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The Sociology of Scientific Knowledge: Can We Ever Get It Straight?

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One would have to search for a long time to find a more startling body of interpretation than the treatment of the work of Barry Barnes and David Bloor, initiators of the 'strong programme' in the sociology of knowledge, Puzzled by this, we offered a sympathetic review of their writings and an appraisal of various criticism of these (Manicas and Rosenberg, 1985), Since then an impressive amount of new work with this orientation has been published which includes an ever-widening spectrum in what, to better reflect this variety of interest and orientation, is better entitled the 'sociology of scientific knowledge' (SSK). Throughout, attacks on Barnes and Bloor and other variations of SSK have continued, while a number of excellent books have appeared which are concerned with the same issues although not specifically focused on Barnes and Bloor. If anything the interpretations have proliferated; even more remarkably, we can now find very good writers who reject the strong programme for precisely opposite reasons! In what follows, we make another effort at clarification, introducing these new materials and attempting to advance the argument from where we left it in 1985.

EPISTEMOLOGICAL INDIVIDUALISM

Surely the most widespread criticism of strong programme sociology of science comes from what we called an 'epistemological individualist' point of view. In its most bald form, it holds that there are beliefs for which social causes are wholly irrelevant. Thus, one can, presumably, contrast the belief that the Virgin Mary miraculously appeared to a person now sainted, an admittedly socially grounded belief, with the belief of person standing in front of Great Panda who says, 'I see a Panda now', Flew asserts, impatiently, that if Bloor thinks that 'intrusive non-social, physiological, and biological facts' arc not sufficient to explain this latter belief, then his view is 'manifestly preposterous and in its implications, catastrophically obscurantist'.[1] He continues, 'perhaps the most elegant way to justify so vehement a dismissal is to point out that the causal efficacy of the object perceived is a logically necessary condition of the occurrence of the perception' (Flew, 1982: 366-67).

Putting aside any issues regarding the special problems of historical knowledge (as in the case of the Saint's vision) or of some blind man's claim that he *sees* a Panda, we can assent that our man's belief that he sees a Panda cannot be causally explained without *something* being present. But the critical and entirely overlooked point is that this something is *conventionally* a Panda. Thus also what we called 'conventional realism'. As Bloor has said, 'the fact is that society gets into knowledge right at the ground floor, in the most elementary steps in language learning, and in the most elementary links that are forged between concepts and the world' (Bloor, 1982: 305).

Flew's formulation is interesting because at least he does explicitly put perception into a causal context even if, for him, there would seem to be 'facts' and somehow we see them! But Flew's discussion is interesting also because it was criticized on just this score by Triplett (1986) who though apparently sympathetic to strong programme claims regarding perceptual claims, was bothered by the idea that mathematical beliefs also fell into the net of sociological explanation.

Triplett accepts Flew's idea that there are different sorts of explanation 'which may in different contexts be meant when "the" explanation for a belief is called for'. In particular, there is a difference between (1) an 'evidential explanation', an explanation which, roughly, is 'rationally' held, and (2) varieties of causal explanations: physiological, psychological or sociological. Triplett asserts that Bloor is never clear on exactly what is to be the role of sociology of knowledge. This objection is widespread. Is sociology of knowledge 'imperialist'? Does it claim to absorb physiology, psychology, and even epistemology? (Restivo, 1980; Grove, 1982 Hirst, 1984; Skorupski, 1985; Trigg, 1985; Oldroyd, 1986; Hekman, 1986; Harré, 1986; Richard, 1987).

IMPERIALISM

In his incisive refutation of Laudan's 'arationality' assumption, Richard Jennings (1984) raises the problem in an interesting but possibly misleading way. Laudan's 'arationality' assumption, a key symptom of the epistemological individualist, goes as follows: '. ..the sociologist of knowledge may step in to explain beliefs if and only if those beliefs cannot be explained in terms of their rational merits' (Laudan, 1977: 202, italics omitted). Jennings argues that Laudan's arguments for this view fail and that the consequences which Laudan draws from his postulates are 'absurd', so absurd, indeed, that one is drawn to the conclusion (neatly turning the tables!) that what best explains Laudan's effort is 'simply the idea of academic territoriality, or professional boundary maintenance' Jennings, 1984:208). According to Jennings, not only is Laudan's view of what counts as rational presumptive, but his insistence that 'the historical sociologist of knowledge must show, for any given episode he wishes to explain, that it is incapable of being explained in terms of rational, intellectual history' (Laudan, *1977:* 209) puts a burden on the sociologist which he could not *possibly* fulfill!

