural rationality.

Members of a witchcraft culture will say they believe in witches because they encounter witches. An anthropologist might say it is because they are symbolising their social experience of living in a small disorganised group prone to scapegoating. The anthropological theory will logically imply that the witchcraft beliefs (taken at their face value) are false. This inconsistency is the logical asymmetry referred to. The existence of such an asymmetry has been emphasised by Hollis in his attack on the symmetry requirement. He says that the sociologist

> must also produce his own explanation of why the actors believe what they believe. In doing so, he cannot fail to endorse or reject the actors' own reasons or, where the actors are not of one mind, to side with some against others. I shall argue . . . that endorsing and rejecting are not symmetrical (Hollis (1982), p. 77).

It is true that endorsing and rejecting are not symmetrical, but this nevertheless leaves the methodological symmetry intact. I will now explain why.

The sociologist of knowledge is committed to some picture of what is really happening. Some characterisation must be offered of what actors are responding to, of what experience they have of their environment, and of what purposes inform their interaction with it and with one another. Such assumptions must be made to get explanation under way, and sometimes (though not always) these may carry logical implications about the truth of the actors' beliefs. But, as we have seen, there is another step in the explanatory story that goes beyond these assumptions. The interesting question is how the world is going to be described by the actors under study. That the world doesn't contain witches leaves open the question of whether it will or will not be believed to contain witches. Having chosen the true option is no less problematic than having chosen the false one: that is what methodology symmetry amounts to.

Newton-Smith (1981, p. 250) says that the idea of 'methodological symmetry' represents a weakening of the original symmetry requirement. The charge rests on the premise that originally the requirement was an 'attack on the very notions of true and false, reasonable and unreasonable' (p. 248). He suggests that the assumption behind the symmetry requirement is that these distinctions are all 'somehow bogus'. Because acknowledging psychological and logical asymmetry is hardly consistent with treating these notions as bogus, I am seen to be in retreat. There is no retreat, however, because the original position did not treat these distinctions as bogus. Far from thinking them bogus I take them to have the greatest utility, and was at pains to spell out their main practical functions (cf. pp. 37–43). There is nothing wrong with using such terms as 'true' and 'false': it is the accounts given of this use that are suspect.

The problem running throughout most exchanges over the status of the symmetry requirement lies in the clash between a naturalistic and a non-naturalistic perspective. The symmetry requirement is meant to stop the intrusion of a non-naturalistic notion of reason into the causal story. It is not designed to exclude an appropriately naturalistic construal of reason, whether this be psychological or sociological. Brown (1989), for example, is typical in mistaking the sociologist's rejection of a non-naturalistic notion of reason as a rejection of reasoning as such.

This diagnosis may be resisted on the grounds that some of the critics of symmetry take their own position to be a form of naturalism. Newton-Smith rejects the symmetry requirement in the name of rationalism, but a rationalism that he seeks to ground in Darwin's theory of evolution. When someone is following the dictates of reason we don't need to enquire any further, but that is because it is a 'brute fact' that being reasonable has survival value. We therefore have a 'standing interest' in being reasonable (p. 256). Here we seem to have an alliance between naturalism and rationalism. Such composite positions, however, are incoherent. They are trying to meet an impossible condition: making reason both a part of nature and also not a part of nature. If they don't put it outside nature, they lose their grip on its privileged and normative character; but if they do, they deny its natural status. They can't have it both ways.

Clear-headed rationalists know what is at stake. Worrall (1990) is firmly against the symmetry requirement and its implied relativism, but sees the weakness of Newton-Smith's appeal to evolution. This can't be ultimate for a rationalist, because there is still the task of justifying our belief in this theory and saying how we know it is true. To do this we must suppose that we can intuit evidential relations and some logical truths. So even here we need access to a realm of epistemological facts, that is: 'abstract, non-physical facts' (p. 314). (The same argument is used with an explicitly theological intent by Geach (1977), p. 51.) This abstract, nonphysical realm must exist over and above the flux of biological and cultural change if it is to be used to explain and justify it. If it were grounded in evolution it would have no more probative force than any other disposition or natural tendency. Above all, this 'code of reason' must be the *correct* one (p. 315). 'As I see it', says Worrall, 'what the rationalist accepts and her naturalizing opponent denies is a world of logical facts over and above any psychological ones' (p. 316). (It would have been better to add 'psychological *and* social ones'.) Worrall, rightly, takes his argument to show that

> *any* attempt to use the evolutionary version of naturalized epistemology to avoid relativism, while at the same time avoiding commitment to logico-epistemological truths, is doomed to failure (p. 318).

