

Naturalism, Epistemological Individualism and "The Strong Programme" in the Sociology of Knowledge

PETER T. MANICAS and ALAN ROSENBERG

INTRODUCTION

It is now over ten years since Barry Barnes and David Bloor first began publishing arguments in favor of what is now usually called "the strong programme" in the sociology of knowledge. Unsurprisingly, we are still a long way from having any sort of consensus on the merits of the program, but surprisingly, at least as we see it, the literature abounds in what to us (and we think to Barnes and to Bloor) seem to be enormous misunderstandings of both what the program is and what it implies. In what follows we make the effort (again) to do some clarifying, to suggest a hypothesis as to what explains the confusion and finally to suggest that the work of Barnes and Bloor is of enormous importance to philosophy, psychology and sociology-not merely to the sociology of knowledge, but more generally to the human sciences.

There would seem to be no easy way to proceed, since as we shall argue, the strong program connects in decisive ways to a large number of tendencies and problems in general philosophy, including epistemology, the philosophies of logic and of language and thence, to questions regarding the nature and theory of the sciences. It may be useful here to state, if somewhat roughly and provocatively, the main claims of our paper:

1. The strong program is an extremely comprehensive and sophisticated philosophical naturalism, but unlike many historical naturalisms, it is neither "half-hearted" nor monistic.¹ Because it is not half-hearted, the strong program is committed to a radical relativism and conventionalism. By rejecting the search for extra-natural and extra-social foundations for knowledge, the strong program situates the problem of vindication historically and is able to do so nonviciously. It can, because as a naturalism it is not only consistent with a realism, it requires one.

2. The assumption which prevents otherwise sophisticated critics from seeing the force of the strong program and which leads to some radical

misunderstandings of it is an epistemological individualism which is so thoroughly pervasive in the literature (and in our culture!) that it goes unnoticed by philosophers and scientists who would be insulted by being identified as representatives of it.

Steven Shapin has suggested a neat way to put the point: If claims to knowledge were simply determined by the nature of reality, then no sort of sociological account could be offered. We could picture the solitary scientist, confronting the world, framing his hypotheses and then testing them until he arrived at the truth.² *Nobody*, we may assume, would these days offer such a crude (positivistic) picture. Yet if so, how could it be forgotten that *everything* bearing on *any* inquiry, the theoretical and methodological considerations, indeed, the very language and ideas in use are social in nature and could not be otherwise.³

3. If Barnes and Bloor are correct, as we think they are, then we can get a clearer picture of the nature and tasks of experimental psychology and its relation to the social sciences. For example, we can see in a clear way what, e.g., a learning theory, should be, and thence why a range of theories, e.g., in current cognitive psychology, are unlikely to bear much fruit. We see this to be a vitally important upshot of the strong program and we make it the final argument of our account.

The Strong Programme

What then is "the strong programme"? David Bloor has usefully defined it with four "tenets": causality, impartiality, symmetry and reflexivity, as follows:⁴

1. *(The inquiry) would be causal, that is, concerned with the conditions which bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief.*
2. *It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation.*
3. *It would be symmetrical in its style of explanation. The same types of causes would explain, say, true and false beliefs.*
4. *It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself.*

None of these "tenets" of course are self-evident nor do they explain themselves. We make the effort, following Bloor and Barnes, to clarify and defend them. But we do this in terms of a comprehensive philosophical view. Tenet number one, for example, requires excursion into the philosophy of science and requires also that we speak directly of psychology. Tenet four needs to be addressed against charges that the strong program is self-refuting.

It is important to distinguish between two questions: whether on the one hand, historical and sociological studies of what is *taken to be* knowledge at any given time and place can proceed without concern for the epistemological status of the beliefs which are held by the pertinent communities under study; and whether, on the other hand, such inquiry itself *presupposes* an epistemology (and ontology)?

The first question seems easily answered in the affirmative since one can readily distinguish knowledge defined as true belief from what is *taken to be* knowledge by the pertinent communities, whether or not it is "true". Since, then, the sociologists' definition of knowledge is different than the philosophers', their problems would seem also to be different and thus it seems that sociology of knowledge is epistemologically either irrelevant or neutral. This carries however only if the second question is in the *negative*.

Neither Barnes nor Bloor have been as clear as they might be here and this has, in part at least, propelled confusion over what is already a difficult and confusing set of issues.⁵ This confusion is most vivid perhaps as regards criticisms, from the "left" as well as "right" of the application of the strong program to the sociology of *natural* science.

The "right" has taken for granted that knowledge *is* true belief. It then sees the sociology of knowledge to be concerned, as R. K. Merton long ago put it, with "the relations between knowledge and other existential factors in the society or culture". On this view, science produces true belief but we can still ask what are the social relationships, moral ethos of scientific cultures, institutional and historical factors which either promote or inhibit this. In the extreme form, "social influences" enter only when the scientific effort at producing true belief is *distorted* by non-scientific, i.e., "social" or "political pressure", e.g., as in Lysenkoism. As Werner Stark sharply put the position, "social developments do not determine the content of [genuinely valid] scientific developments, simply because they do not determine natural facts". Hence, in parallel fashion, the history of science can be written "internally". That is, changes in the knowledge claims of science are explainable almost wholly in terms of reasons internal to the activity of science itself.

It is just at this point that *philosophy* of science enters. One must presuppose some theory about what science is and what defines its particular rationality. Larry Laudan's *Progress and Its Problems: Towards a Theory of Scientific Growth* is in this regard exemplary since he, *unlike* most, is clear in contending that the historian of science must make judgments regarding the *rationality* of scientific claims. This, presumably, is a *philosophical* problem, and presumably, historians of science must either be philosophers *or* they must be dependent upon conceptions of scientific rationality as these are articulated and defended *by* philosophers. Laudan is exemplary, of course, because he is clear about this.⁶ Other defenders of what might be called "rationalist" conceptions

of the history (and sociology) of science, Ben-David, Merton, Lakatos, McMullen and others, have been less clear in this regard.

Left criticisms of the strong program, though far fewer in number and influence, take an opposite stance. They suspect, rightly in our view, that the advocates of the strong program remain committed to the realist historians' goal of telling it *wie es eigentlich gewesen*. From the Left's point of view, while the defenders of the strong program correctly reject the "internal/external" distinction as regards the explanation of scientific change, with the "empiricists", they fail to see the difference between asking, "What is really going on in science?" and "How do scientists construct their versions of what is going on?" For left critics, it is only the latter that a sociology of science can answer because the former question, in their view, is *meaningless*.⁸

The point then is this: Both left and right have epistemologies and there is no way for the strong program to avoid the issue. And of course, they have not.

It may be most efficient to begin by noting that versus the "right", the strong program is *inconsistent* with any sort of foundationist (absolutist) epistemology, that following Hesse, the strong program holds that "there are no extra-natural and extra-social grounds of rationality and truth in the *a priori*, the analytic, or the necessary, and *a fortiori* that no transcendental argument or rational intuition can claim to have access to such grounds".⁹ This means not merely that we can never have *certainty*, but that there are no grounds: in logic, "the given", or elsewhere, in terms of which knowledge claims are justified. Now while a relativism (relationalism) is a consequence of the absence of a point of Archimedes, an epistemological nihilism is not. Indeed, in the epilogue to *Scientific knowledge and Sociological Theory*, sensitive to the tension of his "skeptical" position, Barnes asks,

Why, it will be said, if the preceding account makes no claims to being the best account we have in a fully objective [i.e., absolutist] sense, should anyone accord it credibility? This, of course, is the classic argument against relativism in a slightly different guise. The answer is easily given; this whole volume is crammed with proffered reasons why its main tenets should be accepted; its justification lies within itself (p. 156).