For Jennings, 'there is no hard and fast account of rationality, true for all times and places'. This surely sounds like a full-fledged endorsement of the strong programme. But since Jennings holds that the work of (say) Laudan and David Bloor are complementary and not competing, we are not clear that this is so. Moreover, his effort to articulate a non-imperialist role for 'cognitive' sociology seems to miss. This is best clarified by considering Jennings's treatment of Laudan's imaginary example of two inquirers seeking to explain some belief *p* held by *x*, one seeking to explain the belief 'in terms of its being well-founded on other beliefs held by that person', the other 'based on the social/psychological background of the believer'. Jennings argues that there is no conflict since the latter account does not explain how *x* came to believe *p*. That is, 'the story that the cognitive sociologist tells is not a story of a black box in which various psychological and sociological forces play, and out of which ultimately comes a belief, rather he tells how these forces condition the other beliefs that *x* holds, how connections between various beliefs are strengthened or weakened as a result of acquiring new beliefs and interests' (Jennings, 1984: 205),

This is at best confusing. The individual is not a black box in the sense that he/she is at any time void of beliefs in terms of which he/she carries on; but Jennings's formulation suggests both a rather impoverished view of sociology of knowledge and a misconstrual of how the 'philosopher' defines his/her task. Surely, Laudan will not be pleased to know that his proper task is critical biography: consideration of 'the logic or rationality of the story that the actor tells us, taking into account all the various factors that the actor considers relevant' (211), Indeed, as we suggest below, considerably more is at stake and we agree with Laudan that conflict between SSK inquiry and rationalist intellectual history is quite inevitable. It is thus that Jennings's reconcilation is too easy. Similarly while Jennings raises the problem of the relative role and relation of the 'various psychological and sociological forces' in fixing individual belief, he does not provide a satisfactory answer to the question.

It is presumably not Jennings's intention to assert, as per Flew, that there are beliefs which are not 'conditioned' by 'various psychological and sociological forces'. That is, we arc not to believe, presumably, that there is a time in which a person has beliefs which are not the product of various psychological *and* sociological factors. But what then is meant by saying that the cognitive sociologist 'tells how these forces condition the *other* beliefs that *x* holds'? Similarly what is intended by the claim that he tells us 'how connections between these various beliefs are strengthened or weakened as a result of acquiring new beliefs and interests' -as if there were some 'original' set of belief regarding which sociology had no role to play?

Other writers are bothered by problems related to those which bother Jennings. Layder (1984) finds Barnes guilty of the sort of sociologism characteristic or earlier structural-functionalism. For him, Barnes offers a 'dehumanizing analysis…which relies on an image of human beings as "inductive learning machine's" ...and social life in general as "bootstrapped induction"' (1984: 403), For him, Barnes's analysis lacks 'substantial people with real feelings, motivations, objectives, etc.' and 'smacks or a kind of' mechanistic conception of human beings'. Rudwick (1981: 250), sympathetic to strong programme inquiry and evidently not bothered by Barnes's metaphors, sees an 'anti-individualist tendency' in which the 'personal dimension' is 'devalued until it is almost out or sight'. For him, too little attention is paid to 'the reality of personal purposes in scientific work'. More generally, then, is SSK inquiry a sociological reductionism?

