Is Epistemology Redundant? A Sociological View

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INTRODUCTION

This paper advocates the strong programme of the sociology of knowledge, and seeks to defend that programme against attacks made by philosophers of science. It concentrates in particular on objections raised by Popperians to such a programme. The following points will be argued: contemporary philosophy of science is in the main committed to the elucidation of the 'logic' of science. The 'logic' is that which is held to produce the best possible knowledge. Philosophers radically disagree amongst themselves about the nature of such a logic, but usually agree that it is important that it should be established. Most of them concur that its elucidation depends in part on the empirical study of science, whether historical or sociological. However, they are all agreed that a distinction in principle must be made between philosophy and such empirical disciplines. The distinction is based on the nature of the logic, which is in turn tied up with the philosopher's disciplinary self-identification: the logic of science is both *conceptual* (rather than, say, social), and it is *normative.* The role of the philosopher is that of judge, and this commitment is non empirical.

Sociologists vary widely in their attitudes towards the evaluative goal of philosopher and the various logics proposed. Some appear, at least by implication, to accept the subordinate position permitted them in the philosopher's scheme of intellectual activities. By keeping their noses out of epistemological issues a *modus vivendi* is possible in which sociology concentrates on the study of scientific irrationality and error. However, others, perhaps encouraged by their reading of Kuhn, adopt a relativist position in which the possibility of non context-dependent criteria of demarcation is denied, and clearly trespass, whether deliberately or not, into areas previously reserved for philosophers. This paper, arguing from the radically relativist position of the strong programme of the sociology of knowledge, catalogues the most important objections raised by the Popperians to such a position, and then seeks to answer them. Little here is novel—the debates have been played and replayed in the recent philosophical and sociological literature—and it seems unlikely that their further rehearsal will affect the fundamental issues. However, the underlying aim of this paper is to suggest that advocates of the strong programme of the sociology of knowledge should develop greater self-confidence in the pursuit of their own goals. That, correspondingly, epistemological issues as conceived by evaluative philosophers of science are unhelpful to such a programme, and that it in no way enhances the analysis of esoteric or alien belief systems to distinguish between scientific procedures as they *ought* to be, and those procedures. as they actually (so far as we can tell) are.'

THE POPPERIAN PROGRAMME

No attempt will here be made to summarize the full detail of the Popperian programme. This discussion will rather be used to bring out certain features in that programme which are important for later discussion.[2]

Popper's general aim is clear—it is to develop a rational and evaluative theory of science which will, at one and the same time, protect science against irrational and prejudiced attacks, distinguish it finally from pseudo-science, and maximize its effectiveness in ensuring the rapid growth of knowledge. The criterion of demarcation, and the analysis of those procedures which will most effectively further these goals hinges on the rejection of induction, and the adoption, in its place, of falsifiability.

In the Popperian view scientific method is distinctive in certain important respects. The scientist aims to produce theoretical conjectures that are falsifiable—which forbid certain empirical states of affairs. The scientist is then enjoined to try to falsify the theoretical conjectures with empirical findings. When he succeeds, this should be viewed as a success, and he should then go on to propose a hypothesis of greater empirical content, which should in its turn be falsifiable, and ultimately falsified.

In this way Popper is able to explain the growth of knowledge, to institutionalize his adage that we learn by our mistakes, and to distinguish between the pseudo-sciences (such as marxism or psychoanalysis) which adjust their theories to fit any empirical states of affairs by means of `adhocness', and the true sciences which accept a counter-instance as a definitive refutation.

It is important to be clear that Popper's aim is not neutral. He is not proposing a description of all science as it actually is, but rather an analysis of the structure that he holds to underlie the best of science. He is a 'revolutionary conventionalist', accepting that science rests upon methodological conventions, but arguing that advance depends on the selection of *appropriate* conventions. He therefore rejects those conventionalist strategies which would save theories from falsification, and proposes those which would tend to undermine theory. Thus, he writes of his methodological rules:

> First a supreme rule is laid down which serves as a kind of norm for deciding upon the remaining rules, and which is thus a rule of a higher type. It is the rule which says that the other rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification (Popper, 2959, p. 54).

Popper is not a crude empiricist, however. This being the case he is obliged to make certain decisions concerning the nature of the falsifying empirical base. Specifically, decisions are made which (1) distinguish between low-level empirical statements and higher-level theoretical conjectures, and (2) determine, given the unavoidably conjectural nature of the former, when such statements are to be regarded as having been established. The second decision is social in nature. Empirical statements are accepted (provisionally) by intersubjective agreement between competent scientists. The effect must, in other words, be reproducible, both proposed and corroborated (Popper, 2959, p. 86) within the scientific community:

Any empirical scientific statement can be presented ... in such a way that anyone who has learned the relevant technique can test it (Popper, 1959, p. 99).

Popper thus admits that these basic observation statements have the status of social dogmas (Popper, 1959, p. 105) but insists that they are innocuous because they *could* be further tested, and they are not in any case being used to *prove* high level statements.

It is important to be clear of the absolute distinction of principle that Popper makes between psychological analysis (the study of the forces that motivate actors to belief) and epistemology. The latter is concerned with what *in principle* may be asserted, and is seen as being logically prior to psychology. Empirical psychological studies have no bearing on epistemological issues. Thus, Popper writes of the empirical base:

> I admit . . . that the decision to accept a basic statement, and to be satisfied with it, is causally connected with our experiences—especially with our *perceptual experiences.* But we do not attempt to *justify* basic statements by these experiences. Experiences can *motivate a decision,* and hence an acceptance or a rejection of a statement, but a basic statement cannot be *justified* by them—no more than by thumping the table (Popper, 1959, p. 105).

This doctrine has been reformulated more recently in the epistemological (and ontological) distinction between the second and third worlds (the physical world being the first):

> . . . *scientific knowledge* simply is not knowledge in the sense of the ordinary usage of the words 'I know'. While knowledge in the sense of 'I know' belongs to what I call the 'second world', the world of *subjects,* scientific knowledge belongs to the third world, to the world of objective theories, objective problems, and objective arguments (Popper, 5972, p. 108).

The third world of knowledge 'in itself', of knowledge 'without a knowing subject', constitutes the realm of study of the epistemologist—of what can or cannot in principle be asserted. The third world is indeed created by men, but once created it is largely autonomous. It possesses its own structure, its interrelations can be studied, and its growth assessed. It feeds back into the second world, the world of beliefs and dispositions, and indirectly, via the actions of men, affects the first world of material objects. Popper suggests that traditional epistemology has insufficiently distinguished between the second and third worlds, confusing belief with matters of principle. He asserts that, not only is an understanding of the second world irrelevant to an understanding of the third, but, a stronger thesis, that the study of the third is essential to the proper understanding of the second:

> ... scientists act on the basis of a guess or, if you like, of a *subjective belief . . .* concerning what is promising of impending *growth in the third world of objective knowledge* (Popper, 1972, p. ass).