It will be well to be as explicit about this move as possible. It involves showing that the argument from self-refutation is fallacious and that once its presuppositions are exposed, they can be replaced with vindicable assumptions which powerfully support the claims of the strong program. As regards the argument from self-refutation, Hesse writes:

The usual form of the argument goes as follows: Let P be the proposition "All criteria of truth are relative to a local culture; hence nothing can be known to be true except in the sense of "knowledge" and "truth" that are also relative to that culture. Now if P is

*asserted as true, it must itself be true only in the sense of "true" relative to a local culture (in this case ours). Hence there are no grounds for asserting P (or, incidentally, for asserting its contrary)".*¹⁰

As Hesse points out, the problem here is the conclusion, "Hence there are no Grounds for asserting P". "No grounds" must mean no grounds in the *absolutist sense*. That is, *in the absence of a foundation*, there are no grounds in this Archimedean sense. But that surely comes as no surprise to the relationalist (anti-foundationist). Indeed, "if a redefinition of cognitive terminology as relative to a local culture is presupposed in asserting P [as is the case as regards the strong program], then P must also be judged according to this redefinition" (p. 42), not according to the absolutist standard. The question for us is whether *relative to what we know*, the case can be made for the strong program. The case is not made absolutely, nor if the strong program is correct, could it be so made. Indeed, we should think of the whole program, with its epistemological and ontological presuppositions--to be detailed shortly--and its consequences, e.g., its bearing on inquiry in the human sciences--to be remarked on before we finish--as parts of an extended argument in its favor.¹¹ We think that it stands up very well and as part of the problem of showing this, we can turn first of all to the *ontology* which is a key element of its philosophical naturalism.

CONVENTIONALIST REALISM VERSUS EMPIRICAL REALISM

A recurring argument offered by advocates of the strong program is the argument that the accumulation of evidence from anthropology and the history of science powerfully supports relativism in the sense that there is no valid way to discriminate between what is true and what is locally accepted as true. A number of writers have found this claim to be especially paradoxical. Steven Lukes, in several papers, has insisted that unless some relativism is denied, that evidence would never even show up. He writes:

*...In the very identification of beliefs and a fortiori of belief systems we must presuppose commonly shared standards of truth and inference, and we must presuppose a commonly shared core of beliefs whose content or meaning is fixed by application of the standards. Neither the evidence of cross-cultural variation in the schemes of classification, nor that of radically divergent theoretical schemes or styles of reasoning, nor arguments for the possible applicability of alternative logics undermine this position, which, must indeed, be accepted before the problem of relativism can be set up on the first place.*¹²

This argument, called the "bridgehead argument" is explicitly and carefully defended by Lukes, Martin Hollis and others, but it is crucial to notice that it is the taken-for-granted point of departure of a great deal of recent

philosophy including work by Davidson and Quine, among others, who, perplexed by the problems of translation, have argued that to quote Davidson, "we cannot make sense of error until we have established a base of agreement". This, argues Lukes, is an *a priori* truth, although he concedes that there remain disagreements between those who argue that the bridgehead is "fixed" (Hollis) or "floating" (Newton-Smith, Horton etc.). A useful way to characterize all such views (fixed *and* floating versions) is to identify them as *empirical realist*, defined as the view that *something* of reality is a *shared experienced reality*.

Neither Barnes nor Bloor are, then, empirical realists, since they deny that something of reality *must* be shared reality. But this is no solipsism since translation--indeed, *communication--presupposes no shared experience*, even though it needs, at a minimum, *a shared material world. This difference is fundamental.*

The material world is *part cause* of our (yours and mine) experience. But the material world is not *itself experienced*. Its "effects" are what is experienced. Kant's *Ding an sich* is part cause of our experience but it is never known directly in experience. It is always "known" as "verbalized", always *mediated by socially constituted forms*.

Skeptical arguments about "shared experience" do not and cannot lead to solipsism since there is material world, or better, there is a nonexperienceable world which is part cause of our experience. This is not, as some writers have suggested, either "an unargued metaphysical claim" or an assumption "inconsistent with the overall conception of knowledge and interests" put forward by Barnes and Bloor.¹³ It is indeed presupposed by the conception of knowledge put forward and is *defended* as part of the defense of *that* conception of knowledge. To be sure, more needs to be said on how it becomes part of the naturalized epistemology which also undergirds that conception of knowledge.

This *weak* form of realism--that there is an independently existing 'material', world, *never experience as it is--is sufficient* to dissolve the problem of "other minds" *and* to guarantee the *possibility* of communication or translation which, of course, begins at home! Moreover, once put this way, the idea of common *experiences* can be explained without the need of a bridgehead, even though, as Bloor has said, it may be the case that we do *in fact* have common experiences.¹⁴ This is an empirical question to be decided by inquiry. We can, of course, justifiably believe that we are creatures of similar sorts and thus that we have the same sorts of perceptual and cognitive potentialities. Since, further, there is a material world existing independently of us, it would be hardly surprising were there to be elements of our experience--the stubbornness of sensation--which are more or less "universal". The immediate practical similarity of this position and the very different "bridgehead" posture of half-hearted naturalisms has tended to confuse the argument between the sort of minimal *empirical* realisms of, for example, Lukes or Quine and the relativism of Barnes and Bloor.

But further confusion comes from the other side with the sort of tolerant relativisms of the Winch, Rorty variety, at least as they often express themselves, who suggest that in the absence of an *epistemological* mooring, *anything* goes. That is, while they might not deny the existence of an independently existing causally efficacious reality, their relativisms seem to be utterly unglued, in effect *idealisms* in which there are no extra-social, extra-historical constraints on belief. On such a view, it is hard to see what science could be *about*. On the view of the strong program, as we argue below, it is precisely about causes operating in the world, including therefore the physical, psychological and social causes of experience and belief.¹⁵ On the other hand, in contrast to half-hearted naturalisms, because the independently existing material world is but *part* cause of our experience, "conventions" explain the most radical of relativisms, perceptual, cognitive *and* epistemological. All of this, as Barnes and Bloor have insisted, is open to naturalistic inquiry .

The foregoing is precisely summarized in Barnes's *Interests and the Growth of Knowledge* (1977) where he writes:

The naturalistic equivalence of the knowledge of different cultures is merely a finding, something which happens to be the case. To be sure, it implies the conventional status of naturalism itself, but this is not disaster. It does not imply the abandonment of naturalism in favor of necessity elsewhere...

Naturalism, moreover, implies the most intensely serious concern with what is real. ... Everything of naturalistic significance would indicate that there is indeed one world, one reality, "out there", the source of our perceptions if not their total determinant [that is, though not their total determinant!], the cause of our expectations being fulfilled or disappointed, of our endeavors succeeding or being frustrated. But this reality should not be identified with any linguistic account of it, or, needless to say, with any way of perceiving it, or pictorial representation of it. Reality is the source of our primitive causes, which, having been pre-processed by our perceptual apparatus, produce changes in our knowledge and the verbal representations of it which we possess. All cultures relate symmetrically to this reality. Men (sic) in all cultures are capable of making reasonable responses to the causal inputs they receive from reality—that is, are capable of learning. That the structure of our verbal knowledge does not thereby necessarily converge upon a single form, isomorphous with what is real, should not surprise us. Why ever should we expect this to be a property of our linguistic and cognitive capabilities (p. 25)?

CONVENTIONALIST REALISM VERSUS PROGRESSIVE REALISM

We have so far argued that the strong program rejects empirical realism; it is rather an argument for the existence of an independently existing reality which is not experienced as it is. But more must be said about the realism of the strong program. We argue that it is committed to a form of "scientific realism", understood as the idea that scientific theories are *about* real [continued]

mechanisms and causal processes, but that both "progressive realism" and an "essentialist realism" in which nature is understood to be constituted of essences must be rejected. The ideas which taken together comprise the "conventionalist realism" of the strong program makes for a realism which, we think, is original and important. As we argue, the first tenet itself pre-supposes a non-foundational scientific realism but neither progressive realism nor essentialism.

"Progressive realism" may be considered first. Following Barnes, we may say that it is "one of the ideal accounts of scientific knowledge which has it moving toward something, in this case a description of the real mechanisms in the world".¹⁶ Such a view, if it could be sustained, would be gratifying and, we may suppose, would go a long way toward putting the critics of the strong program at ease. Unfortunately, from the point of view of the strong program, it cannot be sustained. On the premises of the strong program, our present scientific theories stand exactly symmetrical with earlier theories. As Barnes writes:

...Our current scientific models and mechanisms are likely to be seen at some future time as part of what is an endlessly unfolding chain of such mechanisms, constructed and eventually abandoned (or stripped of their ontological standing) as the activity of knowledge generation proceeds (ibid.).