It seems clear enough that individuals creatively appropriate social forms, and, in turn, that these distinct idiosyncratic orientations (including particular aggregations of beliefs and interests) play a critical role in explaining what becomes consensually sustained belief. That is, personal aims, idiosyncratic perceptions and features or personality -more generally psychological considerations -are pertinent to the sociology or knowledge. But it seems to us that not only is this manifestly clear in programmatic statements regarding SSK inquiry, but even a casual look at the actual work being done shows that such considerations are nowhere missing.[2]

On the other hand, we believe that strong programme methodology would profit from a firmer incorporation of Giddens' notion or 'structuration'. That is, if inquiry into knowledge-producing practices is not to risk the usual errors of *either* structuralism *or* of psychologistic, action-oriented, interpretative sociology, then an agency/structure *duality* must be firmly acknowledged. Thus while intentioned, idiosyncratic individuals are always the causal agents who reproduce *and* transform relatively enduring social forms, including therefore *mentalités.* 'All action exists in continuity with the past, which supplies the means of its initiation' (Giddens, 1979: 70)

This view makes all human activity social, but there is ample place for individual initiative, creativity, motivation and interests, just as there is ample room for acknowledging the greatly different access among persons to social resources, including, especially, power.[3] But to repeat, as we read the concrete work of writers associated with the new sociology of scientific knowledge, we find such a view at least implicit.

These considerations do not, however, touch the question, hinted at by Triplett and surely the red-flag issue for Laudan and most 'philosophers', of whether sociology of knowledge absorbs not merely psychology, but *epistemology.* Still, because the relation of psychology to epistemology remains unclear and contested, more needs to be said regarding the question of psychology .

PSYCHOLOGY, LOGIC AND PHILOSOPHY OF LANGUAGE

We believe that monumental misunderstanding stems from widespread misunderstanding on the role of physiology and psychology in matters of belief. This was, indeed, a main claim of our previous paper. Without here attempting to extend that account, we can say that the problem, incredible on its face, is the persistent tendency in the psychological sciences to take for granted exactly that which most needs explaining (Manicas, 1986) Thus, 'learning theory' happily takes for granted that learners *see* pandas, and the theory of perception, like recent 'cognitive psychology', assumes that 'information'--facts?- -come prepackaged for use by a psychologically autonomous subject (a 'sentential automaton'). At the bottom of this, we offered, were assumptions derived from philosophy, but especially the assumption that if the process is 'cognitive', then the only relations between the contents of cognitive states are *logical.* But as if this were not sufficiently presumptive, since logical functions hold between *abstract predicates,* we are then bound to a philosophy of language which is inescapably either Platonist or verificationist (Manicas, 1987).

Barnes and Bloor have taken different pieces of this. Barnes's 'finitism' is an attack on customary extensional semantics (Barnes, 1983); Bloor has written extensively against 'rationalist' (anti-naturalist) conceptions of logic and mathematics (Bloor, 1976). Since if they are correct, the entire program of 20th century 'analytic' philosophy is radically misconceived, these views have, of course, outraged mainstream philosophers. But they do *not* imply, as some have supposed, that psychology gives way to sociology. Put briefly, we are inductive learning machines of some sort, have the capacities to learn and, in normal development, we acquire a language along with a host of other skills and characteristics (Barnes, 1981). How we do this seems to us to be the fundamental problem of (more) experimental psychology. But, given the constraints imposed by our species-specific human biology *and* by the nature of the external world, explaining *what* is learned requires sociology. That is, causal inputs from the external world surely do have effects on what we believe: There is something before me when, in normal conditions, I assert 'I see a Panda' just as when I assert 'I see the Virgin Mary'. And the *concept* 'Panda' is as much a social product as the concept 'the Virgin Mary'. No one would deny, we take it, that this story is frightfully complicated. But if so, then we cannot help but wonder what motivates the shrill voices raised against SSK inquiry? Part of the explanation for this is suggested by our deliberatively provocative formulation: 'given the constraints of our species-specific biology and by the nature of the external world, what we learn cannot be answered apart from sociology'. The formulation implies both that psychology alone is incapable of solving the problem of belief: but also that it cannot be disconnected from physiology. But more than this, it raised the question of realism and its relation to the relativism of the strong programme.