Worrall's picture is clear from his analysis of logical inference. A and B reflect on a piece of logical reasoning. It is invalid, but A sees this and B doesn't. The case is treated by analogy with visual perception. A simply sees what is there because the relevant perceptual processes are operating properly. B's vision, by contrast, is 'clouded', or 'occluded', by some interfering factor. Given that, in the logical case, A's insight is into a 'nonphysical' realm of epistemological truth, where does this leave causality? On this view ordinary causes, of the kind dealt with by psychologists or sociologists, might help to explain why B's vision is clouded; and they might explain how A came to be in a position to see the truth (e.g., how education, training, intelligence, etc., opened the way for an unimpeded view of the truth). Causation won't, however, explain the final grasp of truth itself. The rational act isn't a species of causal relationship.

Here we have *exactly* the asymmetrical, teleological picture that I have maintained all along represented the underlying model of the rationalist opposition to the sociology of knowledge. I have not been attacking implausible extremists (as alleged by Chalmers (1990), p. 83). Rather, I have been addressing a consistent argument that represents the only real alternative to the strong programme.

Mathematics and the Realm of Necessity

To show that a sociological account of mathematical knowledge was possible I argued that an alternative mathematics was conceivable. Critics have asserted: (1) that the evidence for any alternative mathematics is unconvincing, and (2) that I ignore and cannot explain the vast measure of agreement between practitioners of mathematics who are separated from one another in both space and time. See Freudenthal (1979), Triplett (1986) and Archer (1987).

Freudenthal dismisses the examples of alternative mathematics that I offered (which range from Greek mathematics to Lakatos's account of Euler's theorem). He says they have 'nothing to do with . . . [the] sociology of mathematics' (p. 74). His claim is that they deal only with the definition of concepts and not with the reasoning of the proof itself. Thus:

> While definitions are indeed the object of a community's consensus, yet they *do not fall* (and *were never taken to fall*) *within the realm of mathematical necessity* (pp. 74–75).

Negotiating about definitions is one thing: disputing the validity of proofs is another (p. 80). My failure to see this derives from insensitivity to the distinction between mathematics proper and 'metamathematics', which includes all the 'underlying philosophical presuppositions' (p. 75). Triplett independently makes the same point, and Archer endorses Freudenthal's 'detailed dissection' of my examples (p. 238).

Replies by Gellatly (1980) and Jennings (1988) effectively located the weakness in these arguments. By helping themselves to the boundary between mathematics and meta-mathematics the critics beg the question. My claim was that such a boundary is itself a convention and an historical variable. Seeing how people decide what is inside or outside mathematics is part of the problem confronting the sociology of knowledge, and the alternative ways of doing this constitute alternative conceptions of mathematics. The boundary cannot just be taken for granted in the way that the critics do. One of the reasons why there appears to be no alternative to our mathematics is because we routinely disallow it. We push the possibility aside, rendering it invisible or defining it as error or as nonmathematics. (I shall give an example in a moment.) These interpretive practices for responding to alternatives help shore up our conviction of their nonexistence. For reasons I do not pretend to understand, we seem capable of engaging in the requisite interpretive activity whilst being unaware of what we are doing. I was at pains to draw attention to these practices (cf. p. 129). What, now, have my critics done in response? They have simply *utilised* the practices I described, and then cited the results of that use against my conclusion. This is audacious, but it hardly meets the point.

Consider Wallis's way of proving that the area of a triangle is half the base times the height. He used infinitesimals and fractions with infinite numerators and denominators (cf. p. 126). We no longer accept this proof, but for Wallis it was in the realm of necessity. That is: it was a demonstration that the formula was true. In calling this a candidate 'proof' I am using the word as teachers of mathematics and practising mathematicians use the term. Freudenthal sidesteps such examples by shifting the meaning of 'proof' and using it in a special way, i.e., treating it as an abstract inference schema. Influenced by symbolic logic, this characterisation lacks the essential ingredient of mathematical thought. Lakatos has taught us that this ingredient, the proof idea, is the quasi-empirical model which motivates and organises the symbolic manipulation (1976). Wallis's proof, of course, contains a clear proof idea. This shift of meaning results in legitimate examples, such as this, being unfairly dismissed. Detecting these interpretive ploys does not, however, mean that I can dismiss the entire objection. The question is now: does the special, stripped-down sense of proof lie beyond the reach of the sociology of knowledge? I will come back to this later with a specific example.