Current science--as all other--is but an interpretation or representation of reality and there can be no way to measure any such representations against reality-in-itself--the assumption of all absolutist theories of truth. But if so, then there can be no way to know whether some particular representation of reality is a truer (in the absolutistic sense) approximation of reality. We may, of course, want to believe that it is; moreover it may even be a normative ideal of scientific inquiry, worth maintaining. Finally, we may say that some representation is *better or worse*, but then we have altogether altered the problem, for with this move, we adopt not the disengaged perspective of the absolutist view, but the engaged perspectival views of the socially produced and sustained conventions of particular cultures and societies.¹⁷

A number of writers have assumed, we believe, that while this move is salutary, it makes judgments of even better or worse between different cultural perspectives and indeed between competing theories within "science" ultimately *irrational*, substantially because in the absence of *shared* criteria which can themselves be justified, no rational basis of judgment remains. The objection, of course, is exactly on a par with the argument from self-refutation (above). As with the assumption that knowledge requires an absolute ground, it is here assumed that judgment requires some ultimate evaluative premise. But why should this be so? If the strong program is correct, we should expect judgments (what is true/false, better/worse, etc.) to be context-bound and unfinal. Moreover, and crucial as regards the problem [continued]

of science, no judgment requires the assumption of progressive realism even if all of them suggest that we see knowledge as related to activity which consists in human efforts to deal with "a world", social world, to be sure, but a social world which has as its primitive determinant, a reality which exists independently of us.

THE STRONG PROGRAMME AND SCIENTIFIC REALISM

A number of critics of the strong program have attacked the idea that the strong program represents "a robustly 'scientific approach' to the problem of human belief" and in particular they have challenged the claim that the four tenets which define the strong program are "principles of inquiry to which every genuine scientist, natural or social is committed".¹⁸ In effect, then, the challenge is that what Barnes and Bloor identify as science is *not* science!

But of course, from the point of view of the strong program, what is usually dubbed "philosophy" of science is mainly or not at all a *description* of science but is a normative inquiry, "a rational reconstruction" of science, uncontrolled by evidence and designed to stipulate what science should be.¹⁹ To be sure, the naturalism of the strong program must begin with scientific practice as it is and has been if it is to shed any light on the nature of scientific knowledge. We cannot here develop in any way the picture of science and the evidence for it which has been offered by the work of Kuhn and by researchers following the tenets of the strong program and we must therefore but assert that this picture seems to us to be a more adequate picture than the one which Scheffler called the "standard view".

On the other hand, we think it important to confront one issue in recent philosophy of science, relevant to the realism of the strong program and especially to the notion of causality which is presupposed in its key first tenet.

In codifying the standard view of science, Ernest Nagel many years ago concluded that as regards instrumentalism and realism, "the opposition between these views is a conflict over preferred modes of speech".²⁰ In our view, this conclusion is both widely held and pernicious exactly because it is only if there are relatively enduring real agencies and mechanisms which are causally efficacious that we can understand both ordinary causal talk and the more sophisticated efforts of scientific theory .

Nagel held that for the instrumentalist, "a theory is held to be a rule or a principle for analyzing and symbolically representing certain materials of gross experience, and at the same time an instrument in a technique for inferring observation statements from other such statements" (p. 129). For the instrumentalist, theories are but "convenient fictions" and the task of science is but to provide "a suitable language for describing the body of knowledge-- gained by observations--which is relevant for predicting the future behavior of the system".²¹

The scientific realist has a radically different view of the matter. For the realist, theory aims to represent real structures and mechanisms and the goal of science is to explain via appeal to these. That is, as belief is the causal product of a variety of mechanisms: physical, psychological and sociological, so too is everything else. There are thus different *kinds* of causes (mechanisms, structures) operating in the world.²² We can say then that the different "sciences" have the problem of attempting to articulate these in theory.²³ We believe that it is not difficult to show that the instrumentalist conception of science fails to do justice to actual scientific practice.²⁴ But indeed, "instrumentalism" fails all the more to do justice to the more primitive efforts of ordinary people in their encounters in ordinary life.

This can best be brought out, perhaps, by considering the problem of truth. David Bloor has rightly observed that "there is little doubt about what we mean when we talk of truth. We mean that some belief, judgment or affirmation corresponds to reality and that it captures and portrays how things stand in the world". Moreover, "talk of this kind is probably universal".²⁵ Bloor then insightfully discriminates three functions of talk of truth. First, "there is what may be called the discriminatory function. Men are under the necessity to order and sort their beliefs. They distinguish those which work for them from those which do not".²⁶

Indeed, it is just this function of talk of truth which pragmatist philosophers and instrumentalist theories of scientific theory latch onto--and of course, it is an essential feature of all our talk about truth. Moreover, if this were the full story as regards the use of truth-talk, it would be possible "to see theories entirely as conventional instruments for coping with and adapting to our environment". We could dispense with "truth/false" and replace it with "works/does not work" or "predicts/does not predict", etc.

But this is not the whole story. There is also a rhetorical function: "these labels play a role in argument, criticism and persuasion" (p. 34). If, which is not the case, "our knowledge were purely under the control of stimulation from the outside world" there would be no problem about what to believe (*ibid.*). It is precisely the point of the strong program that because our knowledge is socially produced, there is "a continuing problem of maintenance" and that "the language of truth is intimately connected with the problem of cognitive order" (*ibid.*). "Nature has power over us, but only men have authority". But a purely instrumentalist vocabulary could *not* serve *this purpose* and that is exactly because of what Bloor calls the "materialist function" of truth-talk: "

All our thinking instinctively assumes that we exist within a common external environment that has a determinate structure. The precise degree of its stability is not known, but it is stable enough for many practical purposes. The details of its workings are obscure, but despite this, much about it is taken for granted. ...Often when we use the word "truth" we mean just this: how the world stands (p. 36).

As is quite plain, it is just this feature of talk about truth which *traditional* realisms and the so-called correspondence theory of truth latches on to, as indeed, it is just this feature of *our experience* which leads us to do *science--as* that is *continuous* with common sense. Bloor exactly diagnoses the trouble with traditional realisms and their corollary, the correspondence theory of truth. The *meaning* of truth is illicitly converted into a *criterion* of truth. That is, "truth" *does mean* "correspondence with reality", but it is *not possible* to step out of experience and compare our beliefs with reality. Thus it is that "all matters of substance, all issues of particular content have to be fought out in their own terms and independently" (p. 36).

To say that a current physical theory *is true* is to say that it "represents" reality, *as far as we know*. And this means that, as far as we know, the mechanisms postulated do *exist*. The norms for making this judgment are *internal*, created and sustained by pertinent communities. And no doubt, pragmatic and instrumental considerations enter particular judgments, if by that one means that there are empirical consequences. On the other hand, as correspondence with reality is not a *criterion* of truth, neither is "success" or "coherence". Indeed, the argument that neither logic nor experience is *sufficient* for judgment cuts as much against instrumentalism as it does against traditional realisms. And the tension between the two is resolved by noticing, as Barnes and Bloor have both repeatedly insisted, that knowledge is not related to activity *rather than* to reality, but is related to activity which consists in our persistent efforts to deal with our world. In contrast to traditional realisms, theories are not judged to be true *because* they correspond to reality. In contrast to idealistic theories of knowledge (of which pragmatisms often bear striking familial resemblances), "knowledge is found useful precisely because the world is as it is; and it is *to that extent* a function of what is real, and not the pure product of thought and imagination".²⁷

CONVENTIONALIST REALISM VERSUS ESSENTIALISM

The realism of the strong program is also to be distinguished from essentialist realisms which hold that "since the realist mode of speech is so widespread and effective ... physical nature must surely be constituted of essences underlying the appearances of phenomena".²⁸ As Barnes has argued, while there can be little doubt that essentialist "strategies" are profoundly characteristic of science (vs., of course, instrumentalisms) and moreover, while all cultures use what amounts to essentialist strategies in seeking "to organize an unimaginably complex environment by the use of a few thousand symbols" (*ibid.*), this is not sufficient to hold that physical reality must conform to *our* strategies for grasping it. As Barnes writes:

It is correct to say that the very Structure of the knowledge which men produce presumes that reality is constituted in terms of enduring agencies and mechanisms;