REALISM AND RELATIVISM

Following a suggestion by Barnes (1987), we distinguish 'double-barreled realism' and 'single-barreled realism' ('residual', 'modest', 'minimal'" 'conventional' realism). We define these as follows: The doubled-barreled realist holds that the external world 'has its character independently of any knowledge or experience of it, *so that* sentences about the world are either determinately true or false' (Grayling, 1982: 233). The single- barreled realist refuses the second clause, the one which begins with the 'so that'. The double-barreled realist *cannot* be a relativist (Trigg, 1985: 22). The single-barreled realist is (usually) a relativist since he (she) believes that there is no *one* set of sentences which *can be shown to be uniquely true of that world.* As Barnes writes, 'it is not the lack of an external world which leads to the equivalence of different bodies of knowledge, but the silence of that world in the face of alternative accounts of it, its evenhanded indifference, its tolerance' (1987: 5) .The single-barreled realist is Kantian, however, in holding that the external world (the world independent of all possible experience) *has effects on us.* It is a world to which, as Barnes says, 'we are, as it were, causally connected'.

The point is critical. On the one hand, it is just this which distinguishes strong programme sociology of knowledge from varieties of idealism (explicit or incipient). By virtue of strong programme realism, there are definite if indeterminate limits on the belief-systems of human communities. Moreover and with greater problems, the postulate or a casually (more)

effacious reality provides a bite into offering some kind of rationale for strong programme commitments. We return to this in the final sections of our essay.
The postulate of such a world provides limits in just the sense that while there can be an enormous variety in indifferently 'true' belief systems, each must allow that community to reproduce itself. 'Because we live in one world we all see the bird flying by at the same time. Because they live in that world Karam all see Yakt flying by at the same time'.

The point may seem unimportant since--and this must be emphasized --the 'postulate' of an knowledge-independent external world is not sufficient to allow us to determine the truth (or falsity) of bodies of belief held by existing (or historically enduring) human communities, between say Karam taxonomy and the scientific taxonomy of Western biology. Thus, the symmetry tenet.[4]

There is, then, a form of realism which is consistent with relativism (and may indeed, *demand* relativism). This point is persistently missed. Thus when Barnes and Bloor are read as saying that 'the source of all our judgments lies in society, rather than reality', Trigg is led to conclude that 'reality *thus* drops out of the picture and, so far from acting as a constraint on the content of our theories, is totally forgotten' (1978:274, our emphasis) .Of course, reality drops out as *epistemically* irrelevant, but, to repeat, it does not drop out as causally relevant. On the other hand, if by 'constraint on our theories', Trigg means that we are in a position to disentangle that element of belief which is not socially infected (Harré, 1986: 14) then, short of adopting some form double-barrelled realism, it is hard for us to see how this is possible. In an attempt to escape 'the mess of relativism', Harré has recently made the effort; it will pay us to look, if briefly, at his complicated argument.

Harré rejects what he calls 'truth realism', or any realism which depends upon the truth of sentences. As Harré uses the term, 'truth realism' seems to us to refer to any double-barreled realism. Instead, Harré opts for what he calls 'referential realism'. The defense of referential realism is complex and involves several stages, beginning with a careful development of a theory of reference in which 'terms denote, but people refer'. Accordingly, 'it is possible successfully to refer to something by the use of a descriptive phrase which is not actually appropriate to the being in question' (1987: 97). Harré's theory, we believe, is consistent with Barnes's finitism. The second stage is more directly 'realist' and involves a rejection of a Kantian interpretation of space and time. Its upshot is 'in effect to reduce the metaphysics of space-time to the logical grammar of indexicality, the properties of 'here', 'now', 'this' and so on.' This allows that 'all other referential acts but those that tie existence to the immediate physical neighborhood of token-reflexive utterances are tinged with theory, and in that measure speculative' (141). This implies that, so far, 'referential realism' is consistent with a relativism.

It is the next step which, for Harré, precludes relativism. His strategy is to foreclose a skeptical gap in perception by rejecting 'representationalism', neatly summarized by the formula: O (object) causes S (sensation) which is interpreted (non-inferentially) as p (percept). It is, he argues, just this which is challenged by J J .Gibson's theory of perception. This is plainly no place to raise questions about Gibson's theory. On Harré's version, the critical point is that perception is direct, analysed in terms of 'affordances', or specific bounded dispositions determined by the specific sensory mechanisms of living things standing in complementary relations to mechanisms in the physical world. Patterns or energy exist independently of systems that may receive it. But patterns are information only insofar as they have significance for receivers, only as they stand in specific nomic relations to a certain kind of living thing.