The notion of situational logic (Popper, 1972, p. 579) further reflects the distinction between belief and knowledge in the objective sense. Situational logic is used in historical study, to distinguish between the objective problem-situation surrounding the actor, and his perception of that problem-situation. In this way it is possible to determine whether or not an actor has a correct appreciation of the problem-situation, and thereby to explain the failure of his actions if, for example, he has not.

The general outline of Popper's theory of knowledge is thus clear. It is objective, stressing the irrelevance of 'belief' to issues of epistemological or logical principle. Knowledge is seen as existing in the third world, and objective relations exist between different parts of that knowledge (even though no man may be aware of it). Growth of objective knowledge occurs when men advance falsifiable conjectures of greater empirical scope. Although these *activities* take' place in the second world, it is in terms of the third world that we must conceive and judge them. Indeed, there can be no *logic* of the development of conjectures at all.

The basis of Popper's fundamental distinction between epistemology and the empirical sciences of psychology and sociology is thus clear. But despite the fact that science actually depends on social conventions for its development, Popper has very little time for sociology. The source of this hostility lies in his attack on historicism, an attack which will not be developed in any detail here. He opposes the sociology of knowledge because it links beliefs to social positions and leads its adherents to study these links and *assume* the invalidity of rival beliefs. It acts, thus, as a bar to the rational discussion of differences (Popper, 1945, p. 215). His objection to the sociology of knowledge is thus generally consistent with his commitment to rational discourse. He makes a second and much more specific objection to the sociology of knowledge which, in the light of recent work in the sociology of science, is no longer tenable. He suggests that the sociology of knowledge

> exhibits an astounding failure to understand precisely its main subject, the *social aspects of knowledge,* or rather, of scientific method. It looks upon science or knowledge as a process in the mind or 'consciousness' of the individual scientist, or perhaps as the product of such a process (Popper, 1945, pp. 216-7).

Popper thus criticizes the sociology of knowledge for what amounts to psychological reductionism.

OBJECTIONS TO SOCIOLOGY

For Popper, Lakatos[3] and many other writers in this tradition, the rational pursuit of science is a tentative process which is always in danger of being undermined. The process of criticism, of listening to reason, the abandonment of mistaken beliefs, is one that is frail and always under attack by the forces of dogmatism and authoritarianism. In the early days Popper was mainly preoccupied with the failure of inductivism, for in his view its failure opened science to the dangers of an irrationalist or subjective analysis. His criticisms a little later of historicist doctrines, whether Marxist or Nazi, grew from the same concern—to preserve the role of 'reason' in public affairs. We have seen briefly above that this underlay his opposition to the sociology of knowledge.

More recently, however, pressure has been put on this view from another quarter entirely—from the presuppositionalist analysis of the history of science exemplified most strongly by the writings of T. S. Kuhn (1970). Thus Lakatos writes:

> For Popper scientific change is rational or at least rationally reconstructible and falls in the realm of the logic of discovery. For Kuhn scientific change—from one 'paradigm' to another—is a mystical conversion which is not and cannot be governed by rules of reason and which falls totally within the realm of the (social) psychology of discovery. Scientific change is a kind of religious change (Lakatos, 1970, p. 93).

The recent sociology of science, heavily influenced by Kuhn's writing, is clearly in immediate danger of falling foul of the epistemological standards advocated by the Popperians. At the risk of some repetition, the various criticisms of sociology may now be listed.

(1) Sociologists provide no criteria of demarcation, thereby implying that such do not exist. This lays science open to the dangers of irrationalism, and of subjectivism. Lakatos presents this view forcefully:

> If even in science there is no other way of judging a theory but by assessing the number, faith and vocal energy of its supporters, then this must be even more so in the social sciences: truth lies in power. Thus Kuhn's position would vindicate, no doubt, unintentionally, the basic political *credo* of contemporary religious maniacs ('student revolutionaries') (Lakatos, 1970, p. 93).

This is an extreme expression of a line of thought that runs right through writing in the Popperian tradition.

(2) A similar, though perhaps more interesting criticism: that sociologists do in fact lay down criteria of demarcation, but that these are of the wrong kind. Kuhn is accused (Watkins, 5970, p. 29) of seeking demarcation criteria in the rather special nature of normal science, an enterprise which most Popperians regard as bad science (if they concede its existence at all). Musgrave comes close to making a similar assertion in a review of Ziman's book *Public Knowledge:*

> The logical analysis of what an experiment can actually show about the truth or falsity of a theory is replaced by socio-psychological analysis of its persuasive power. The question of whether or not a theory could ever be established as true by experiment is replaced by the sociological question of whether it won universal acceptance (Musgrave, 1969, p. 94).

(3) Associated with the above is the suggestion that sociologists have no way of assessing scientific progress. Unless a theory of scientific rationality is upheld we must

> abandon efforts to give a rational explanation of the success of science. Scientific method . . . conceived as the discipline of rational appraisal of scientific theories—and of criteria of progress—vanishes (Lakatos, 5970, p. 115).

(4) The similar suggestion: that sociologists or historians, in treating science and non-science, or belief and knowledge together in the same way, display a cavalier and methodologically inappropriate view of the truth.

> A sociological approach always threatens to turn truth into what most experts believe. A little philosophy of science can be an effective antidote to this. (Musgrave 1969, p. 94).

Jarvie, developing this theme in an attack on Berger and Luckman, seeks to make a distinction between 'certain knowledge (current science) and mere delusive opinion (anything else)' (Jarvie, 1972, p. 537). Thus he writes:

> The very idea that science or philosophy is *a* symbolic universe, or even a way of looking at the world is a gross oversimplification. Philosophy and science can also be seen as the opposite of legitimating symbolic universes. They are rather *methods* of critically challenging and scrutinizing those symbolic universes that are on offer (Jarvie, 1972, p. 44).

(5) A further claim, similar to the above four to the point of indistinguishability: that sociologists (and indeed inductivists) confuse the *actual* judgements made by scientists with questions of epistemological *principle,* and then imagine that they are talking about principle. Popper's discussion of the third world seems designed to make this point (Popper, 1972, p. 112).

(6) There is a further point of potential dispute, one which has not surfaced in the recent literature, but which probably will in the near future. This has to do with the status of logic. In 1934 Popper wrote:

> ... not so long ago it was held that logic was a science dealing with mental processes and their laws—the laws of our thought. On this view there was no other justification to be found for logic than the alleged fact that we just could not think in any other way (Popper, [1]959, P. 9[8]).

However, Mills stressed the possibly social nature of logic (1963, p. 427), and if Wittgenstein's similar views (developed in *Remarks on the Foundation of Mathematics* and *Philosophical Investigations)* were incorporated within a strong programme of the sociology of knowledge, something which has been proposed by Bloor (1973b), then this would no doubt prove unacceptable to the Popperians.