*this is how knowledge gains its essential coherence, and why its verbal component is invariably a finite system of symbols. It is also correct to insist that existing knowledge, the material cause of new knowledge, will always be more than arbitrary.*²⁹

But as with the claims of progressive realism, these points do not suffice to infer that there are essences which are objects of knowledge. The point is not just that "the essences of yesteryear may be dismissed today as nonexistent or as metaphysical garbage",³⁰ for that it is to acknowledge what seems undeniable, viz., that scientific theories *along with* their ontologies are rejected and replaced. It is to see also that essentialist strategies are just that--extremely persuasive ways by which cultures organize "an unimaginably complex environment by the use of a few thousand symbols". That is, "essences" identified by this or that culture, its division of "natural kinds", its "real definitions", even its "logical necessities", as we shall argue, are but "conventions" produced and sustained by social activity. These are not, to be sure, made true simply by decision and they are not arbitrary, if by that one means that we can make any decision we please. Indeed, the "conventions" of language and its categories, as all other socially constituted and therefore but relatively enduring social structures, are reproduced and transformed from materials at hand, materials which can seem to have the force and liveliness of "necessity". We can, with Bloor, say that such conventions are constrained by "social credibility and practical utility", but this says very little of real interest to us as inquirers, for indeed, the particular and concrete forms of "social credibility" and "practical utility" are just that which become the genuine topic of empirical work in the sociology of knowledge.

A "conventionalist realism", however, must speak as well of the role of psychology in our understanding of "the world". The critics of the strong program have failed to notice that the realism of the strong program is joined to a conception of psychology in which humans are capable of learning but at the same time where all knowledge is social. To say that *all* knowledge is social is no trivial matter. Indeed, it is not extreme to say that *most* of the epistemology in the Western philosophical tradition denies it, that it is committed to an epistemological individualism whose implications for psychology and the social sciences are truly monumental.³¹

EPISTEMOLOGY SOCIALIZED

We can begin with the obvious: We have natural reasoning propensities and we are socialized into specific societies. So much is not denied by anyone. Yet if so, we need to see why, as with the claim that all knowledge is social, this point too is not trivial. A full-fledged naturalism is willing to press out the full significance of these facts. Epistemological individualisms, "naturalized" or "unnaturalized" are not.

Barnes and Bloor have both utilized a distinction between "natural rationality" and "normative rationality". Both are of concern here, and as they are understood by Barnes and Bloor, both profoundly challenge the ideas of much recent philosophy. Bloor writes:

*Natural rationality refers to typical human reasoning propensities; normative rationality refers to patterns of inference that are esteemed or sanctioned. The one has reference to matters of psychological fact; the other to shared standards or norms.*³²

Now it may be supposed that this distinction poses no difficulty for the typical epistemologist, that he or she will not deny that there is a *psychology* of cognition. The epistemologists' concern is normative in the sense that it is concerned with the problem of justifying our claims to knowledge-an entirely different question. But as is clear, for the strong program, what patterns are esteemed or sanctioned is also an empirical question. Indeed, for a *naturalized* epistemology, the philosophical project of grounding true belief has failed, and it failed because it sought grounds in the *extra-natural*. Quine's advocacy of a naturalized epistemology is a good contemporary example. He was correct to insist that "we are after an understanding of science as an institution or process in the world, and we do not intend that understanding to be any better than the science which is its object".³³

We are, of course, interested in "science" in its *original* sense, *scientia*, "knowledge", but *that* is not the serious objection to Quine's formulation of his project. It is the second occurrence of "science" which is troublesome, for Quine seems at least to mean by this that but one science is pertinent to epistemology-the science of *psychology*. But if *all knowledge is social* then an adequate account of knowledge will have its psychological side, but it will have its sociological side as well. Bloor, in response to a fundamental misunderstanding by one of his critics says: "The fact is that society gets into knowledge right at the ground floor, in the most elementary steps of language learning, and in the most elementary links that are forged between concepts and the world".³⁴

In what follows we try to clarify this versus a pervasive and unacknowledged epistemological individualism. But it will pay to emphasize also that the individualistic epistemologies which must be rejected pop up everywhere, in philosopher's talk about induction, confirmation/falsification, in psychological talk about cognition and language, even in sociological talk about science and in anthropological talk about classification. In all such instances, as we shall see, the theorist starts the account with the assumption that "social processes have already done their work and then pretends that they don't exist"!³⁵

NATURAL RATIONALITY

The strong program presupposes that humans have cognitive propensities, that they can learn. While this is a problem for experimental psychology, it will be important to see that, if the main conclusions of the strong program can be sustained, (1) not any sort of psychological theory will do and (2) there is a further problem which must be faced, whether or not the theory of natural rationality will be "tolerant" or "intolerant". Both problems stem from the same obvious fact, that no human being ever becomes a human being apart from some society (or other).

Let us consider the second issue first. "By an intolerant theory", Barnes writes, "I mean one which strictly specifies and limits kinds of inference which are naturally rational and hence the kinds of belief which are acceptable in the light of natural reason".³⁶ With an intolerant theory presumed, *differences* in modes of thought might then be *irrationalities* and there might be *no* "psychic unity" in mankind. By contrast, a tolerant theory would *not* strictly specify and limit kinds of naturally rational inference. Thus differences in modes of thought would be *cultural* differences and *no more than that*. Similarly, mankind would exhibit psychic unity such that the *same* cognitive propensities could issue in discernibly different culturally structured modes of thought. This is an *empirical matter* however difficult it may be to see how one might test the difference. Clearly, there are *methodological* advantages from the point of view of both empirical psychology and the strong program if we can presume a tolerant theory of natural rationality. Barnes notes that "sociologists and psychologists studying different cultures would then be justified in treating cultures as--equally problematic or equally puzzling".

Barnes rather cautiously says of the various existing accounts of natural rationality, that he finds the tolerant theories to be most convincing, but it seems to us that considerations from biology, comparative ethology, psychology and anthropology all strongly favor a tolerant theory. While this is not the place to make the effort to develop this, it seems to us that the burden of proof is on advocates of intolerant theories.³⁷

The problem of restrictions on the type of psychological theory suggested by the strong program is also complex, but we believe that the main lines of the issue can be adequately clarified. At least we here make that effort. We can begin by noticing what has often been noticed, that psychology has for a long time been burdened by assumptions deriving from philosophy. These are not merely the burdens of an untenable philosophy of science, though they relate to this: there are additional burdens deriving--if the strong program is correct--from anti-naturalism in philosophy of logic and individualism in the philosophy of language. Taken together these have been an unmitigated disaster for psychology. In what follows we try to show this by

showing how one problem from empirical psychology , the psychology of abstraction, relates to the idea of natural rationality, already introduced, and to Barry Barnes's "finitism". "Finitism" is a convenient term for a theory aimed at providing an understanding of concept-application. It involves, as we shall see, a theory of language which stands diametrically opposed to what Barnes has termed "extensional semantics".³⁸

"Finitism"³⁹

The concept of natural rationality is meant to imply that we are inductive- learning machines of some sort, that we have the capacities to learn and that in normal development each of us quite naturally acquires language and learns to use it. We readily make apt discriminations, we reason, we anticipate possibilities, etc. Barnes has argued that in verbally mediated learning- learning characteristic of human development-two types of processes can be employed: ostension and generalization. While as we shall suggest, we utterly lack understanding of *how* we learn ostensively, there can be little doubt that it is "an essential element in all verbally mediated learning. It is the ingredient which knots terms to the environment itself" (p. 306).

Generalization, then, the second main process connects terms together , e.g., "birds are egg-layers", "birds can fly", etc. Barnes writes that "like ostension, generalization is an essential ingredient in all verbally mediated learning. Generalizations provide expectations of experience. They are what make us regard a form of culture as a body of knowledge rather than a mere taxonomy" (p. 307). Again it seems that this can hardly be denied.

Barnes argues that these two processes are probably sufficient and this suggests, he says, that the knowledge of a competent member of a linguistic community can be modelled by a "Hesse net".⁴⁰ A "Hesse net" or a "net-work" is a metaphor for the idea that concepts are tied together by generalizations in various ways. Links between concepts are interwoven into a "fabric" in which concepts have many ties and in turn, are tied by many strands to other strands. As Barnes writes, "the net should be imagined extending out...involving more and more terms, until the entire conceptual resources possessed by the individual are included within it to comprise a connected whole" (p. 308).

