Cetina and M. Mulkay (eds.), *Science Observed: perspectives on the social study of science*, Sage, London (1983), pp. 239–266; S. Woolgar and D. Pawluch, 'Ontological Gerrymandering: the anatomy of social problems explanations', *Social Problems*, **32** (1985), pp. 214–227.

3

Opening the Black Box: Logic, Reason and Rules

Thus far we have discussed the wide variation in conceptions of science (Chapter 1) and noted the importance of the idea of representation (Chapter 2) which, we suggested, is deeply implicated in all efforts to be 'scientific'. In this chapter we start to look at how sociologists (in particular, but also some historians and philosophers) have attempted to escape the constraints upon our understanding of science exerted by the dominance of essentialism. As a specific example we examine attempts by sociologists to study logic or reasoning, especially that said to be involved in promoting scientific and mathematical laws and truths.

One of the major consequences of sociologies of science which adopted the received view was their neglect of the nature and structure of scientific knowledge. In general, these sociologies operated by treating scientific knowledge — the theories, formulae, physical laws, mathematical equations and proofs — as a black box. For sociological purposes it was assumed that nothing was to be gained by opening the box and scrutinizing its contents; the social origins of scientific knowledge were regarded as simply not relevant to its content. Some sociologists even went so far as to suggest that attention to content detracts from the proper task of sociological analysis. In terms of our representation couple, this attitude treated the relationship between 'scientific knowledge' and 'the objective, natural world' as a black box. The nature of the relationship was considered beyond the realm of sociological inquiry: it was neither necessary nor desirable to consider how they are conjoined.

A particular feature of this viewpoint was that the adequacy of the connection, whether or not scientific knowledge is an accurate representation of the world, was regarded as a question of methodology rather than sociology. Thus Merton, when introducing his famous discussion of the normative ethos in science: 'To be sure, methodological canons are often both technical expedients and moral complusives, but it is solely the latter which is our concern here. This is an essay in the sociology of science not an excursion into methodology.' [1]. In this usage, Merton portrays 'methodology' pejoratively, as mere methodology. Moral compulsives are said to be socially organized and structured but technical expedients (methodology) are assumed to be socially neutral. When incorrect knowledge of the world arises, the source of error is a misapplication of method, not a question for sociology. In this case, the 'distorting factors' in Fig. 3.1 (which deflect the

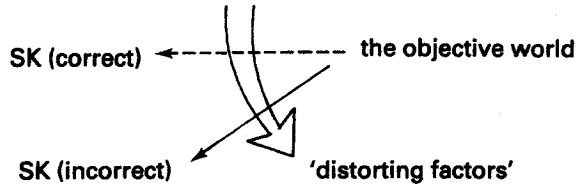

Fig. 3.1.

rightful connection between world and scientific knowledge) correspond to the misapplication of scientific method.

Within the received view, some sociologists and historians have considered methodology as a social phenomenon, but have confined their attention to occasions resulting in incorrect scientific knowledge. This approach has been called the sociology of error. Sociologists are only consulted when things go wrong, in cases of deviation from the supposedly true path between the world and knowledge about it. Their brief is then to discern the source and nature of (external) social factors which lead to a distortion of knowledge about the world. Typically, in these sociological accounts, competition for rewards (and resources) appears as the cause of a deflected connection between the world and true knowledge. This position, it should be noted, is an asymmetric sociology of scientific knowledge: social factors are relevant in cases of false or incorrect knowledge; but the sociologist has no part to play when methodological connections are effected so as to produce correct knowledge. The sociology of error investigates the production of 'incorrect' knowledge but otherwise treats the generation of scientific knowledge as a black box.

The crucial flaw in sociologies of science which adopt the received view

is their uncritical acceptance of what is said to count as 'true' and 'false' knowledge. Where knowledge was held to be correct, they saw no necessity for their involvement; where knowledge was deemed incorrect, they took this assessment as their starting point to ask what could have lead scientists to go wrong. They failed to consider that the determination (definition, assessment) of the truth status of knowledge is itself a social process. It was the major achievement of writers like Kuhn to establish the historically (and, by extension, the socially and culturally) relative character of scientific truths [2]. Consequently, the sociologist could no longer accept as given the distinction between true and false scientific beliefs. Instead, the sociological task was to discern what *counts* as true and false belief; in particular, what social processes are involved in the construction, assessment and evaluation of knowledge. It became clear that a sociological understanding of the construction of scientific knowledge requires a sophisticated appreciation of the technical content of the knowledge at issue and, preferably, a close contemporaneous investigation of scientists' technical endeavours (see Chapter 6).