All three critics treat the widespread agreement amongst mathematicians, and the continuities in the history of mathematics, as direct evidence against the sociology of knowledge. Such facts, it is alleged, would be miraculous if the strong programme were correct. Thus Freudenthal says that the conditions that underlie mathematical thinking 'are so pervasive as to exclude any role for a *sociological*, necessarily differential, investigation' (p. 70). The words 'necessarily differential' are crucial. Archer makes a similar inference when she says that the strong programme is 'relativist' (which is correct), and

then treats 'relative' as the opposite of 'universal' (e.g., pp. 235 and 237).

The logic of these inferences is questionable on two counts. First, the opposite of 'relative' is not 'universal': it is 'absolute'. To refute relativism the critics need more than the mere generality of opinion: they need opinion to be right. Even unanimity is no guarantee of the quality they require. As Worrall said: the code of reason must be correct. Second, in what sense is sociological enquiry 'necessarily differential'? If this means that any conventional arrangement could be different in principle, i.e., that it must be *possible* for it to be other than it is, then this is correct. But this does not mean that in practice, or empirically, a conventional arrangement must exhibit variation rather than constancy. This is again to overlook the possibility of regularity that arises for purely contingent reasons.

The difficulty, for both supporters and critics of the strong programme alike, is to know what degree of cultural variation would be expected in mathematical knowledge if a sociological analysis is correct. There are certainly some reasons to expect a measure of uniformity, and resources to explain it. These are: (1) shared reasoning propensities that are innate and common; (2) a common environment that provides the empirical models for elementary mathematical operations; and (3) the contact between cultures and the inheritance of cultural resources. On the other hand, variation would be expected in, say, responses to counterexamples and anomalies, and in the dimensions described in chapter 6. Until such time as the programme is turned into a proper theory (for an attempt, see Bloor (1978)), all that can be discussed with any certainty is the question of possibility. Is it *possible* to have the kind of variation that would be permitted on a sociological account? In particular, are such possibilities of variation to be found in the 'realm of necessity', the logical heart of a proof conceived in its most abstract and rigorous form?

As an example, consider the logical schema called *modus ponens*. This says that if you grant p, and p implies q, then you must grant q. Symbolically:

$$\begin{array}{l} p \\ \underline{p \supset q} \\ \therefore q \end{array}$$

Is there any escaping the compulsion and necessity of that? If the premises are true, doesn't the conclusion *have* to be true? That, of course, is a definition of a *valid* form of inference, and here we surely have an example of such a form that our rational faculty can directly intuit, provided our minds are not clouded. Here we seem to have a rational, or absolute, universal in the face of which the strong programme must prove powerless. How could a naturalistic and sociological approach illuminate such elements of our cognitive life?

Here is how. First, following the line taken in Barnes and Bloor (1982), I would suggest that the widespread tendency to argue in this form is because the pattern is innate. Its internal representation is not yet known, but in some form it is a feature of our natural rationality. (This suggests that it will be present in animals too, and it is.) Critics treat this step dismissively: 'They wheel in biology', says Archer (p. 241). From a naturalistic standpoint, however, this is perfectly proper, but it can only be the beginnings of the story. Second comes the sociology. The line to be taken should be familiar. The generality of a pattern like *modus ponens* in our natural rationality will give it salience. When we come to erect cognitive conventions they are therefore likely to utilise such salient solutions to the problem of organising and coordinating our collective thinking. In short, it is likely to be elevated to the level of a cognitive institution. As a logical convention it will now be subject to special protection, e.g., from counterexamples and anomalies in its application.