Under concepts, then, stand a number of specific concrete instances of the concept. These Barnes calls the *tension* of the concept--in deliberate allusion to the "extension" of a term as that is used in current semantics and logical theory .The key point of difference must be emphasized: "In the tension of a term are included only past instances of use--a finite number of instances. To talk merely of the tension of a term is to accept that its future use is indeterminate" (p. 308) .

This may seem like a minor amendment to standard theories: It is not.

Concept application is not a social activity in the sense that it is determined by a *culturally given classification of reality, but a social activity which gives rise to and develops the pattern of that very classification. The pattern does not account for the activity; rather the activity accounts for the pattern (p. 310).*⁴¹

This difference is radical since if it is true, then *language is sui generis* and real only as incarnate in the practices of individuals. It thus is *incapable of being formulated as a closed System of rules*. This conclusion, we should add, is strongly supported by a host of evidence and argument from a very wide variety of quarters, from continental philosophy to post-Wittgensteinian philosophy.⁴² And, of course, it is central to the strong program. Moreover, lest we lose sight of one of the main conclusions of the strong program, between any two networks it is anything but self-evident that one and *not* the other provides an accurate or "true" classification of reality. As Barnes writes, "the clustering is something which we do to the particulars, not something which is already done in 'reality'" (p. 315). Second, the reasoning processes involved in learning a net "are involved whichever network is considered" (p. 316). But "this strongly suggests that different nets stand equivalently as far as the possibility of 'rational justification' is concerned... Accepted systems of classification are institutions which are socially sustained" (*ibid.*).

NATURALISM AND COGNITIVE PSYCHOLOGY

How then does the foregoing bear on the psychological theories of cognition? In particular how does it bear on the psychology of abstraction? It is quite clear that in the ostensive learning process (at least), abstraction is a fundamental process. Moreover, it is quite clear that this always operates in the context of "a pre-existing set of concepts and beliefs and (that) these cannot be explained or justified by the operation of natural rationality itself". As we noted earlier, the existing body of knowledge is always "material" for appropriation (and transformation) by learners. Do we then have any sort of psychological theory of abstraction which can square with these facts?

If David Kelly and Janet Kreuger are correct, we do not have *any sort* of adequate theory of abstraction, still less one which squares with the foregoing constraints.⁴³ And the reasons for this are not unrelated to the argument of the foregoing.⁴⁴

In the first place, Kelly and Kreuger show that the classical paradigm for experiments on concept-formation were mainly irrelevant since as Jerome Bruner has noted, they look at "concept attainment with the perceptual- abstraction phase bypassed" (p. 44). Second, the classical theories, which involve the logical notions of intension and extension, and the more recent "cluster" and "prototype" theories, founder totally on their inability to deal with genuine abstraction, either presupposing it as some stage in the

argument or leaving it wholly a mystery. Indeed, in addition to the genuinely intractable local problems in current theories, the standard philosopher's argument against the very idea of abstraction has force: "The observation that two objects are similar, it is argued ... will involve at least the concept SIMILARITY, which is just as abstract as any contents which abstraction is supposed to explain" (p. 63f.). This argument, in turn has called for *innatist* theories of which Fodor's is perhaps the clearest. We can quote Fodor's account of the main idea:

If learning a language is literally a matter of making and confirming hypotheses about the truth conditions associated with predicates, then [it] presupposes the ability to use expressions coextensive with each of the elementary predicates of the language being learned.

*But...the truth conditions associated with any predicate L [of some natural language] can be expressed in terms of the truth conditions associated with the elementary predicates of L. [Hence] one can learn what the semantic properties of a term are only if one already knows the language [i.e., a language not learned naturally] which contains a term having the same semantic properties.*⁴⁵

The relations between this argument and all the tendencies rejected by the strong program must here be emphasized. If this argument held, then the theory of meaning is entirely independent of any psychological considerations (anti-psychologism) since meanings are presupposed and depend upon either verification (positivist foundationism) or Platonism (meanings are reified concepts having an independent existence) or both (e.g., Platonism in logic and mathematics; verificationism otherwise). Moreover, the theory of meaning can proceed independently of any *sociological* considerations since language must stand in some straightforward "correspondence" to the world! Indeed, a vibrant form of epistemological individualism!

But Fodor's argument against the possibility of learning a genuinely new concept, viz., that "one can't learn [that is, acquire by a cognitive process] a conceptual system richer than the conceptual system that one starts with, where learning is a process of hypothesis formation and formation" *carries, if we allow assumptions shared by him and current abstraction theories. That is, as Kelly and Kreuger rightly argue, both parties assume that:*

the only relations between the contents of cognitive states which could make a process involving those states a cognitive process are the sorts of logical functions used in classical experiments. Logical functions hold between abstract predicates (p. 64, our emphasis).

And Kelly and Kreuger respond:

But suppose we reject this assumption. We could then consider the possibility that abstraction is a preconceptual process, involving non propositional modes of cognition

such as perception. It would not be a rational process, strictly speaking, since there are no logical relations, strictly speaking, among nonpropositional contents. But there can certainly be intelligible relations between them, and that is enough to earn abstraction the title of a cognitive process (p. 64) .

Kelly and Kreuger are surely correct in clinging to the idea that abstraction is a *cognitive process*, but we can see the perniciousness of anti-naturalism if in their otherwise incisive account they too defer to the idea that if learning is to be a *rational* process, it, strictly speaking, must be a process modelled on the propositional calculus! Logical relations do indeed hold between abstract predicates whether we consider this in the extensionalist terms of the readily available calculus of modern deductive logic or in the comparable terms of the unrealizable dream of a Carnapian inductive logic. But why should "rational process" even "strictly speaking", be so restricted?⁴⁶

As the strong program has *also* insisted, logic (and mathematics) must not be treated any differently than any other type of knowledge. We *do* have *natural rationality*. We *are* inductive learning machines, but as Barnes and Bloor have rightly insisted:

*No account of our biologically-based reasoning propensities will justify a unique system of logical conventions. Just as our experience of a shared material world does not itself guarantee shared verbal descriptions of it, so our shared natural rationality does not guarantee a unique logical system.*⁴⁷

That is, we put the cart before the horse if we suppose that *implication*, a logical concept deriving from the development of modern formal logic can account for *inference*, a psychological process. From a naturalistic point of view, implication is itself an *abstraction* made by us which *seems to us* to be psychologically compelling. Indeed,

Logic, as it is systematized in textbooks, monographs or research papers, is a learned body of scholarly lore, growing and varying over time. It is a mass of conventional routines, decisions, expedient restrictions, dicta, maxims, and ad hoc rules. The sheer lack of necessity in granting its assumptions or adopting its strange and elaborate definitions is the point that should strike any candid observer. Why should anyone adopt a notion of "implication" whereby a contradiction "implies" any proposition? What is compelling about systems of logic which require massive and systematic deviation from our everyday use of words like "if", "then", and "and"?⁴⁸

The basic upshot of this argument for a cognitive psychology we hope is clear. Anti-naturalism in the philosophy of logic and philosophy of language was an anti-sociologism, a refusal to press the full significance of the social character of knowledge. Instead, logic escaped to the transcendent, and language was unpacked in terms of a logical atomism, with verificationism as its point of connection with the world. And along the way, of [continued]

course, the earlier legacy of epistemological individualism was thereby saved. This is most graphic in the idea that "the intelligent organism is a sentential automation, whose behavior is the outcome of a sequence of mental states (beliefs that *p*, desire that *p*, etc.) and the processing will be described in terms of the *semantic* and *syntactic* relations among the content-specifying sentences". This idea which must be rejected--for reasons developed in the foregoing--is rejected also by Patricia Smith Churchland--for related but different reasons--in her spirited defense of an alternative paradigm, grounded in neuroscience.⁴⁹

Churchland argues in naturalist fashion that nature "abounds" in intelligent behavior. Moreover from the point of view of the strong program, we can clearly see how really bizarre is the idea that "where there is cognition, there also is linguistic representation, no matter how far, phylogenetically or ontogenetically, the creature is from overt language".⁵⁰ For Churchland, a more modest psychology of cognition would have more humble aspirations, recognizing at the outset that papers with such titles as "Command Neurons in Pleurobranchia Receive Synaptic Feedback from the Motor Network They Excite" are directly pertinent even if they seem very far removed from "the problems we face in figuring out how we perceive and how we can use language to represent the world". On the other hand,

A neurophysiological account of how an organism processes information will include hypotheses about what really is information contained in certain neuronal states at various levels from the periphery, what information is filtered in and filtered out, and how information is integrated. (p. 96)

This is not the place to pursue the many difficulties in what Churchland calls "a quaquaversal, or multi-directional approach" to the persistently intractable problems of how it is that intelligent organisms perform the genuinely miraculous feats they do perform. Nevertheless, we can notice that a theory of natural rationality is a theory of reasoning *propensities* or *capacities* and not an account of the full-fledged *competences* of human language-users.⁵¹ From the present point of view, it is the unnoticed influence of epistemological individualism which has led us to suppose that *individual psychology* by itself could provide any sort of adequate account of linguistic behavior, indeed, that by itself it could provide any sort of adequate account of human behavior, which because it is *human*, is socially constructed.