(7) There is the attack on the sociology of knowledge for its inability to grasp the social nature of science, which was mentioned above (Popper, 1945, p. 216).

(8) There is the circularity of using logic to deny the importance of logic. Noting that Kuhn argues that dogmas rule in science for extended periods of time, rendering cross-paradigm debate very difficult, Popper writes:

> What are his main arguments? They are not psychological or historical—they are logical: Kuhn suggests that the rationality of science presupposed the acceptance of a common frame - work. He suggests that rationality *depends* upon something like a common language . . . .

This is a widely accepted and indeed a fashionable thesis: the thesis of *relativism.* And it is a *logical* thesis (Popper, 1970, p. 56).

(9) More generally there is the relativism circularity. If one adopts a sociology of knowledge position, does one, like Mannheim, or for that matter Marx, except one's own insights from the general argument that actors' views are linked in some way to their social position? If so, then on what grounds? Surely one is treading on thin ice here—for most groups are liable to believe that they have a special insight into the truth. As Popper writes (in connection with Mannheim's discussion of a freely floating intelligentzia):

> Is it not … to be expected, always assuming the truth of (the theory of total ideologies), that those who hold it will unconsciously deceive themselves by producing an amendment to the theory in order to establish the objectivity of their own views? (Popper, 1945, p. 216).

Yet if the sociologist does not make some such move, why should we consider his analysis in any case? If he only claims its validity for others who share his social position or interests, we do not have to take it seriously at all. Furthermore, according to his own views, our views may be specially justified for us!

(10) Finally, we arrive at the paradox of a theory of discovery. Poppet takes the view that

> every discovery contains 'an irrational element', or 'a creative intuition', in Bergson's sense (Popper, 1959, p. 32).

If this view is maintained, then, as Jarvie notes

> to discover a theory of discovery is to push the bus you are riding on (Jarvie, 1972, p. 136).

THE STRONG PROGRAMME OF THE SOCIOLOGY OF KNOWLEDGE

If these objections to the sociology of knowledge were sustained, then it would clearly condemn that enterprise to a rather limited future.[4] Yet a much stronger programme for the sociology of knowledge exists, with its own well defined explanatory aims. These are fourfold. First, the strong programme seeks general causes of beliefs. Secondly, it is impartial as between true beliefs, and those that are false. It does not, that is, except from analysis those beliefs that are held to be true. Specifically, and this is the third requirement, it does not except itself: beliefs held by its own practitioners are not excluded from analysis. Fourth, it makes use of the same types of explanation in its analysis of the causes of both true and false beliefs. The above features, which will be called the 'strong programme' of the sociology of knowledge, have been called the requirements of *causality, impartiality, reflexivity* and *symmetry* (Bloor, 1973b, pp. 173ff).

Such a programme obviously contrasts and conflicts with the primarily evaluative aim of the philosophers of science. Some of these conflicts have been outlined above. It was noted that the philosophers' aim is to establish a 'logic of science' which will ensure the optimum advance of learning and the rejection of error and falsehood. Yet this very evaluative aim which is so distinctively philosophical raises a number of serious problems for the philosophy of science. Consider the Popperian programme in a little more detail.

Popper asks his readers to follow him in seeking and defending conceptual criteria that will allow the growth of objective and scientific knowledge. Many of the criticisms made against sociology allege that the latter ignores the importance of such criteria. However, I want to suggest that there is a fundamentally ambiguous relationship between the Popperian enterprise and the history of science. For Popper's claims, unlike those of such writers as Frege, are empirically based. He and his followers (for example, Lakatos) are committed to the proposition that in some measure the criteria claimed are actually to be discovered in science. Good science will reflect these while bad science Chad science', as Watkins calls it) will not. Yet the relationship between such criteria and the history of science is deeply ambiguous, because while the history of science is *used,* it is not at the same time transparently *obvious* that the various criteria identified *in fact* underlie the success of science, however intuitively attractive they may sound.

The point is that once having admitted the relevance of the history of science, the inferred criteria are always open to revision. Indeed, it is consonant with Popperian methodological commitments that they should be revisable. What position, then, is to be adopted if careful study of the history of science suggests that many experiments appear to result in conformation, rather than in refutation? Or that most 'crucial' experiments can be seen to decide between two competing theories rather than simply against one? These examples are not chosen at random, for it is on these two main grounds that Lakatos revises Popper's criteria, and postulates his own alternative: that we seek to assess the growth of objective knowledge by means of rational reconstruction, that this can only be done by means of historical hindsight; that choice between different research programmes cannot be made instantly, and so on. While the commitment to a logic of science remains, it may be and has been) suggested that Lakatos's revision of Popper is so radical that it constitutes a sell-out to Kuhn (Bloor, 1971, p. 104).

The ambiguous relationship between these epistemological efforts and the history of science poses several questions. First, given that no historical findings are theory-free in any case (Lakatos, 1971) how might we choose historical accounts to decide between one or another logic of science? How do we decide when to commit ourselves to a particular set of criteria, and move from an attempted value-free historical analysis to an avowedly evaluative one? How do we know that we actually have the best criteria in our hands? (We should not forget that the `justi-ficationists' show no signs of learning from their mistakes and withering away.) Then again, how do we know that further research *within the Popperian programme* will not produce yet further radical amendments? The elucidation of such criteria is a working example of what the ethnomethodologists call the 'documentary method' (a term culled from Mannheim). The evidence at hand is taken as a 'document' of an underlying pattern (the logic of science) and is interpreted in terms of that pattern. Yet though interpretation of the document depends in this way on the assumed underlying pattern, what we take that pattern to be may be altered through the evidence of certain documents which suggest a different pattern. This is a perfectly acceptable procedure in everyday life, and indeed in empirical investigation—the ethnomethodologists suggest that we use this method all the time. But it becomes unacceptable when the current assumed pattern is elevated to a universal normative status. The question remains: why should we take *this* set of criteria more seriously than any of those that preceded it ?'