The received view is consistent with sociologists' inattention to the content of scientific knowledge, an emphasis on science as a social institution and on the social relationships between knowledge producers. For this reason, the style of (traditional) sociology of science which adopted the received view was essentially a *sociology of scientists*. By contrast, more recent work emphasizes the relativity of scientific truth, calls for a sociological analysis of technical content and thereby attempts to open the black box of scientific knowledge construction. This latter style of sociology of science is more aptly dubbed a *sociology of scientific knowledge* (SSK).

In opening the black box, SSK significantly counters that aspect of the received view which sees no part for sociological investigation of the generation of scientific knowledge. But we recall (from Chapter 1) that the received view also implies a commitment to essentialism. To what extent does SSK rid us of this particular, important vestige of the traditional view of science? In order to tackle this question, we look in detail at one particular style of SSK.

THE CALL FOR A STRONG PROGRAMME IN THE SOCIOLOGY OF SCIENTIFIC KNOWLEDGE

The received view is consistent with some philosophical positions (which we shall refer to as 'rationalism') that the generation of true, correct knowledge simply does not require sociological explanation. From the perspective of rationalism, true, correct knowledge is explicable in terms of its rational merits: knowledge is truth which is believed for the right

reasons. The general acceptance of the received view was for a long time reflected in the division of labour in the study of science — between sociologists investigating social factors affecting the production of erroneous knowledge and (rationalist) philosophers looking at the rational basis for truth. Philosophers and sociologists coexisted peacefully because each addressed the question — what is science — to different phenomena.

The situation changed dramatically with the insistence of sociologists that both truth and error are equally amenable to sociological analysis. David Bloor, in particular, challenged the exclusion of sociologists from studying how 'true' scientific knowledge is produced [3]. Whether labelled as 'true' or 'false', scientific knowledge could and should be the target of sociological analysis. Bloor complained that the insistence of rationalist philosophy on the inherently (given) true or false character of knowledge was directly opposed to attempts to study the social determination of 'truth' and 'falsehood'. Rationalist philosophy assumed that genuine (proven) knowledge was not *caused* (except, perhaps, by 'rational reasons'); it was the upshot of rational method and/or logical extrapolation from existing knowledge. In this view, logic, rationality and truth are their own explanation, and 'causes' are those (external) sociological, psychological and other factors which come into play in the genesis of false or erroneous knowledge: false knowledge is caused but true knowledge is simply the upshot of rational process.

Of course, it is just this view, in its popularized form which makes the idea of a sociology of scientific knowledge seem counter-intuitive: the sociology of the family, deviance, education and so on seem eminently possible, but what 'social' factors can possibly be involved in scientific knowledge? Are sociologists going to tell us that '2 + 2 = 4' is a social construct? The widespread feeling of counter-intuition associated with SSK is itself testimony to the influence of rationalist ideas. Scientific knowledge is assumed, by definition, to be precisely that which is *not* social; knowledge is thought only to become scientific in virtue of the exclusion of social factors.

Bloor [4] formulated four key requirements for a 'strong programme' which would take sociological study beyond the rationalist view of how scientific knowledge is generated: (1) Causality. The aim of the sociology of scientific knowledge is to discern which conditions bring about beliefs or states of knowledge. Bloor noted that these conditions could be psychological, economic, political or historical as well as social. (2) Impartiality. The sociology of scientific knowledge should not select instances for study with respect to their perceived truth or falsity, rationality or irrationality, success or failure. The emphasis is on the fact that truth, falsity and so on are perceived as such. These determinations are the upshot of social

process and therefore part of the phenomenon to be studied. (3) Symmetry. Similarily, once instances of scientific knowledge have been chosen for study, the sociologist should use the same types of cause in explaining instances of scientific knowledge, whether they are classified as false or true, etc. In particular, argues Bloor, the sociologist should not invoke, say, sociological causes for 'false' beliefs but resort to, say, psychological (or worse, rationalist) causes for 'true' beliefs. (4) Reflexivity. In principle, the patterns of explanation of the sociology of scientific knowledge have to be applicable to sociology itself.