Because these points are perfectly general, they apply to human perception. But it is critical to notice that as regards the human animal there are some troublesome complications. In particular, because humans are social beings, concepts and beliefs which are cultural products infect human perception. Accordingly, as Harré writes, the world which we *directly* perceive is 'coarse-grained'. It answers closely to Kantian schemata, 'things', events, spatial, temporal and causal relations. Evolution, we may believe, equipped us with brains and sensory systems. It had to give us discriminative and comparative capacities, the *basis* of judging, predicating or subsuming. It could, contrary to the entire representative tradition, give us direct perception of orderly patterns in the external world. But it could not give us *concepts,* still less *veridical* concepts, concepts true of the world which exists independently of us.

Gibsonian theory (for us and for Harré), yields no 'recognizably corrigible statements'. But, and this is where our difficulties begin, Harré also asserts that 'without some basis in veridical perception scientific realism, whether it be based on 'truth' [which he rejects] or upon 'reference' [which he affirms] must founder in a mess of relativism' (161).

We are unclear about this 'mess of relativism'. First, even if Gibsonian perception gives us direct access to the external world (and thus guarantees communication), this is no epistemological 'bridgehead' exactly because there are no incorrigibles. Human percepts are 'blind' with respect to linguistically articulated knowledge.

Second, Harré acknowledges that he seeks not 'foundations' but *grounds* and that these are found, not in statements, but in material practices. Harré assents that the 'epistemic access' which Gibsonian theory yields cannot discriminate between alternative epistemic practices. As he says, there are convincing arguments against the attempt to provide a (more) selectionist-adaptationist account of the conventional practices of particular ways of 'finding out' even if there are grounds for giving a biological account of our capacity to employ the 'generic' categorial scheme which Kant had mistakenly located in the transcendental ego (Harré: 161-164).

Indeed, as Harré's provocative account of the *moral* order which sustains science suggests, it seems to us that the problem is not so much relativism, but the desire to 'ground' those material practices which Harré analyses in his important book, That is, it seems to us that the problem which Harré has addressed, and to which we return, is not so much the problem of relativism (as that is often construed), but the problem of why, against alternatives, anyone should prefer those practices which we call science. The point plainly bears on strong programme commitments. Indeed, it raises the question of the fourth 'tenet' of the strong program, that their inquiry is 'reflexive'. Similarly, it raises the question, so frequently adduced against them, as to why, given their programme, anyone should believe them! But before we treat these issues directly, it may be useful to consider, if briefly, some pertinent debate on 'realism' *within* the sociology or scientific knowledge camp.

REALISM AND SSK INQUIRY

While some SSK writers (Woolgar, 1976, 1981, 1987; H.M. Collins, 1985; Latour and Woolgar , 1986) arc easily enough read as holding that realism (in any sense) is a non-issue, they too seem to hold to at least the minimal realism here identified, Thus, 'reality', Latour writes, 'as the Latin word *res* indicates, is what *resists.* What docs it resist? *Trials of strength.* If, in a given situation, no dissenter is able to modify the shape of a new object, then that's it. It *is* reality, at least for as long as the trials or strength are not modified' (Latour, 1987: 93). The 'it' is reality but what *we* think is real is 'it' *after* there is agreement *that* it is and that is a so-and-so. The point applies as much to viruses as to pandas.