Could there be counterexamples to a valid inference form like *modus ponens*? In fact they have been known for centuries, but they have lived a strange life on the periphery of our cultural consciousness, half known and half not-known. Logicians long ago realised that some applications of *modus ponens* will carry us from true premises to false conclusions, but they called these applications 'paradoxes'. I am referring to the 'sorites paradox', i.e., the problem of the heap. If you have a heap of sand and remove one grain, you still have a heap. So remove a grain. Now you have a heap. If you have a heap of sand, and you remove a grain . . . We have here an inference of a *modus ponens* form, but if we keep applying it the grains of sand will eventually run out, and the conclusions will be false: we won't be left with a heap, we will be left with no grains at all. The premises are true, the reasoning is *modus ponens*, and the conclusion is false. So it isn't a valid form after all. Or do we say: it *is* valid (because we can *see* its validity); therefore the blame *must* lie elsewhere and the example is a mere 'paradox', a puzzle and an oddity? The traditional response is to lay the blame on the use of 'vague' predicates, like 'heap'. Allegedly, logic only applies to clear, or well-defined concepts. Only recently has the experiment been made of taking the other path, and revising our ideas of what is happening when we use *modus ponens* (Sainsbury (1988)).

Of course there are other candidates as well as *modus ponens* that have been put forward as embodiments of absolute necessity. Archer (1987) proposes the 'law of non-contradiction', that a statement cannot be both true and false: $\sim (p \cdot \sim p)$. Again it is logicians who have provided sociologists of knowledge with the material needed to make the case for relativism. They have devised formal logical systems that violate the 'law', e.g., three-valued logics (cf. Makinson (1973)). The issue then turns on the meaning of these technical systems. The sociologist will find that various rhetorical devices are used to marginalise them. We are not told that they are paradoxes, but that they are 'parasitic' on the two-valued systems that allegedly underlie them, i.e., systems which *do* embody the law of contradictions. (This is analogous to the old arguments designed to marginalise non-Euclidean geometries: they were said to be parasitic on our unique Euclidean spatial intuition. Cf. Richards (1988).) This technique for downgrading three-valued logical systems is far from compelling. As formal systems, the three-valued and the two-valued systems are exactly on a par. The formal machinery of the first need not be seen as utilising the formal machinery of the second. The two systems work independently and side by side. Three-valued formal systems do, however, presuppose our natural rationality, viz. our informal thought processes and mental skills for manipulating their symbols. But this is needed to underpin the formal machinery of the two-valued system as well. The innate skills may be general, and even contingently universal, but they don't endow the law of noncontradiction with any absolute status.

The predicted alternatives to these 'absolute universals' have thus been exhibited. A counter-intuitive, indeed a deeply implausible, prediction of the strong programme has thus been corroborated. Another general covering law has been put to the test and survived. The point to emerge is that the aura of the absolute that surrounds these candidates must have come from the social contrivances that constituted their special status. When we feel their compelling and obligatory character it is cultural tradition and convention to which we are responding. The 'realm of necessity', therefore, turns out to be the social realm.

Conclusion: Science and Heresy

Not long ago I discovered to my surprise that the arguments I have just surveyed—my own included—are just a replay of a controversy that took place over a century ago (Bloor, (1988)). The debate over the strong programme has all been gone through before in another context, namely theology and the history of religious dogma. When I argued in chapter 3 that we protect science from sociological scrutiny by treating it as sacred, I spoke more truly than I knew. The strong programme first emerged in connection with sacred rather than scientific beliefs, and the arguments used against it then were exactly those used now. Today we debate the proper way to write the history of science; yesterday it was the proper way to write the history of church dogma, but all of us would have been completely at home in that argument.

The strong programme is the analogue of the position that was associated with the so-called Tübingen school of church historiography. Under the leadership of Ferdinand Christian Baur (1792–1860) these scholars set about ruthlessly applying the techniques of historical scholarship to the history of Christian doctrines. They rejected the old paradigm of church history that Baur called 'supernaturalism'. As Baur explained in his 'Epochs of Church Historiography' (1852) (see Hodgson (1968), p. 53), 'supernaturalists' divide the history of dogma into two parts which are treated in different ways. One part is the record of authentically apostolic truth. This flows from divine sources, and needs no other explanation beyond its divinity. The other part is the record of heresy and doctrinal deviation. This is to be accounted for by everything that can cloud the vision of the faithful and lead them astray. Here explanation is in terms of ambition, greed, ignorance, superstition and evil. We are fallen creatures, and this explains deviations from the path of true dogmatic development.