It should be clear from these tenets that mathematical statements such as '2 + 2 = 4' are as much a legitimate target of sociological questioning as any other item of knowledge (some sociologists use the term 'knowledge claim' rather than 'knowledge' to emphasize their impartiality). What kinds of historical conditions gave this expression currency and, in particular, what established (and now sustains) it as a belief? This kind of question is posed without regard for the (actual) truth status of the statement, but asks instead under what conditions it is regarded as 'true'. We are reminded, for example, that prior to the invention of mathematics, or for young children, such statements are meaningless; or again, that in the context of vector addition '2 + 2 = $\sqrt{2}$' is true. The strong programme claims not to be in any way assessing or evaluating the claimed truth status of the statement, but it is easy to see how analytic attention (of any kind, sociological or otherwise) to widely believed statements can be understood as casting aspersions on the veracity of the statement. Despite programmatic declarations of impartiality, the advocates of the strong programme are unavoidably embroiled in an agonistic discourse.

DEBATE OVER THE STRONG PROGRAMME

Controversy over the strong programme in the sociology of scientific knowledge has been heated: much vitriol and flying fur has accompanied exchanges between supporters of the strong programme and rationalist philosophers. The mood is captured by Bloor's infamous remark that 'to ask questions of the sort which philosophers address to themselves is to paralyse the mind' [5]. One possible (albeit *weak*) explanation for the intensity of the controversy emphasizes the importance of the entrenched division of labour between sociologists and philosophers of science. The call for a strong programme transgressed this division of labour by suggesting that the very content of scientific knowledge is amenable to sociological analysis; rationalist philosophy was outraged at the invasion of territory previously their exclusive domain.

Although the debate is about the best ways of finding out about the

nature of science, its most curious feature is that participants themselves draw upon preconceived ideas about science in order to criticize or defend aspects of the proposed (strong) programme.

Bloor says that the principles of the strong programme 'embody the values which are taken for granted *in other scientific disciplines*' [6]. The strong programme, he says, 'possesses a certain kind of moral neutrality, namely the same kind as we have learned to associate with all *other sciences*' [7] and denial of its tenets would be a betrayal '. . . of the approach of empirical science' [8]. 'If knowledge could not be applied in a thorough-going way to scientific knowledge it would mean that science could not scientifically know itself' [9]. 'In a very orthodox way I have said: only proceed as the other sciences proceed and all will be well' [10]. 'If we want an account of the nature of scientific knowledge, surely, we can do no better than adopt the scientific method itself' [11].

Larry Laudan, Bloor's chief critic, is quick to spot the apparent circularity involved here [12]. He points out that Bloor seems to be assuming the very answer he is setting out to discover: Bloor's characterization of, and justification for, his tenets puts the cart before the horse. But Laudan also deploys a version of 'what science is like' in his own attempt to discredit Bloor's claims. He thus argues that not all science is causal; that impartiality occurs in no science of which he is aware; and that the reflexivity tenet is redundant if the objective is a generally applicable theory [13]. Laudan is especially troubled by the symmetry postulate because it runs against *what he regards as the best established precedents in the natural sciences* namely, that scientists invoke different causal processes to explain different phenomena. It would be absurd, suggests Laudan, to try and explain both gravitational and electrical phenomena using the same kind of cause. Of course, it is possible that Laudan has misread Bloor on this point. Bloor does not insist on the same cause for different phenomena, but on the same *kind of* cause for both true and false instances of the same phenomenon. A better analogy is not with the unicausal explanation of gravitational and electrical phenomenon, but with social interests giving rise to both N-rays and X-rays. Specifically different social interests might have operated in each instance, but social interests would nonetheless account for an instance of (perceivedly) true scientific knowledge as well as a false one. Bloor wishes to avoid the kind of situation where social interests are used to account for N-rays but rationality and logic are used to account for the emergence of X-rays.