By the same token, it is the same epistemological individualism which has distorted so much of the explicitly *social* sciences which, as Bloor wrote, so often begin with the assumption that social processes have done their work and then promptly forget that they exist at all. We would like to end this already lengthy account with a sample of such distortions, from the sociology of science to the pure theory of consumer behavior, but perhaps it will be [continued]

here sufficient to emphasize what we see to be the basic source of its insidiousness and then to illustrate it with one brief example.

Why Epistemological Individualism?

In earlier pages we noted that it was an empirical question whether or to what extent there are relatively stable-and universal, elements in our (human) cognitive relations to the world. As seems not unlikely, given the relative species constancy of our neuropsychologically grounded reasoning capacities, there will be some stability and universality, at least at the level of un verbalized observation. But if so, this would allow for the persistent tendency in our culture (at least) towards forms of epistemological individualism. It would be easy for even sophisticated theory to confuse this fact (were it a fact) with "bridgeheads", "observation-sentences", etc. Indeed, in less refined and sophisticated forms, "the natural world" is understood to be essentially as it *appears* to be. Once embarked on this slippery slope, the next step is easily taken and easily goes unnoticed. Instead of comprehending nature "humanized", society is comprehended as "naturalized". Social facts have the same "necessity" as "natural" facts and as Marx so profoundly diagnosed, everything that happens, happens "naturally". Once we are epistemologically reduced to "spectators"--the metaphor, of course is Dewey's--we are easily seduced into supposing that the social world is as inhuman as are hurricanes. "Convention" may still be preserved, to be sure, but it is restricted to what is fully a matter of our conscious decisions and/or to what is "arbitrary". With this step we have the basis for a very strong doctrine of "human nature". Alternatively, with this step, it will be possible to admit that forms of social organization and society do vary, but to insist at the same time that these are to be understood in *our* terms, since our terms are *the* terms. Indeed, these differences are but unimportant variations of what we know to be true of *our* world, since of course, *our* world is *the* world. That such thinking is pervasive is hardly to be denied, but we would submit that it is pervasive among social scientists---half-hearted naturalists--and that it accounts for much of the furor-and persistent misunderstanding--of the strong program.

Our example is Bulmer's account of Karam taxonomy, an example used by Barnes. It is not our point to pick on Bulmer for we believe that he is not by any means a lonely minority among his colleagues in the social sciences. We think that the case is useful exactly because of Bulmer's anthropological sophistication.

Bulmer attempts to explain why, in the Karam taxonomy, the cassowary is placed in the "special" taxon "kobity", which is set apart from the taxon "yakt" of birds and bats. Bulmer writes:

At (the level of terminal taxa) Karam show an enormous, detailed and on the whole highly accurate knowledge of natural history, and...though, even with vertebrate animals, their terminal taxa only correspond well in about 60 per cent of cases with the species recognized by the scientific zoologist, they are nevertheless in general well aware of species differences among larger and more familiar creatures.

At the upper level of Karam taxonomy, however, objective biological facts no longer dominate the scene. They are still important, but they allow a far greater, almost infinitely varied, set of possibilities for the taxonomist. This is the level at which culture takes over and determines the selection of taxonomically significant characters.⁵²

The move is clear: Culture can explain what "nature" cannot. Bulmer simply assumes that our taxonomy--presumably it is *consistently* dominated by "objective and biological facts"--correctly corresponds to "nature" and differences in Karam taxonomy are *distortions* which can be "culturally" explained.

To repeat then: "The fact is that society gets into knowledge right at the ground floor, in the most elementary steps of language learning, and in the most elementary links that are forged between concepts and the world". A full-fledged naturalism will be willing to press for the full significance of this--however much it forces us to abandon what, for good historical reasons, seems to us to be painfully obvious.

*Peter T. Manicas and Alan Rosenberg
Department of Philosophy Queens College, CUNY*

NOTES

¹ Quine's characterization of "naturalism" is apt: "With Dewey I hold that knowledge, mind and meaning are part of the same world that they have to do with, and that they are to be studied in the same empirical spirit that animates natural science. There is no place for a prior philosophy" (W. V. Quine, "Ontological Relativity" (The John Dewey Lectures, March 1968), *Journal of Philosophy*, Vol. LXV, No.7 (April 4:, 1968) p. 185).

For an older and directly pertinent treatment of naturalism and the pertinence of the sociology of knowledge to it, see Thelma Z. Lavine, "Naturalism and the Sociological Analysis of Knowledge", in Y. H. Krikorian (ed.), *Naturalism and the Human Spirit*, Columbia University Press, New York and London, 194:4:) pp 183-209. Lavine points out that "half-hearted naturalism" ...is readily available in any age: extolling the consolidated achievements of empirical studies, it is engaged in protecting currently threatened phenomena against the encroachments of scientific investigation (p. 185).

The strong program is not "monistic" since, as we shall argue, it holds that there are different and non-reducible types of causes operating in the world, including, for the strong program, causes which "cooperate in bringing about belief".

² See Steven Shapin, "History of Science and Its Sociological Reconstructions", *History of Science*, Vol. XX (1982), p. 159.

³ A perusal of the critical literature will demonstrate that this is *usually* forgotten or at least that writers do not grasp the full significance of what it says. Larry Landau's "The Pseudo-Science of Science?" is an outstanding example. See the Symposium in *Philosophy of Social Science*, Vol. II (1981), pp 173-198. For a more subtle lapse, see Gerd Buchdahl's "Editorial Response to David Bloor", in another symposium on the strong program in *Studies in History and Philosophy of Science*, Vol. 13, No.4 (December 1982), pp 299-304 and Bloor's reply, *ibid.*, pp 305-311.

⁴ David Bloor, *Knowledge and Social Imagery*, London: Routledge and Kegan Paul, 1976. See also Barry Barnes, *Scientific Knowledge and Sociological Theory*, London: Routledge and Kegan Paul, 1974 and Barnes, *Interests and the Growth of Knowledge*, London: Routledge and Kegan Paul, 1977.

We should notice here that in their concrete work in the sociology of knowledge, Barnes, Bloor and their associates have made substantive theoretical claims in addition to those which define the program. For example, Barnes has appealed to "interests" as a key explanatory notion and has a distinctive theory of ideology; Bloor has advanced a Durkheimian theory which develops an analogy between religion and science, etc. These theses (and others which might be identified) are not under consideration in anything which follows in this essay.

⁵ In his first important book, *Scientific Knowledge and Sociological Theory* (1974) Barnes wrote, in his Epilogue, that he had "studiously avoided" issues in epistemology and ontology in his discussion. In that brief Epilogue he suggested that one might hold to a clear separation of questions of belief and questions of knowledge, thus making sociology of knowledge and epistemology mutually irrelevant. He said that "such a distinction cannot be argued against" (p. 153) and then promptly hinted at arguments against such a view. But they were hardly developed. Bloor was up front, however, in his *Knowledge and Social Imagery* (1976) and one might argue that the whole book is an essay in epistemology, in particular as a full-blown naturalized epistemology. On the other hand, he undoubtedly threw off a host of readers by saying on p. 2 that "the sociologist is concerned with knowledge, including scientific knowledge, purely as a natural phenomenon. His definition of knowledge will therefore be rather different from that of either the layman or the philosopher. Instead of defining it as true belief, knowledge for the sociologist consists of whatever men take to be knowledge". It *seemed*, accordingly, that he was making epistemology irrelevant (or largely so) for the sociologist.