Nevertheless, it is easy to see how confusion has entered. Harry Collins, for example, is puzzled that Hesse and Barnes wish 'to reserve more or their explanations or knowledge for the fundamental "physics and physiology" of situations' than he finds necessary (Collins, 1985: 172). He is surely correct that if they are single-barreled realists, there is no avoiding conventionalism (social contructionism). Now while he agrees with Hesse (and with Barnes) that 'wholesale change' in a network or belief is 'impossible', he also says that the source of the continuity --more properly, locally perceived continuity--is not physics and physiology…but interests and social conventions' (173). But one would like to know how he knows this? For Barnes and Bloor, this would seem to be an open question, to be decided by inquiry. It may be e.g., that the most plausible account which we can give will lead us to the conclusion that the continuity of belief, like the limits on belief, are owed far more to our species-specific biology than idealist-tending sociological views usually suggest. On the other hand, Collins would seem to be corrcct in holding that even if in some 'basic sense' the physics of the world and the physiology of humans is a source of the continuity (and/or 'constraint' on belief), *for purposes of sociological inquiry,* 'the only sensible way to proceed is to discover what the limits on perception arc, in the world we live in *now'* (173).

A perfect problem for such inquiry and another locus for what seems to be differences between SSK writers regards treatment of the experimental situation. Latour and Woolgar (1986) and Collins (1985) have been critical of Bhaskar's realist view that experiment can give us any sort or access to underlying realities. But Pickering (1987) seems closer to Bhaskar in arguing that scientific knowledge is articulated in accommodation to resistances arising in the material world'. There is, he continues, .a direct and analysable relation between scientific knowledge and the material world, though it is one of made coherence, not natural correspondence'. As his reference to 'made coherence' suggests, Pickering is a single-barreled realist and he would agree with Latour, Woolgar and Collins that not only is experimental closure negotiated, but that the possibilities available for undermining consensus on closure are literally endless. This is, of course, exactly the upshot of Barnes's finitism. That is, the term 'quark' is like 'Yakt' in that people control their use. This means that whether or not an experimental design is quark-producing is a decision subject to an extremely large (and perhaps non-denumerable) number of factors.[5]

It is, of course, perfectly clear that there is wide chasm between SSK inquirers and all realisms which postulate not only an external world, but assume that it is self-identifying. (In weaker Peircean and Habermasian forms, it becomes identified after certain ideal conditions become fulfilled). On the other hand, it not easy to judge between SSK writers what are the consequences of their differences on realism and experimentation in science. We believe that the most critical difference between them (and some others) regards not this, but what they respectively take *science* to be and then, even more importantly, what sort of defense they are prepared to give to it as a mode of fixing belief.

EPISTEMOLOGY, EVIDENTIAL EXPLANATION AND HERMENEUTICS

Why not call the strong programme a sociology of belief instead of a sociology of knowledge (Steward: 1987: 215)? We think that Barnes and Bloor have suggested that this redefinition would not burden the program in the least, To this extent, then, they have encouraged the idea that if the strong program is irrelevant to epistemology, it cannot be a threat to it (see also, Berger and Luckman, 1967: 14), Unfortunately (or fortunately?), things are not so simple.

While the foregoing discussion suggests that the SSK and (traditional) epistemology are strongly in tension (to say the least), we can approach this by picking up on the contrast, already introduced, between 'evidential explanation' and 'casual explanation'. Laudan (1977), e.g., argues that they are incompatible; Jennings (1984), e.g., offers that they arc complementary, Schmauss suggests that 'if we allow a person's reasons for holding a belief to be included among the causes of that belief, the tenents of Bloor's program are rendered uncontroversial' (1985: 189). That is, the problem dissolves. What is going on here?

If we take 'evidential explanation' (as we must?) to assume some Hempelian model of rationality in which we have standard deductive logic, some parallel (and imaginary syntactic inductive logic and some access to (indubitable or at least unproblematically true) evidence sentences, then we have the model for standard epistemology. To be rational is to proceed logically from evidence to belief. Science is the rational enterprise par excellence; thus the program of so much of 20th-century 'reconstructionist' philosophy of science.

We should notice that whether or not reasons are causes, there is nothing in the strong programme to prevent the explanation of some belief as *thus* rational. But this is not interesting. What is interesting is the fact that from the point of view of the strong programme: (1) Such a belief is not thereby absolutely rational, but rational only with respect to that conception of rationality (which might then also be explained) and thus (2) Beliefs which are not so explainable are not absolutely irrational. Beliefs are neither absolutely rational or irrational since the strong programme rejects *that* conception of rationality as universally prescriptable. That is, the strong programme *must* conflict with standard (foundationist) epistemology.