Even at the heart of disagreements about how to find out about science, we find the protagonists utilizing versions of what science is (actually) like. Taken together, their arguments display the kind of variation in preconceptions about science which we found in Chapter 1. It is as if the protagonists

are locked into a discourse which *forces* their use and invocation of one or another preconception about science. This observation further supports the argument that 'science' is best treated, not as a discoverable entity, but as a discursive resource. More importantly, the fact that the arguments of even these analysts have recourse to unexplicated notions of science raises the possibility that we are dealing with a concept which is deeply implicated in practices of argument.

RULES AND LOGIC

The call for a sociology of scientific knowledge attracted a lot of attention, not just because it proposed the sociological analysis of previously philosophical matters — the content and nature of scientific knowledge — but more significantly, because it emphasized the relativity of scientific truth. The implications were that scientific knowledge could no longer be assumed to be straightforwardly 'rational', that the application of 'reason' was no longer any guarantee of 'truth' and so on. In fact, this kind of relativity was no more than a particular case of a more widespread intellectual movement. In particular, SSK shows a marked affinity with a key notion in post-Wittgensteinian thought: scepticism about the view that practice (action, behaviour) can be understood in terms of following rules (guidelines, principles). In order to elaborate this point let us consider the rudiments of the sociology of scientific knowledge position on the nature of rules and logic.

In a famous parable by Lewis Carroll (used subsequently by Winch), Achilles and the tortoise are discussing three propositions — A, B and Z — which are so related, according to Achilles, that Z is claimed to 'follow logically' from A and B [14]. The tortoise agrees to accept A and B as true but asks what would persuade him to accept Z if he does not yet accept the hypothetical proposition C: If A and B are true, Z must be true. Achilles begins by asking the tortoise to accept C, which he does. He then says to the tortoise: 'If you accept A, B and C, you must accept Z'. When the tortoise asks why he must, Achilles says 'it is because it follows logically from them. If A, B and C are true, Z must be true. You don't dispute that I imagine?' The tortoise agrees to accept this last proposition and to call it D.

> 'Now that you accept A and B and C and D, of *course* you accept Z.
> 'Do I?' said the tortoise innocently. 'Let's make that quite clear. I accept A and B and C and D. Suppose I still refuse to accept Z.'
> 'Then logic would take you by the throat and *force* you to do it'

> Achilles triumphantly replied. 'Logic would tell you "You can't help yourself. Now that you have accepted A and B and C and D, you must accept Z". So you've no choice you see.'
> 'Whatever logic is good enough to tell me is worth *writing down*', said the tortoise. So enter it in your book, please. We will call it E (If A and B and C and D are true, Z must be true). Until I have granted *that*, of course, I needn't grant Z. So it's quite a necessary step, you see?'
> 'I see,' said Achilles; and there was a touch of sadness in his tone. [15]

In Lewis Carroll's version, the story ends some months later when the narrator returns to find the couple still discussing the point, with the notebook nearly full. (A more realistic version would depict Achilles as altogether less patient: the story would end in the culmination of Achilles' frustration and his dismissal of the tortoise, certainly no later than about proposition G.)

The moral of the tale is that in principle there is nothing in the logic itself to guarantee the acceptance of a proposition or position. Rules and reasons do not themselves determine the position adopted by parties to an argument. This is so, in particular, because any justification of a particular logical connection is itself susceptible to justification. The search for final or absolute justification is therefore endless in principle. (The methodological horror which applies here is inconcludability — see Chapter 2). In practice, however, participants say that enough is enough, thereby appealing to one another's sense of 'what we could all reasonably expect to be the case'. Logic compels, it has been said, by the sanctions of our fellow men.