That there are so many rival criteria of demarcation constitutes in itself a major obstacle to the adoption of any particular one by the sociologist (Barnes, 1972). But there is a further serious problem—that it appears that the detailed study of historical data is in danger of emptying such criteria of effective content. Popper's original criteria of demarcation and measure of the growth of knowledge had the virtue that they were concrete—the practising scientist could urge himself and his colleagues to adopt a Popperian approach and have available some clear guidelines for action. The philosopher, in his evaluative role, was in a similar happy position. Unfortunately this is not the case for Lakatos's programme. It is only with the benefit of hindsight that the heuristic power of a programme can be determined, and even then it can never be finally determined whether a problem-shift is 'progressive' or 'degenerating' (see Lakatos, 1970). Feyerabend makes just this point:

> ... it is easy to see that standards (involving a period of hesitation) . . . have practical force only if they are combined with a time limit . . .. But introduce the time limit and the argument against naive falsificationism reappears with only a minor modification (if you are permitted to wait, why not wait a little longer?). Thus the standards which Lakatos wants to defend are either *vacuous—one* does not know when to apply them—or they can be *criticized* on grounds very similar to those which led to them in the first place (Feyerabend, 197o, p. **215).**

Feyerabend suggests that this position leaves one with two possibilities. One can either abandon the attempt to seek permanent standards of scientific progress, universally applicable, or one can

> retain such standards as a *verbal ornament, as* a memorial to happier times when it was still thought possible to run a complex and often catastrophic business like science by following a few simple and 'rational' rules (Feyerabend, 1970, p. 215).

Bloor makes a similar point. Noting that Lakatos develops his view in part to rebut the 'irrationalist' position supposedly adopted by Kuhn, he argues that the former has in fact, adopted several characteristically Kuhnian features. The core of his theories (the negative heuristic) is made irrefutable by fiat, and the *ex post facto* decision about the power of a research programme can be seen as corresponding to the fact that only an *ex post facto* decision about whether an anomaly was going to lead to scientific revolution is possible. Bloor writes that

> The close similarity between Lakatos and Kuhn should make it clear that if Lakatos is offering a rational reconstruction of science, then so is Kuhn; and if Kuhn is an irrationalist, then so is Lakatos (Bloor, 1971, p. 107).

The sceptical might be forgiven for suggesting that the Popperian tradition has, itself, all the characteristics of a degenerating research programme!

If Kuhn is accused of an 'irrational' account of paradigm change, then there are very good reasons for suggesting that the term 'rationality' is being misused. Despite the accusations of 'mob psychology' made by Lakatos, and the gross distdrtion of his position developed by Watkins (1970), (remarked upon by both Masterman and Kuhn in the same volume), Kuhn is not suggesting that there are no reasons for changing from one paradigm to another. Thus, scientists do not abandon one paradigm without adopting another; paradigms are held to overlap in certain respects despite their incommensurability; the change involves comparing them with each other and with nature. Indeed, Hesse's recent book, *The Structure of Scientific Inference,* includes an extended discussion of the situation where two 'incommensurable' systems of thought are to be compared (Hesse: 1974). There may be higher-level values (similar to those developed by Toulmin to 'solve' the alleged advocacy of irrationality by Kuhn) to which appeal may be made, and so on (Martins, 1972, p. 37; Bloor, 1971, p. 113 ; 1974a, p. 251). Kuhn writes

> To say that, in matters of theory-choice, the force of logic and observation cannot in principle be compelling is neither to discard logic and observation nor to suggest that there are not good reasons for favouring one theory over another (Kuhn, 197ob, p. 234).

Bloor gets to the heart of the difficulty, suggesting that

> At this point we really need the Popperians to make clear once and for all what they mean by terms and phrases like 'rational', 'objectively defined conditions', and 'rational reconstruction'. It looks very much as if the Popperians are simply trying to corner the market for the use of the term 'rational' (Bloor, 1971, p. 107).

That is, unless a move is held to be in conformity with the (vaguely defined) dictates of that logic of science, it can be rejected as irrational. From the standpoint of sociological or historical explanation this can be seen as a most unhelpful use of the term 'rational'—particularly as it tends to lead to denunciation rather than analysis and explanation (see Barnes, 1974, ch. 5).

So the question arises: is it possible to develop a looser use of the term, one which retains evaluative utility for the philosopher yet does not rule out much 'reasonable' scientific activity? One that allows an objective assessment of the growth of Knowledge. It is to this question that Toulmin addresses himself in the first volume of *Human* Understanding (1972). He writes of his 'ecological' approach outlined in the book that instead of leaving us wandering (as Lakatos does) in an abstract world of 'methodological research programmes' whose very names are inherited from the arguments of the formal logicians, it requires us to focus directly and in detail on the historically-developing problems and strategies with which our rational enterprises are concerned. At the same time, it gives us the means of distinguishing between the actual conceptual choices in fact made by professional scientists . . . and those which the genuine needs of their specific problem-situations would—if accurately judged—have demanded of them; so that we can acknowledge the proper roles of professional elites or 'reference groups', without running the risk (as Kuhn does) of bowing absolutely to the judgements of the currently authoritative groups (Toulmin, 1972, p. 480).

Toulmin is hankering after a systematic approach to 'situational logic', but he accepts that philosophy is presented with some very serious difficulties by the diversity of cultural enterprises, each of which has its own criteria of judgement. His middle way, which treads the delicate path between relativism and absolutism, is to develop a procedure which allows the evaluator a potentially better knowledge of the requirements for objective (but culturally bound) advance than the actors in that culture. He suggests that radical relativism, the claim that comparability and evaluation *between* cultures is not possible, is mistaken because it falls victim to the same 'cult of systemicity' that is to be found in such writers as Frege. The suggestion is that all such writers confuse rationality or reasonableness with logical moves within a closed system. In fact, what constitutes a 'reasonable' judgement is liable to vary greatly.

Toulmin employs two main analogies. The first is that of the common law, where, without a fully determined procedure, the possibility of 'even-handed' judgements is entertained. These depend on exploring similarities between different problem-situations to determine whether these are such that a judgement used in one may reasonably be transferred to another. No exhaustive procedure is possible, but reasonable men can attempt a decision as to what constitutes sufficient similarity for the purposes at hand. Toulmin argues that this analogy suggests the possibility of reasonable cross-cultural comparisons by both scientists and philosophers, so long as reason is not equated with logic.

The second analogy, with population evolution, permits an assessment of the problem-situation. A discipline is viewed as a loosely connected pool of concepts which pose certain problems for their users. The analogy suggests that we should ask how concepts are generated and selected, and this provides us with a basis for historical analysis. In addition, however, it offers us the possibility of evaluation, for with knowledge of the pool of concepts and its associated problems, we can also suggest what would have constituted the best possible development in the circumstances.

The concepts comprising a discipline (and defining its problem-situation) are of two sorts: there are relatively specific theories on the one hand, and disciplinary ideals on the other. 'Even-handedness' between theories is possible in terms of the longer-term disciplinary ideals, even though the former may be incommensurable in strict terms. However, disciplinary ideals may change too, and 'even-handedness' here depends on a set of higher-level standards (which continue to offer the analyst some grounds for assessing similarity). There are, for example, general views as to what 'science' or 'history' should aim to achieve—views which themselves are determined in the light of experience. At a higher level again comparability is made possible in the light of the experience accumulated by all men in all cultures attacking what are held to be a set of common and shared problems. Toulmin emphasizes that his own even-handed standards are at the mercy of history:

> As our experience accumulates still further, our ideas about rational strategies and procedures for dealing with the problems in any field are always open to reconsideration, revision and refinement (Toulmin, 1972, p. 500).