⁶ See Barry Barnes's review-essay of Laudan's *Progress and Its Problems*, "Vicissitudes of Belief", *Social Studies of Science*, Vol. 9 (1979), pp 247-263.

⁷ A sample here might include Joseph Ben-David, "Organization, Social Control, and Cognitive Change in Science", in Ben-David and Clark (eds), *Culture and Its Creators*, Chicago: University of Chicago Press, 1977, pp 244-265; Robert Merton, *The Sociology of Science* Chicago: University of Chicago Press, 1973; Imre Lakatos, "History of Science and Its Rational Reconstruction", in R. C. Buck and R. Cohen (eds), *Boston Studies in the Philosophy of Science*, Dordrecht: Reidel, 1971, pp 91-136; Werner Stark, *The Sociology of Knowledge*, London: Routledge and Kegan Paul, 1958; Larry Laudan, *Progress and Its Problems: Towards a Theory of Scientific Growth* London: Routledge and Kegan Paul, 1977; Ernan McMullin, "The Goals of Natural Science", *Proceedings and Addresses of the American Philosophical Association*, Vol. 58, no.1 (September, 1984), pp 37-64.

⁸ See especially the series of papers by Michael Mulkey and G. Nigel Gilbert, including e.g., "Experiments are the Key: Participants' History and the Historians' Histories of Science", *Isis*. Vol. 75 (1984), pp 105-125 and references therein: Steve Woolgar, "Interests and Explanation in the Social Studies of Science", *Social Studies in Science*, Vol. II (1981), pp 365-394. For criticism of this view, see Steven Shapin, "Talking History: Reflections on Discourse Analysis", *Isis, op. cit.*, pp 125-130.

For some suggestions on Barnes's version of "realism" in history, see his "Vicissitudes of Belief".

⁹ Mary Hesse, *Revolutions and Reconstructions in the Philosophy of Science*: Bloomington, Indiana: Indiana University Press, 1980. Chapter 2, "The Strong Thesis of Sociology of Science", p. 37.

¹⁰ Hesse, *op. cit.*, p. 42.

¹¹ There are, of course, a variety of recent anti-foundationalist tendencies in recent philosophy, from hermeneutics to the similar "dialectical" approach of, e.g., Harold Brown in his useful *Perception, Theory and Commitment: The New Philosophy of Science*, Chicago: University of Chicago Press, 1977. Dewey, of course, had offered in his *Logic: The Theory of Inquiry* (New York: Henry Holt, 1938) a naturalized anti-foundationalist view of knowledge.

¹² Steven Lukes, "Relativism in its Place", in Martin Hollis and Steven Lukes (eds), *Rationality and Relativism* (Cambridge, Mass., the MIT Press, 1982, p. 262. This is an extremely useful volume and includes an essay by Barnes and Bloor, "Relativism, Rationalism and Sociology of Knowledge", and essays by Martin Hollis, Robin Horton and others.

¹³ As said by Paul Tibbetts in his review of Barnes's *Interests and the Growth of Knowledge, Philosophy of the Social Sciences*, Vol. 11, No. 4 (December, 1981), pp. 503-509.

¹⁴ Bloor ponders this issue in his 1975 review of Hesse's *The Structure of Scientific Inference*, "Epistemology or Psychology", *Studies in the History and Philosophy of Science*, Vol. 5, No.4 (February, 1975), pp. 382-395 and there suggests that in her book Hesse was ambivalent as to whether there was "a stable perceptual world". It was not clear where he himself stood on this. In *Knowledge and Social Imagery* (1976) he rather more confidently asserted that "the valuable insight of empiricism is its claim that our physiology ensures that some responses to our material environment are common and constant" (p. 26). In his 1982 essay, "Durkheim and Mauss Revisited: Classification and the Sociology of Knowledge", in *Studies in History and the Philosophy of Science*, Vol. 13, No.4 (December, 1982), he more cautiously said that "it does seem plausible that there are stable elements in our cognitive relation to the world and to locate these in our sensory motor experience and practical handling of the environment" (p. 279).

¹⁵ In *Scientific Knowledge and Sociological Theory*, Barnes remarks that many of the critical points which he had made in the first chapters of his book are "the products of idealist thought" and in this context he cites Peter Winch as advocating a thesis which is "very close" to his own. But he adds immediately that "it is important, at this point, to indicate why idealism must be rejected. ..Idealism is to be rejected because of its hostility to causality and determinism" (p. 70). Worth emphasizing here is that the issue regards not merely the necessity of appealing to sociological causation in the account of knowledge, but as well to psychological and physical causation, to, that is, a causal theory of perception. See our tentative remarks below.

As is perhaps well-known, Barnes has been greatly influenced by the work of Thomas Kuhn, but he recognizes that "Kuhn's choice of vocabulary ...can easily suggest a radically idealist ontology and a denial of a single shared physical environment" (*T. S. Kuhn and Social Science*, New York: Columbia University Press, 1982, p. 70). Barnes insists that "there are far too many references to an independent nature throughout his work as a whole for this to stand as a plausible interpretation of his views" (*ibid.*).

¹⁶ *Interests and the Growth of Knowledge*, p. 24.

¹⁷ Charles Taylor's contribution to *Rationality and Relativism*, pp. 87-105 is provocative as regards the question of the "superiority" of scientific rationality.

¹⁸ Larry Laudan, "The Pseudo-Science of Science?" *Philosophy of Social Science*, Vol. II (1981), p. 179f.

¹⁹ See Barnes, *T. S. Kuhn and Social Science*, pp 58-63; Barnes, "Vicissitudes of Belief"; Bloor, "Popper's Mystification of Objective Knowledge", *Science Studies*, Vol. 4 (1974), pp. 65-76; Bloor, "The Strengths of the Strong Programme", *Philosophy of Social Science*, Vol. II (1981), pp. 199-213, a response to Laudan, *op. cit.*

²⁰ Ernest Nagel, *The Structure of Science*, New York: Harcourt, Brace and World, 1961, p. 152.

²¹ This is quoted from Eugene P. Wigner, "Remarks on the Mind-Body Problem", in Wigner, *Symmetries and Reflections*, Bloomington: Indiana University Press, p. 174.

²² See Roy Bhaskar, *A Realist Theory of Science*, 2nd Edition, New York: Humanities Press, 1979. P. T. Manicas, "Reduction, Epigenesis and Explanation", *Journal for the Theory of Social Behavior*, Vol. 13, No.3 (October, 1983), pp. 321-354.

²³ We should add at once that here, as in the foregoing, we are thinking only of what may be called *theoretical science*, (fundamental science) as that has developed from the 17th Century in the West, first in mechanics and then in the complex structural natural sciences that have taken shape since then. For a brief sketch, see E. McMullin, "The Goals of Natural Science", *op. tit.* With the proliferation of "sciences", especially in this century, from the applied disciplines of psychology, engineering, etc., to "computer science", we would need a far richer and more complex schema. In many of these, of course, "prediction and control" are the uppermost (exclusive) goals.

²⁴ Quantum mechanics is undoubtedly the optimum case for an instrumentalist conception of theory. But as Bernard D'Espagnat and more recently, J. C. Polkinghorne have argued, even here one must accept realism if one is to make sense of what it is that particle physicists *do*. See Espagnat, *Conceptual foundations of Quantum Mechanics*, California: Benjamin, 1971; Polkinghorne, *The Quantum World*, London: Longman, 1984. Polkinghorne remarks, "I have never known anyone working in fundamental science who was not motivated by the desire to understand the way the world is" (p. 79).

²⁵ *Knowledge and Social Imagery*, p. 32.