This view of logic supplants the notion of reason as a determinant of action [16]. Reason and logic are in principle insufficient to compel a particular course of action. (Action, like knowledge, is underdetermined by rules, logic and reason (observations).) Instead, logic and reason are key features of a discourse which is used to evaluate and characterize action. Action comes first, logic second, although this is not just a matter of temporal sequence. Rather we are talking of logic as an antecedent in the fullest sense. Its invocation as an antecedent is inevitably *post hoc* in that actions are envisaged and their grounds only subsequently grafted onto them. Now, of course, I may decide upon a course of action in virtue of my contemplation about 'whether or not it is reasonable'; I could say that I worked out 'what it would be logical to do'; that it 'made sense' to follow one path rather than another. But in all these cases, the envisaged action and the consequent actions are also antecedent to subsequent rationalization. In such cases, the logical schema are imposed upon actions which are imagined (in the light of past experience or whatever). The logic of the situation does not exist outside of descriptions and assessments of the action itself. Logic can not simply 'give rise to' actions.

One classic form of logical reasoning is the syllogism, an example of which is:

(1) All politicians are liars
(2) Mrs Thatcher is a politician
(3) Mrs Thatcher is a liar

The syllogism is such that the conclusion (statement (3)) is said to follow from two premises (statements (1) and (2)). In a manner analogous to Boolean algebra, the first statement defines a category (liars) within which a subcategory (politicians) is contained; the second statement identifies a member of this subcategory, which is also a member of the larger initial category. The difficulty is not that the reasoning is 'wrong', but that the implied necessity of logical deduction is superfluous [17]. We do not need to follow the logical steps of the syllogism to reach the conclusion, for we already 'know' that Mrs Thatcher is a liar *as part of* knowing that all politicians are liars. The deductive form of reasoning required by the syllogism is unnecessary for such knowledge. Its status appears to be a *post hoc* formalization (and hence justification) of something we were prepared to act upon anyway. Once again, we see that logic is subsequent to, rather than an antecedent of, the practical business of knowing something.

But supposing the example was modified. Suppose that although fully conversant with statement (1), we knew nothing about a Mr Bloggs. It was *only subsequently* revealed to us that Mr Bloggs is a politician. Would we not then say that the syllogism enabled us to conclude something we had not previously known, namely that Mr Bloggs is a liar? Surely, it might be claimed, logic helps us deduce something in this situation.

The example is important because it reveals a common confusion between logical and temporal links. In order to make the syllogism work for Mr Bloggs, we have in effect to extend the application of the first statement. For the generality (truth) of the first statement is contingent upon any further instances similarily fitting the mould. Not knowing anything about Mr Bloggs in advance, we might consider he was either a politician who lied or a politician who did not lie. The latter case would invalidate the initial premise of the syllogism. The former case would again make its 'application' trivial since we would not need the syllogism to tell us the terrible truth about Mr Bloggs.

Bloor also relates the argument that legal decisions and judgements can not and should not rest upon logical deduction from rules [18]. This is the way to real trouble. Instead, the decision should be made, and perhaps only later should the justification be retrospectively constructed. A judge in a recent public inquiry is reported to have similarly removed himself from the field of logical contention. Faced with the directly opposing claims of eminent scientists about the environmental impact of a nuclear power development, he sidestepped the truth of the matter: 'I may be right and I may be wrong, but I am never uncertain — I hereby find for the claimant'. The practical resolution of apparent deadlock was achieved by redefining the relevant decision criteria. Logic and truth were left behind in favour of considerations of decisiveness.

We have thus far exploded two myths about logic: the tortoise showed us that logic does not determine a particular course of action (practice, deduction, knowledge); our look at the syllogism suggests that logic is superfluous to a practical course of action. Together these lines of argument reinforce the notion that logic and reasoning have a function quite different from that normally attributed to them. Far from compelling particular courses of action, they form the *post hoc* rationalization for ordered practices and conventional ways of proceeding. Forms of logic, rationality and reason are then formal statements which reflect our acceptance of institutionalized practices and procedures. They are the vocabulary through and within which we reassert the primacy of consensual practice and institution.

REFLEXIVITY AND FEEDBACKING

The last tenet of the strong programme suggests that a form of reflexivity is necessary because otherwise sociology would be a standing refutation of its own theories. In other words, the supposition that sociology is immune from sociological analysis would be to suggest that it has achieved a form which places it above the knowledge enterprises it seeks to explain. It would thus constitute a special case, the very thing outlawed by the quest for a thorough-going sociology of knowledge. To make an exception of the sociology of scientific knowledge would be to curtail the general validity of its argument and we would then be back in the situation referred to (in Chapter 1) as Mannheim's Mistake: the explicit exemption of a particular kind of knowledge (in his case, mathematics and natural sciences) from the purview of sociological analysis.