Toulmin, then, looks for a revisable even-handedness in seeking out the appropriate comparative solutions.

There are several important objections to this programme which have been raised by Bloor. Firstly, and perhaps least important for our present purposes, Toulmin, like Lakatos, argues that Kuhn, at least in his earlier writing, commits himself to an irrational view of paradigm-change. This is quite unacceptable, particularly in view of the fact that Toulmin saves rationality by emptying it of content (cf. Barnes, 1974, chapter 2). This, then is a second point, Bloor writes:

> Principles like 'even-handedness' are, in the abstract, merely empty formulae. As with the dictum 'love your neighbour' nobody knows what actually to do; the result is that one does what one likes with it. Under the pressure of relativism, then, the theory of rationality proposed in *Human Understanding* becomes obscurantist (Bloor, 1974a, p. 252).

Thus one might object that Toulmin, like Lakatos, depends on a retrospective (and revisable) assessment of whether an intellectual strategy has paid off. While there is clearly nothing wrong with *specific* judgements of this sort, made by a scientist's successors, such judgements cannot be elevated into an abstract evaluative system without emptying them of content.

There is another important difficulty in Toulmin's intellectual strategy which may be approached by considering his distinction between magisterial and intrinsic authority. It will be recalled that he seeks to avoid 'running the risk. . . of bowing absolutely to the judgements of currently authoritative groups'. The objective problem-situation exists, and the elites may not respond to it appropriately. This situation clearly corresponds to a commonsense view of the nature of science. But how does an elite maintain its authority if its judgements do not accord with those of Toulmin? Or, in such cases, how is authority transferred to another group?

> The ultimate source of the power that office-holders in a scientific profession wield is the implied consent of their professional colleagues in the same discipline. But this consensus is also the ultimate sanction, by which their power is controlled and their conduct kept within reasonable limits (Toulmin, 1972, p. 278).

But, as Bloor points out (1974a, p. 250) there is a difficulty here. If the magisterial authority of the elite is maintained by its long-term adherence to a general consensus, then surely it follows that in the long run there is no fundamental distinction between what is accepted by the general will, and what is intrinsically rational. This suggests a fully sociological account of rationality, and it also implies that Toulmin, in insisting that we should not bow absolutely to current elites, proposes that we should bow (at least provisionally and revisably) to their successors.

To put this point as concisely as possible : Toulmin and Lakatos both reject the idea that the validity of scientific beliefs should be judged in terms of their acceptability to contemporary elites. But their attempts to avoid this position in fact turn out to involve the disguised use of the judgements of later generations of elites. This is all well and good, but it should not be misrepresented as evaluation in terms of objective or impartial criteria.

To summarize: the above account has suggested the following major difficulties in the writings of the philosophers discussed. (1) The logics of science proposed vary greatly and (2) having an ambiguous relationship with the history of science, they may be expected to continue changing. (3) They have a tendency to become emptied of content, and (4) they can be seen as disguised versions of the judgements passed by later groups of scientists. These by themselves constitute good grounds for questioning the evaluative aims of those philosophers discussed, and consequently disregarding the epistemological objections to the sociology of science listed in a previous section. At this point, however, it becomes perhaps relevant to ask: what good reasons remain for undertaking the epistemological and evaluative study of science?

Before attempting to answer this question there are two preliminary issues which should be cleared up. First, it should be noted that, at least in the hands of certain authors (see for example Hesse, 1974) epistemology has moved close to the realm of speculative psychology. Hesse asks what, as a matter of fact, tends to make scientists and others surer of what they claim to be the case. Hesse's enterprise, whether or not her specific suggestions turn out to be well based, is thus of a very different kind to that of the philosophers previously discussed, and is not open to the same objections. Secondly, a clear distinction must be drawn between the evaluations that we constantly make as individuals on the one hand, and the general epistemological judgements that we are being asked to make by writers such as Popper, Lakatos, and Toulmin. In the latter case we are being asked to make use of general rules or criteria, which are held to be very widely applicable. It should be clear that it is possible to accept that we all make specific judgements, without at the same time allowing the utility of general epistemological evaluation. In what follows it is with the latter which we are concerned.

One reason for undertaking the epistemological and evaluative study of science, a reason that has already been mentioned, is that the establishment of criteria of demarcation is necessary in order to protect science from 'irrationalist' attack. The implication here is that a thin line of epistemologists (or sociologists) is all that stands between science and its annihilation by anti-scientific forces. It is possible that a view of this sort lies behind the writing of Merton and Popper mentioned above. I believe that this argument is fatuous. Masterman is surely correct when she notes that

> if there is not some self-correcting mechanism which operates within science itself, then there is no hope that, scientifically speaking, things will ever be set right when they go wrong. For the one thing working scientists are not going to do is to change their ways of thinking, in doing science, *ex more philosophic°,* because they have Popper and Feyerabend pontificating at them like eighteenth-century divines (Masterman, 1970, p. 6o).

Science, in other words, seems well able to look after itself, and most scientists are uninterested in the epistemological campaigns that are fought on their behalf. There is another, more fundamental reason this argument about the supposed irrationalism of Kuhn's position must be rejected as an argument for falsificationism. That is, quite simply, that it is an argument from consequences. Popperians dislike the supposed consequences of adopting a Kuhnian stance. Therefore, they are arguing, such a stance is wrong, or misguided, and should be abandoned. To argue in this way is, of course, quite illegitimate. Are Popperians really wanting to suggest that if an argument does not have desirable results, then it should be rejected as incorrect? If so then this is indeed a strange position for a philosopher. What matters, surely, is whether Kuhn's arguments are correct or not—not whether his arguments have certain unfortunate consequences.

A second possible justification for the study of epistemology must be taken a great deal more seriously. Thus, it might be argued that all analyses of science, historical or sociological, in fact employ epistemological criteria to demarcate science, and that the *implicit* use of these is dangerous: that clarification would be achieved and the analysis would be more firmly grounded by means of prior epistemological study. (A line similar to this is developed, for example, by Whitley (1972), who suggests reasonably enough that Merton's writing implies an inductivist epistemology, and by Martins (1972), who attempts to unravel the epistemological implications of the Kuhnian revolution.)

At this point, however, it is important to make a careful distinction between criteria of demarcation on the one hand, and the possibly underlying epistemological structures on the other. It seems intuitively probable that the human brain has certain capabilities, and lacks others. Hesse supposes an underlying propensity for inductive inference, and structuralists such as Chomsky or Piaget also have theories about human capabilities. But the important point about such 'psychological' epistemology is precisely that it does *not* offer us criteria of demarcation. Science is in exactly the same situation as other cultural enterprises.