²⁶ *Ibid.*, p. 35. Bloor is not clear here. There are two points. First, in these pages he writes of a "pragmatic and instrumentalist notion of correspondence" which is *not* a correspondence of the theory with reality but the "correspondence of the theory with itself". It is indeed a criterion of *coherence*. This sounds much like a pragmatic theory of truth and an instrumentalist conception of theory. Second, in the sentence, "it should be possible to see theories entirely as conventional instruments for coping with and adapting to our environment", the emphasis should be on "instrumental" and not on "conventional". For the strong program, all theories are *conventional* in the very clear sense that they are *human creations*, sustained by "conventions". This is hardly the issue here. Finally, then, for the same reason, *everything* which is humanly created and sustained is in some sense "an instrument for coping with and adapting to our environment. Accordingly, *that* would not seem to be the key point. As far as we can tell, e.g., general relativity has *no practical* consequences. Does it help us "cope" or "adapt"? If we include under that heading its capacity to help resolve some *theoretical* problems regarding the perihelion of mercury, it does. But surely, at this point we have lost all sense to the distinction. One of the authors, Rosenberg, would argue that Barnes and Bloor attempt to overcome the distinction between a realist conception of theory and an instrumentalist one. The other author, Manicas, holds that this seems to put him in league with Nagel (above) and rob the program of what is vital to it: viz., the claim that there are underlying causal mechanisms which can be appealed to for explanations.

²⁷ Barnes, *Interests and the Growth of Knowledge*, p. 10, our emphasis.

²⁸ Barnes, *T. S. Kuhn and Social Science*, p. 82.

²⁹ Barnes, *Interests and the Growth of Knowledge*, p. 22. 30 Barnes, *T. S. Kuhn and Social Sciences*, p. 80.

³¹ In his brilliant reading of Wittgenstein, Bloor comments that "everyone accepts that human beings are social animals and that knowledge is, in some sense, a collective achievement, but it is all too easy to miss the full significance of these facts and to trivialize their implications" (*Wittgenstein: A Social Theory of Knowledge*, New York, Columbia University Press, 1983, p. 2).

³² Bloor, "Strengths of the Strong Programme", p. 207; See also Barnes's very important essay "Natural Rationality: A Neglected Concept in the Social Sciences", *Philosophy of the Social Sciences*, Vol. 6 (1976), pp. 115-126.

³³ W. V. Quine, "Epistemology Naturalized", in Quine, *Ontological Relativity and Other Essays*, New York: Columbia University Press, 1969, p. 84.

³⁴ Bloor, "A Reply to Gerd Buchdahl", *Studies in History and Philosophy of Science*, Vol. 13, No.4 (December, 1982), p. 305.

³⁵ Bloor, "Strengths of the Strong Programme", p. 202. ³⁶ Barnes, "Natural Rationality", p. 120.

³⁷ Cf. Melvin Konner, *The Tangled Wing; Biological Constraints on the Human Spirit*, New York: Holt, Rinehard and Winston, 1982; Manicas, "Reduction, Epigenesis and Explanation".

³⁸ Barnes notes that "finitist" approaches derive from Wittgenstein and cites a variety of writers, in sociology, anthropology, ethnomethodology and philosophy who, influenced by Wittgenstein, are finitists. These include H. Garfinkel, *Studies in Ethnomethodology* (1967); Bloor, *Knowledge and Social Imagery* (1973); Mary Douglas, *Rules and Meanings* (1973); Mary Hesse, *The Structure of Scientific Inference* (1974); D. L. Phillips, *Wittgenstein and Scientific Knowledge* (1977).

³⁹ The following is a summary of the key ideas of Barnes's important but neglected essay, "On the Conventional Character of Knowledge and Cognition", *Philosophy of Social Sciences*, Vol. 11 (1981), pp. 303-333. See also T. S. Kuhn and Social Science, Chapter 2; Barry Barnes and John Law, "Whatever Should be Done with Indexical Expressions"? *Theory and Society*, Vol. 3, No.2 (Summer 1976), pp. 223-238; Barnes, "On the extension of concepts and the growth of knowledge", *Sociological Review*, Vol. 30, No.1 (February 1982), pp. 23- 44.

Bloor has put these ideas to good use in his "Durkheim and Mauss Revisited: Classification and the Sociology of Knowledge", *Studies in History and Philosophy of Science*, Vol. 13, No. 4 (December 1982), pp. 267-297.

⁴⁰ From Mary Hesse. See her *The Structure of Scientific Inference*. Hesse notes that her work was influenced by Duhem and Quine. See also her comments to Bloor's "Durkheim and Mauss Revisited", *op. cit.*, pp. 325-331.

⁴¹ The idea that the activity accounts for the pattern *and not* conversely is, of course, a key theme of Anthony Giddens's concept of "structuration". See his *New Rules of Sociological Method*, London: Hutchinson, 1976; *Central Problems in Social Theory*, Berkeley and Los Angeles: University of California Press, 1979 and from the same press, *Profiles and Critiques in Social Theory*, 1982, Chapters 1-3.

⁴² It is the central critical theme of Joseph Margolis's indispensable, *The Philosophy of Psychology*, Englewood Cliffs, N.J.: Prentice-Hall, 1984. See also his "Relativism, History and Objectivity in the Human Sciences", *Journal for the Theory of Social Behavior*, Vol. 14, No. I (March, 1984), pp. 1-24.

⁴³ David Kelly and Janet Kreuger, "The Psychology of Abstraction", *Journal for the Theory of Social Behavior*, *ibid.*, pp. 43-68. Aaron Ben-Zeev's essay in the same issue, "The Kantian Revolution in Perception" is, as we shall see, also pertinent and useful.

⁴⁴ Both Kelly and Kreuger and Ben-Zeev rightly see the roots of the current dilemmas in classical epistemology. Empiricisms and their Kantian alternative aimed to close "the gap between the meaningless and unorganized...stuff which exists independently of the agent, and the meaningful organized world in which the agent lives" (Ben-Zeev, p. 69). For the empiricists, the problem was one of understanding how abstract concepts could be formed from the observation of particulars. Kant, of course, argued that the (empirical) subject did not encounter "the meaningless stuff", since it was already organized by contributions from the (transcendental) subject. Both sorts of theories unhesitatingly offered speculative *psychological* theories (associationism, discrimination of faculties, etc.) as part of

their epistemological problem, a fact of considerable importance for the development of subsequent philosophy *and* psychology. But indeed, the key disaster was the complete absence of *any* genuinely *social* component in this development.

⁴⁵ This is conveniently quoted from Margolis's incisive treatment in *The Philosophy of Psychology*, p. 79.

⁴⁶ It is too often supposed that "foundationism" in epistemology demands only secure foundations - premises. As important, it has also always demanded that logical relations between abstract predicates hold. The absence of an inductive *logic* is the skeleton in the closet of empiricisms from Hume to Carnap to Goodman to Popper--who at least saw the problem!

⁴⁷ Barnes and Bloor, "Rationalism and the Sociology of Knowledge", in Hollis and Lukes, *Rationality and Relativism*, p. 44. The naturalizing of logic and mathematics has been the special concern of Bloor's work. See his *Knowledge and Social Imagery*, Chapters 5-7; *Wittgenstein: A Social Theory of Knowledge*, Chapters 5 and 6. See also the exchange between Gad Freudenthal and Angus Gellatly in *Studies in History and Philosophy of Science* Vol. 10, No.1, (1979), pp. 67--84 and Vol. II, No.4 (1980), pp. 325-339. Gellatly's useful paper, "Logical Necessity and the Strong Programme for the Sociology of Knowledge" will be of special interest to psychologists.

⁴⁸ "Rationalism and the Sociology of Knowledge", p. 45.

⁴⁹ Patricia Smith Churchland, "A Perspective on Mind- Brain Research", *The Journal of Philosophy*, Vol. LXXVII, No.4 (April, 1980), pp. 185-207, from which the foregoing text is quoted.

⁵⁰ *Ibid.*, p. 189.

⁵¹ See P. T. Manicas, "The Human Sciences: A Radical Separation of Psychology and the Social Sciences", in P. F. Secord (ed.), *Explaining Human Behavior*, Beverly Hills, CA.: Sage, 1982, pp. 155-173; P. T. Manicas and P. F. Secord, "Implications for Psychology of the New Philosophy of Science", *American Psychologist*, Vol. 38, No.4 (April, 1983), pp. 399- 413.

⁵² Quoted from Barnes, "On the Conventional Character of Knowledge and Cognition", p. 317, from R. Bulmer, "Why is the Cassowary not a Bird?" *Man*, 2 (1967), pp. 5-25.