An interesting question arises in respect of the possible outcome of this debate. What will mark the end of the Bloor–Laudan debate? A definitive answer would of course provide a test of their respective theories. The

rationalists' answer seems straightforward: the inherent logic and rationality of their argument will ensure the correctness of their position. Further, the error of the sociologists is easily explained by their inattention to matters of logic and rationality, an oversight brought on, perhaps, by their (illegitimate) desire to extend the empire of their professional expertise. The strong programmers' answer is less clear-cut. For, to be consistent, they would need to admit that factors other than the intrinsic merit of their case will bring about a resolution [19]. They would grant that the competing interests of rationalists and themselves are at play, but would not easily be able to predict an outcome.

The inability of the strong programmers to predict the outcome is consistent with sociological scepticism about the idea of a 'definitive outcome'. We might all agree that a time will (has?) come when talk about and concern over the debate has ceased. At that point, the same mechanisms for rewriting the history of this episode will come into play as operate in the history of natural science. It will be possible, in principle, to portray the outcome as consistent with a victory for either the strong programme *or* the rationalist position. Whether Bloor or Laudan is correct is not some matter inherent to the argument, just awaiting the discovery of the necessary hidden manuscript. It is instead a matter of public perception in the light of concurrent and competing views and positions. Truth or falsity is perceived (and achieved) rather than inherent.

What then is the status of the strong programme in relation to sociological practice? Each of the four tenets which make up the strong programme take the form of a methodological injunction for the sociologist: the sociologist *should* be impartial and so on. But what is the relationship between such pronouncements and practice. If we carry through the scepticism about logic and reasoning to our consideration of rules, we see that these kinds of injunction do not *guide* practice, but merely provide *post hoc* justifications for the conventionally binding character of certain forms of practice. In what sense can we then suppose that the enunciation and elaboration of these tenets will generate the kind of SSK which supporters of the strong programme favour? Rules do not determine social action. Why then should these tenets lead to a certain kind of sociological research? The impressive analysis of logic produced by writers like Bloor suggests we understand these tenets as *post hoc* justifications of sociological research. They constitute, in other words, a resource for the characterization and evaluation of research practice.

We noted in Chapter 1 that the normative ethos of science was unsatisfactory in so far as it gave no good account of the generation of scientific knowledge. Indeed, it has been argued that the deliberate *transgression* of norms has lead to the generation of certifiable, valued new

scientific knowledge [12]. We also noted that the philosophical quest for decision rules ran into trouble once it was recognized that 'true' knowledge could result from the deliberate disregard for what was perceived as the rational course of action. The policy implications of this line of argument are intriguing to say the least Feyerabend's (in)famous recommendation for science is 'anything goes' — that the specification of rules for rational procedure is counter-productive [21]. Does the same apply to the strong programme? Our 'reflexive' consideration of the status of Bloor's methodological injunctions suggests a further set of 'anti-guidelines' for conduct. Can we conclude that the health of SSK depends upon our deliberately contravening its tenets?

CONCLUSION

A central achievement of the sociology of scientific knowledge is its scepticism about the role of logic and reason especially in mathematics and science. This is elided with and derived from the scepticism about rule following of the later Wittgenstein.

SSK has established that the esoteric details of scientific activity (the process whereby knowledge about the world is produced, the work of connecting the left- and right-hand sides of our representation couple) is an appropriate focus of sociological interest. In particular, this chapter has outlined the argument for a first key inversion with respect to science. By looking at reason and logic, we find that reason, logic and rules are *post hoc* rationalizations of scientific and mathematical practices, not their determining force. Logic does not give rise to a particular deduction or proof but instead justifies the conventionally accepted manouevres which count as that proof. The implication for social science and more generally for all attempts to explain and account for phenomena — whether human, animal, mechanistic or inanimate, etc. (see Chapter 7) — is that we are not governed by logic, nor by rules nor reasons.