This line of argument, then, while accepting that arguments and beliefs are liable to take certain forms, thus denies that these forms distinguish science from non-science. Furthermore it directs us to look for such distinctions in a very different way. Wittgenstein offers us a clue here, in his analysis of what it is to follow a rule. He leads the investigator to ask: why in any case should one *expect* consistent criteria of demarcation between science and non-science? Why assume that the word 'science' has a single meaning?

> How should we explain to someone what a game is? I imagine that we should describe *games* to him, and we might add: 'This *and similar things* are called "games" '. And do we know any more about it ourselves? Is it only other people whom we cannot tell exactly what a game is ?—But this is not ignorance. We do not know the boundaries because none have been drawn. To repeat, we can draw a boundary—for a special purpose. Does it take that to make the concept usable? Not at all! (Except for that special purpose.) (Wittgenstein, 1968, p. 33.)

Might not the sciences be seen as enterprises that resemble one another as members of a family? And why, in any case, should such resemblances always be abstract and *conceptual?* Might they not be in part social? (See Barnes, 1974, p. 99-102; p. 175, n. 13.) Looked at from this Wittgensteinian stance the philosophers discussed seem to be asking entirely the wrong questions. *Of course* you cannot expect to find an intellectual rule of hygiene that will distinguish between science and non-science for all purposes. *Of course* you find that the more sensitive you are to historical nuance the emptier your criteria become. People use terms like 'science' or 'good science' in different ways at different times. You can never fully anticipate how such a term might be used, what scientists might consider good science. You are asking the wrong sorts of questions. If you want to *draw* such distinctions, then this is all well and good, but they will be useable only for the purposes you have in mind.[6]

Broadly, then, the proponent of the strong programme of the sociology of knowledge responds to the accusation that he makes use of *implicit* epistemological criteria with cheerful agreement. What he denies is that these criteria have relevance for demarcation. He is directed to make such distinctions in terms of the cultural or social, and not in terms of the epistemological.

There is another version of this epistemological argument. It is suggested that the sociologist has no way of determining the extent of progress in science. This is true, of course, if the aim is to measure progress in terms of abstract criteria; but that it should be seen as a criticism of the sociological programme rests on the previously discussed confusion between personal judgement and general epistemological evaluation. Thus as citizens we are able to retain (or not) a commonsense belief in progress, while at the same time finding the possibility of progress problematical within the programme. Clearly the *perception* of progress by actors may be an important sociological issue, one which will be taken into account. But this does not mean that we must commit ourselves to an 'objective' account of progress (see Barnes, 1974, pp. 122-4).

From a sociological stance the actor's belief in progress can in any case be explained—for example with reference to Kuhn's discussion of authority in science, and the continual rewriting of scientific history undertaken in paradigm textbooks. And in a more fundamental way, a sociological analysis of our own commonsense belief in scientific progress can be suggested. Dolby writes:

> What is seen as progress in one cultural context can be seen as a distraction of the true path of science in another. We see science as having progressed, but this is largely because it is our own science that is most fully directed to the problems we are concerned with. We can predict that science will continue to progress by our present assessment only if we assume that future generations will tackle our problems and make assessments by our standards (Dolby, 1971, p. 15).

There is, however, another important argument for evaluation. This suggests that rational beliefs (knowledge) differ from irrational beliefs in their practical consequences: or specifically, rational beliefs work and irrational ones (often) do not. If this objection were sustained it would be important to distinguish between rational and irrational beliefs in order to develop a sociological analysis. Hence a criterion of demarcation would be necessary. Barnes has answered this in two parts (Barnes, 1972, p. 379). For first, it is historically the case that many 'objectively' false beliefs have none the less sustained manifestly efficacious practices, such as primitive agricultural techniques, or the eighteenth-century European chemical industries. Secondly, he makes the distinctions already made above, between what 'really' happens, and what the actors perceive as happening. Thus, the important explanatory variable is not the 'objective' assessment of the situation, but the actors' own judgements of efficacy.

Barnes advances essentially the same objection to Gellner's plausible view (1962) that anthropologists have a biasing tendency to assume that alien belief systems are rational, and then proceed to interpret them as such if this is at all possible. Gellner's view is that the objective rationality of belief systems may itself constitute an important variable for sociological analysis. Inconsistency in a belief system may, for example, lead to social change. Barnes suggests that

> Gellner is right to argue that sociologically interesting contradictions, inconsistencies and meaning-shifts occur in belief systems, and should not be glossed over. But, although these may be unperceived by the actors, they derive no sociological significance from their 'objective' features. If indeed they are of sociological interest they will be *potentially* intelligible to actors in their own terms (Barnes, 1972, p. 380).

This, of course, is a point that may be quite generally made in relation to philosophers' claims about the importance of the structures of objective knowledge.'

Yet another variant of the epistemological argument is reminiscent of one of the 'circularities' mentioned in an earlier section. Essentially this boils down to the assertion that proponents of the strong programme both want to eat their cake and have it: they want to deny the possibility of rational choice (on epistemological grounds) between theories in (say) physics, while advancing rational grounds for preferring their position as opposed to others in sociology. (See, for example, Whitley, 1974, p. 3-4.)

In fact it is difficult to see the force of this argument for it rests upon the mistaken assumption that in denying the relevance of an objective epistemology, the sociologist thereby forgoes the possibility of 'situated' evaluation. Consider this argument in more detail.

Clearly the sociologist of knowledge hopes to convince other sociologists of the wisdom of his particular view, and this implies that he has a view as to what constitutes a reasonable, indeed to him a convincing argument. But he also knows as a matter of fact that his arguments do not always convince others, and others think in different ways. At this point it is necessary to apply what will here be called a 'political' analysis.' Each group in sociology advances arguments that are 'reasonable' in its own lights, and hopes to convince others. Some sort of dialogue may be possible, for some commitments are shared between camps—most of us have undergone, for example, a similar professional training. But this does not mean that different camps share all relevant non-empirical commitments, and it does not mean that there exist overarching and neutral epistemological criteria with which they may finally convince one another. (As has been suggested above, the trouble with such claimed criteria is that they are sociologically empty, and may 'reasonably' be used in different ways.)

An identical 'political' picture of physics may also be presented, with the difference that here the sociologists do not (or should not) constitute one of the camps. Rational discussion is certainly possible, but there is no reason the sociologist must conceptualize this in terms of outside epistemological criteria.

The force of the criticism evaporates once it is understood that rational argument is always rational in relation to a particular 'political' stance (and is not monopolized by 'neutral' epistemological claims). Indeed, I suggest that it is important to avoid making the distinction between epistemological and sociological rationalities which constitutes the core of this objection. If the term 'epistemology' is to be used at all, then it must be related to what actors themselves regard as rational activity. The political analogy suggests that unless this is done, the term is merely being used as an ideological weapon by one camp against the others.