Clearly, the assumptions behind 'supernaturalism' are identical to those that inform the historiography of today's rationalists when they reflect on science. In the place of the historical unfolding of divine inspiration we have the unfolding of rational enquiry, the 'internal' history of science. In the place of heresy we have irrationality and the socio-psychologically caused deviations from the true scientific method, the 'external' history of science. Doctrinal error in theology has given way to ideological bias in science. Today's rationalists say:

> *When a thinker does what it is rational to do, we need enquire no further into the causes of his action;* whereas, when he does what is in fact irrational—even if he believes it to be rational—we require some further explanation (Laudan (1977), pp. 188–89).

The position of yesterday's supernaturalist can be precisely characterised by using these same words and making a few substitutions. Thus:

> When a Christian believes what is orthodox, we need enquire no further into the causes of his belief, whereas when he believes what is in fact heretical—even if he believes it to be orthodox—we require some further explanation.

Baur replaced this venerable, but stultifying, vision by a study of the political conflicts and negotiations between the competing parties of the early church. He analysed doctrines in terms of their 'tendencies', i.e., the interests that informed them, and refused to structure his enquiries around a prior doctrinal judgment as to which of these tendencies was theologically correct. In short, he studied the social construction of our most cherished dogmas, and he did so as a pious and respectful believer (Hodgson, 1966).

Baur and the Tübingen school were true pioneers in the sociology of knowledge. How sad that their great achievements did not feed into the common consciousness of philosophers, sociologists and historians of science, so that the same debate has had to be repeated. Let us also devoutly hope that these historical parallels go no further. Baur and his colleagues ultimately failed in their efforts to modify the way that members of the theological tradition reflected historically on their own beliefs and practices. Why pay all this attention to theological disputes? asked their critics. Don't disputes come to an end, and doesn't that prove that the reality of the godhead and the truth of church dogma always finally asserts itself (e.g., Matheson (1875))? Despite their detailed and extensive enquiries, and the wealth of evidence they produced, the Tübingen school were merely seen as denigrating what they studied. Ultimately their influence was crushed under the weight of obscurantism, bigotry and a reactionary theology abetted by authoritarian government.

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Knowledge and Social Imagery

Second Edition

David Bloor

The University of Chicago Press
Chicago and London

The University of Chicago Press, Chicago 60637
The University of Chicago Press, Ltd., London

Printed in the United States of America
99 98 97 96 95 94 6 5 4 3 2

ISBN 0-226-06096-9 (cloth)
ISBN 0-226-06097-7 (pbk.)

Library of Congress Cataloging-in-Publication Data

Bloor, David
Knowledge and social imagery / David Bloor. —
2nd ed.
p. cm.
Includes bibliographical references and index.
1. Knowledge, Sociology of 2. Science—
Philosophy. 3 Mathematics—Philosophy.
I. Title.
BD175.B57 1991 91-9141
306.4'2—dc20 CIP

♾ The paper used in this publication meets the
minimum requirements of the American National
Standard for Information Sciences—Permanence of
Paper for Printed Library Materials, ANSI
Z39.48-1984

For Max Bloor

Contents

Preface to the Second Edition (1991)

The second edition of 'Knowledge and Social Imagery' has two parts: the text of the first edition plus a new and substantial Afterword in which I reply to critics. I have resisted the temptation to alter the original presentation of the case for the sociology of knowledge, though I have taken the opportunity to correct minor mistakes such as spelling errors. I have also made a few stylistic alterations where the language of the book has become dated. Otherwise the first part is unchanged. As for the second part: attacks by critics have not convinced me of the need to give ground on any matter of substance. Indeed, their failure to make inroads has reinforced my belief in the value of a naturalistic understanding of knowledge in which sociology plays a central role. I hope that the arguments I offer in the Afterword show this to be a reasoned and justified response. Because of the volume of the criticism I could not allow myself to follow every twist and turn in the argument. I have therefore restricted the discussion to essentials, and avoided repeating replies that I have given elsewhere. Nevertheless, the topics covered in the Afterword represent the main areas of dispute in the field. The only exception is that I have left aside the standard objection that a relativist sociology of knowledge is self-refuting. This is discussed in the main body of the text, and the further points that need making seem to me to be cogently expressed in Hesse (1980).

If I were beginning the book today, I would be able to call on a substantially larger body of empirical work in the historical sociology of knowledge. The main proof of the *possibility* of the sociology of knowledge is its *actuality*. Shapin's admirable bibliographical essay, History of Science and Its Sociological Reconstructions (1982), has