But at this point a crucial set of alternatives opens up. We could either abandon the attempt to explain science by logic (rules and reasons) in favour of some other expanans *or* we could abandon the attempt to explain science in this way at all. Advocates of the strong programme seem to come close to suggesting that we understand scientific activity (or, at least, the practical operation of logic) in terms of conventions. The important point is that pursuit of the strong programme means we remain committed to a particularly scientific notion — explaining — in trying to make sense of science. It is not hard to see the similarity between the explanatory format of Mertonian and strong programme accounts. 'Social interests' take the place of 'social norms', but otherwise the form of explanation is essentially

unchanged. Is it wise to persist in this explanatory mould, or should we use the occasion of increased scepticism to explore some (more) radical alternatives to explanation altogether? As a first step in the search for alternatives to explanation, we begin, in the next chapter, to apply inversion and feedbacking to other aspects of the traditional idea of science.

NOTES

[1] R. K. Merton, 'Science and Technology in a Democratic Order', *Journal of Legal and Political Sociology*, **1**, (1942), p. 116. Article subsequently published as 'Science and Democratic Social Structure' in R. K. Merton, *Social Theory and Social Structure*, and as 'The Normative Structure of Science' in R. K. Merton, *The Sociology of Science: Theoretical and Empirical Investigations*, University of Chicago Press, Chicago (1973), pp. 267–278.

[2] T. S. Kuhn, *The Structure of Scientific Revolutions*, 2nd edition, University of Chicago Press, Chicago (1970).

[3] D. Bloor, *Knowledge and Social Imagery*, Routledge & Kegan Paul, London (1976).

[4] Ibid., Chapter 1.

[5] Ibid., p. 45.

[6] Ibid., p. 4.

[7] Ibid., p. 10.

[8] Ibid., p. 10.

[9] Ibid., p. 40.

[10] Ibid., p. 141.

[11] Ibid., p. ix.

[12] L. Laudan, 'The Pseudo-Science of Science?', *Philosophy of the Social Sciences*, **11**, (1981), pp. 173–198.

[13] Ibid.

[14] L. Carroll, 'What the Tortoise Said to Achilles' in L. Carroll, *Complete Works*, Nonesuch Press, cited in P. Winch, *The Idea of a Social Science*, Routledge & Kegan Paul, London (1958), pp. 55 ff. For a related demonstration of the conventional base for rule following, see the example (adapted from Wittgenstein and Winch) in H. M. Collins, *Changing Order: Replication and Induction in Scientific Practice*, Sage, London (1985), pp. 12–16.

[15] Winch, op. cit (note 14), p.56.

[16] David Bloor shows how this argument applies as much to mathematical logic as to the logic supporting the Azande oracle. Bloor, op. cit. (note 3), Chapters 6 and 7.

[17] Ibid., p. 117 ff.
[18] Ibid., p. 118.
[19] Actually, sophisticated strong programmers would object to this formulation since it implies that 'intrinsic merit' is separable/distinct from 'other factors'. They may wish to make the point that such 'factors' *constitute* value, truth, merit and the rest.
[20] M. J. Mulkay, *The Social Process of Innovation*, Macmillan, London (1972).
[21] P. K. Feyerabend, *Against Method*, New Left Books, London (1975).

4

Inverting Nature: Discovery and Facts

Logic and reason are just one focus of the challenge to the 'received view' of science [1]. In the last chapter, we looked in particular at the way the strong programme in SSK took issue with the view that scientific knowledge was generated as a result of rational (reasonable, logical) extrapolation from either (or both) existing knowledge or observations of the world. In terms of our representation couple, critical attention was directed towards the *character* of the link between right-hand side and left-hand side:

knowledge ——————— the world
new knowledge ——————— old knowledge

The strong programme argued that the creation of these links could not be assumed to be the upshot of a rational process, if that meant their exclusion from sociological investigation. It was argued that the sociological perspective enabled us to understand rules, reason and logic as social conventions for accomplishing and creating these links.

We thus see that sociologists have unequivocably rejected the assumption in 'the received view' that the production of knowledge about the world, that is, the establishment of connections between right- and left-hand sides of our couple, is not amenable to sociological study. However, many seem uncertain about taking issue with a further key assumption, that the world exists independently of, and prior to, knowledge produced about it. Although they are clear about the need for a sociological position on the