To summarize: the proponent of the strong programme hopes to convince others with what he thinks to be good arguments—but he doesn't necessarily expect to succeed. In denying the possibility of `apolitiar¯argument, he analyses discourse in the sociological profession in the same way as any other. Argument can only be persuasive and 'rational' from a particular stance. In this sense, however, all argument is rational, in sociology as in physics. But the difference is that sociologists participate in sociology, but not in physics !'

The above suggests that there is no reason the proponent of the strong programme should find the arguments in favour of epistemological analysis conclusive. It has been suggested that objective evaluation is irrelevant for an approach which seeks to emphasize the integrity of different cultural enterprises, and the very different standards of evaluation which they sustain. Thus, it is being suggested that it is important to develop an analysis of the situation from the point of view of the actors themselves, and to study the acceptance and justification of knowledge as it is actually practised. It is important to be clear on this point. Much work done by philosophers falls in part under this rubric. Popper, Lakatos, and Toulmin all use a variant of this method. This is well and good. However, the sociological end product is an entirely different one from that of the philosopher—the aim is that of explanation, the traditional aim of science, rather than that of judgement. The interest in evaluation is irrelevant to this programme and adds nothing to a sociological analysis. On these grounds it may be suggested that an objective epistemology is at best unhelpful.

In the final section I shall go further and suggest that the importation of an 'objective' analysis may in some respects constitute a positive hindrance to sociological analysis. To support this claim it is necessary to discuss the aims of the strong programme in the sociology of science in a little more detail.

DEVELOPING THE STRONG PROGRAMME

Consider, first, the possible aims and procedures of the strong programme in the sociology of science. At its most general such an enterprise seeks to understand the relationship between scientific beliefs and other aspects of the social world, whether these are belief systems of a different type, or features of the social structure which may not be visible to all or any of the actors in that structure. It commits itself to the four principles outlined in a previous section: causality, impartiality, reflexivity and symmetry.

It is important to understand that beliefs (or knowledge) do not exist in the abstract. Beliefs are held by people, who use them to organize action. Indeed, there is a truly intimate relationship between beliefs and action that tends to escape those who are committed to the separation of epistemological and sociological issues. There are, however, several sociological vocabularies that allow us to make this link. One of these is the symbolic interactionist notion of 'role-taking'. Essentially this stresses the claim that actors construct actions to achieve their purposes, this achievement being contingent upon the elucidation of the appropriate response from other relevant actors. People construct actions having made educated guesses about the responses of those around them. These educated guesses are built up by means of a process that Mead called 'taking the role of the other' (see Mead, 1962; Blumer, 1966). This involves putting oneself into the others' shoes and trying to assess the situation from their standpoints. Role-taking, then, has both cognitive and normative aspects, in exactly the same way as beliefs. Beliefs help to organize action while action helps to organize and reorganize beliefs. The symbolic inter-actionists tend to argue as if beliefs can be seen as internalized action, stored-up programmes, as it were, which can be called into play at appropriate moments.

This vocabulary can be developed in several different useful directions. For example, socialization is seen as a process of developing the ability to role-take successfully. But socialization also allows the sociologist to ask very general questions about the availability of certain role models. Thus, in the sociology of science we may ask about the availability (including desirability) of the 'scientific' role model, or that of 'physicist', `x-ray crystallographer', or 'protein x-ray crystallographer'. The actor is seen as learning to construct his own social and intellectual categories in general conformity with his socialization experience. If, for instance, the category of 'scientist' is not available to him, then this is clearly an interesting sociological observation."

It is important to remember that scientific beliefs may constitute an essential part of this role-taking ability. Thus, it is impossible to be a good crystallographer without a 'sound' grasp of the necessary techniques. Fisher uses broadly this perspective (1966) to bring out the joint social and intellectual nature of scientific beliefs. So the sociologist, by examining the availability of role models, can make quite general assessments of the relationship between beliefs and other aspects of social structure?'

Another use to which symbolic-interactionist vocabulary may be put is a detailed analysis of the construction of action. Law and French have argued that Kuhnian puzzle-solving is identical in form to the symbolic-interactionist view of action (Law and French, 1974). Symbolic interactionists assume that action, however badly constructed by 'objective' standards, must be seen as a genuine and thus (in the actor's terms) rational attempt to achieve a desired end. Clearly the actor must take a great many considerations into account before acting. In the realm of science these may include the various elements subsumed by Kuhn within the 'disciplinary matrix', or Toulmin's theoretical and disciplinary ideas. They will also include all sorts of 'non-scientific' elements. How they are marshalled depends both on the purposes of the actor and the nature of the situation as he perceives it.

With this in mind we now have a sociological rationale for the redescription of such epistemological antimonies as true/false, rational/irrational, logical/illogical and internal/external. Most scientific actors no doubt operate in terms of such categories, but we cannot move from this observation to the assumption that such criteria are uniform between different subcultures. The subject of sociological interest is precisely the way in which such categories are *used* in normal practice. No doubt some scientific actions are seen, for example, as irrational, depending perhaps on irrelevant external criteria. But our concern is not to abstract epistemological rules from the actual situations of their use. This, as Wittgenstein reminds us, is liable to prove a fruitless enterprise. The question, rather is with *which* rules actors use, and *how* they make use of them. Note in this context that the current `oversocialized' view of the scientific actor which seems to have been encouraged by Kuhn's commitment to a monistic paradigm (Martins, 1972, p. 25, Whitley, 1972, p. 78; Law and French, 1974) is inimical to this approach. W have to anticipate different accounts of the 'same phenomena', depending on the social position of the protagonist, and then resist the temptation to synthesize a 'true' account of what went on. (See Law, 1974)[12] What constitutes a justifiable innovation for one group may be seen as being distorted by outside influences by another. The job of the sociologist, then, is to describe the various rival accounts of the area with which he is concerned. He must then fit these accounts and the differing conceptions of 'what goes' into the context of what was called above a 'political' study of the structure of science. He must in other words attempt a study of the rise, maintenance or fall of particular viewpoints, realizing that such processes will not be seen as scientifically acceptable in all eyes.[13]

It becomes more than ever clear that 'objective' judgements do not have a role within such an analysis. The various antimonies discussed above must be related to the 'political' positions and purposes of the various groups under study. Their varying *use* as rhetorical devices is of interest. Judgements which correspond to attempts to get at a single and true version of an historical case must be resisted, as must general epistemological evaluations as to what constitutes the 'best' strategy.

It can now be suggested that the search for general epistemological standards tends to obscure the vitally important 'political' processes which exist in science. We must be aware that we are in no position to offer a balanced judgement of the views of the different parties. To imply that we can do so is a self-delusion which covers up a form of partisanship behind ostensibly neutral standards. As such it has no place in the sociological analysis of science.

I suggested in the introduction to this paper that the distinction between an evaluative and a non-evaluative approach rests in the last resort on non-empirical foundations. But in view of the above, there seems to be no reason why a vigorous programme in the sociology of knowledge should be inhibited by epistemological criticism. Sociologists should rather involve themselves in the development of their own approach and attempt to achieve their own goals. And as a footnote, they might ask, 'Are the sociologists or the philosophers more likely to change their minds?' Given the fact that the philosophers' position appears to be unstable in its own terms, a bold sociologist might be willing to predict that his own programme will outlive that of the epistemologist.

NOTES

1. This paper, which is primarily a review of the current state of the debate, borrows extensively from other writers. I have acknowledged specific borrowings, but I would like in addition to indicate my general intellectual debt to Barry Barnes and David Bloor, both of the Science Studies Unit, University of Edinburgh, without, of course, wishing to incriminate them in any of my own formulations.
2. The interested reader may consult Popper's own works, or Lakatos's incisive analysis of the development of Popper's thought (as well as his own): See Popper 1959, 1965, 3972; Lakatos, 1970. See also Barnes, 1974, pp. 22-6; pp. 45-8; p, 162, n. 9).
3. Lakatos's substantial revision of Popper's programme (Lakatos, 1970) has been read by its critics as a betrayal of this programme, being identical in form to the much criticized Kuhnian view. (See, for example, Bloor, 1971.) Some of these issues will be discussed below.
4. Although a Popperian sociology of science would have to avoid discussion of vital epistemological issues, it is none the less a conceivable enterprise. Merton's writing (although inductivist (Whitley, 1972)) illustrates that this is the case. He avoids all discussion of scientific method, implying that sociology is not competent to discuss such issues (Merton, 1957, p. 554 The 'ethos' of science, and in particular the emphasis on autonomy and purity (Merton, 1957, p. 543) can sound distinctively Popperian. Thus, both Merton and Popper seek to defend the rationality of science against outside authoritarian. Their rhetoric, that of the 'reason' of liberal democracy, is identical (Merton, 1957, p. 561; Popper, 1945, p. 223). Popper's curious claim that the sociology of knowledge is psychologistic can surely be disregarded.
5. One is reminded of the scorn poured by Popper on the sociologists of knowledge for inferring underlying distorting 'interests' in actors' beliefs. Yet the Popperians are in a structurally identical position, interpreting action in terms of underlying logics of science.

6. Recent ethnomethodological writing in sociology has made essentially the same Wittgensteinian point in relation to the following of norms. See, for example, Wieder (1974). Kuhn also uses Wittgenstein extensively in broadly the manner suggested above.
7. Consider, for example, Popper's third world. It will be recalled that Popper argues that a proper understanding of the second world depends on knowledge of the third world. In fact there is no little ambiguity about the status of the third world. At times it seems that Popper means it to apply to structures that are intelligible, or at least potentially intelligible to actors—to the sorts of structures that Barnes recommends that sociologists should consider. Various writers have pointed out that there are important similarities (though some dissimilarities) between Popper's third world and what sociologists call culture (Martins, 1972, p. 42) or the social world (Bloor, 1974b, p. 70). Read in this way, Popper's claim can be redirected to the assertion that it is impossible to understand the psychology of beliefs or actions without a proper understanding of the cultural or social worlds—a claim that is, of course, a commonplace in sociology. Yet, in view of the fact that Popper claims objective epistemological (and ontological) status for the third world, it seems very unlikely that he would find this reinterpretation acceptable!
8. In order to avoid misunderstanding it is necessary to note that the metaphor of politics, like all metaphors, is only partially appropriate. It is here intended to draw attention to the following features of sociology and science: that different traditions or 'camps' have (I) somewhat different ways of viewing the 'same' material, (a) somewhat different views about what constitutes a 'reasonable' argument. The above are a result of (3) somewhat different non-empirical commitments which can in turn (4) be traced at least in part back to different socialization experiences. It should be made clear, however, that certain features of politics—for example 'propaganda'—do not have their metaphorical equivalents in sociology, or science.
9. Perhaps the other circularities may be briefly discussed at this point. The first concerned the discovery of a theory of discovery. It has been widely suggested that innovation may relate to the use of metaphorical extension (see Hesse, 1966; Schon, 1967; Bloor, 19730; Barnes, 1972), and can sometimes be understood in this way. But these analyses do not involve the development of a theory of discovery in a literal sense—the sense that we know what is to be discovered before it is actually discovered. No one has proposed that this is a possible task!
The second concerns the use of logic to deny the relevance of logic, which in this discussion may be linked to Popper's objection to the idea that logic is equivalent to the fact that we cannot think in other ways. The status of logic will not be discussed in detail here, as it has already been examined by Bloor elsewhere (Bloor, 1973b). Essentially these objections may be handled in the following way: leaving aside the identification of rational discourse with logic, which is implied in Popper's objection (Popper, 1970, p. 56), it may be noted that the force of these criticisms depends on the notion that logic exists in a platonic realm. Once this realm has been discovered, action that conforms needs no further explanation. Thus, Kuhn is seen as using the structures of this realm to deny its existence—a position which is self-contradictory. Bloor notes that even Mannheim appears to have accepted this platonic view of logic. However, he notes that an alternative analysis based on Wittgenstein's *Remarks on the Foundations of Mathematics* is possible. Essentially this alternative view rests on the suggestion that logic and mathematics are compelling not because they correspond to a platonic realm of truth 'as such', but rather because they correspond to social norms:

> the laws of inference can be said to compel us; in the same sense, that is to say, as the laws of human society (Wittgenstein, 1956, p. 34e).

In this view the laws of inference may not be psychologically compelling (the suggestion opposed by Popper) but rather social. A similar view was adopted by Mills (1963), a point recently outlined by Phillips (1974).
If this position is adopted then it may add further weight to Kuhn's argument about the incommensurability of paradigms, and it certainly undermines Popper's point about using logic to deny the possibility of logical discourse. For in this view, Kuhn is using (situated) inference to argue that inference is situated, which seems a perfectly consistent position. It should, however, be noted that it may be the case that social laws of inference are widely shared throughout the Western world, and as such are shared by both Kuhn and Popper, as well as the adherents of rival scientific paradigms!
10. Much work on this, though using a slightly different vocabulary, has been undertaken by Ben-David (1971).

11.Consider in this context Mullins's excellent work on the development of the phage group of molecular biologists (1972).
12. In science one of these views often wins out. We then get the reinterpreted history of science which we are warned against by Kuhn and others. Collins (1973) makes the point that we may still be editing our accounts of science to eliminate the many minor false starts, mistakes and incidents of laboratory procedure in ways which would be avoidable by means of detailed ethnographic techniques. Having now learned to avoid the pitfalls of *grand* reinterpretation we may still have to deal with *mundane* (but highly important) reinterpretation.
13. For another view similarly stressing the importance of authority in science see Phillips (1974).

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