Indeed, as seems clear enough, Bloor and Barnes were (in part) led to their views for epistemological reasons, not least, their perception of the consequences of the demise of standard (empiricist) epistemology, including under-determination, the failure of analyticity, logicism, etc. On the other hand, it is not clear what they see as replacing standard epistemology. In our first review, we offered that they were offering some version of a naturalized epistemology and we do not now take this back. Indeed, we think that it is just this which explains Hekman's rejection of their view (1986) and which makes the recent criticisms of Harré (1986) and Margolis (1986) important.

In this, section we consider Hekman and in the next, Harré and Margolis. Hekman's book, *Hermeneutics and the Sociology of Knowledge*, defends a strong version or sociology or knowledge, the grounds for which are to be found in the hermeneutics of Gadamer. She opposes the epistemological nihilism which is the characteristic posture or Nietzsche, Foucault, Derrida, and Rorty. Moreover, for her, Habermas and Apel finally assume an absolute or universal ground for purposes or their 'historically and culturally modified form or objectivism' (163). She opts finally for Gadamer on grounds that Gadamer acknowledges that 'interpretation must take place within two horizons -that or the text and that of interpreter. The horizons place limits on the interpretation and form the basis for the judgment or the correct and incorrect interpretations' (195). 'Tradition supplies the means by which understanding is possible in human social life. Prejudice is not simply another concept to be deconstructed but, rather, creates the possibility or human understanding itself. Gadamer's ontological perspective reveals that human beings live inside tradition and prejudice. They cannot transcend it. The claim to do so denies not only the possibility or human communication but what Gadamer and Heidegger have revealed to be the ontological condition of human beings' (196).

Given this orientation, then, Hekman rightly sees that Barnes, Bloor and Roy Bhaskar are realists who bear an affinity to her preferred anti-foundationist epistemology. As she says, their 'program involves an attack on the sacredness or science and its exemption from social determination' (39). She writes:

> What Barnes appears to be advocating in these works is a conception of knowledge that is 'relativistic' in the traditional sense, that is, one that asserts that all scientific knowledge is socially produced and hence, that there is no universal or absolute standard for knowledge. But a closer reading reveals that this is not Barnes's thesis. There is another aspect to his work, and that of the strong program in general, that belies the relativist approach…In an attack on phenomenologists and ethnomethodologists in the sociology of knowledge, Barnes argues that they ignore the question of whether the world in any way constrains our knowledge (41) .

It is clear that it is exactly her (incipient) idealism and rejection of naturalistic epistemology which leads Hekman to reject strong programme sociology of knowledge. It is just this which, for her, is one of the 'serious liabilities' of the strong programme. Their 'naturalism' is 'seriously confused,' revealing 'the realist's failure to transcend the Enlightenment conception of knowledge. Despite their redefinition of the scientific method [as hermeneutic], it is evident that the realists continue to stick a *foundation* for their conception of knowledge provided by the "real world".' Indeed, instead of 'debunking the sacredness of science they are, in essence, redefining the scientific method and using the new definition to claim more territory for the domain of science'(39).

As noted, Barnes and Bloor do presuppose 'a real world', but it is a bad misreading to hold that this real world could secure a 'foundation' for knowledge. On the other hand, given Hekman's rejection of the nihilist tendencies in post-structuralist philosophy, she is vulnerable to an *ad feminem* argument that tradition would seem to presuppose a social world which predates individuals and which is a necessary condition for their activity (as Bhaskar writes); but in turn, this would seem to presuppose some sort of 'natural' world which limits and constrains human activities and, accordingly, our beliefs about both these activities and the natural world.

It is also true that Barnes and Bloor reject traditional epistemology, redefine 'scientific method' as inevitably hermeneutic, *and* enlarge the domain of inquiry open to science. This is, indeed, exactly where Hekman's version of 'hermeneutics' divides from the naturalistic hermeneutics of the strong program. But this is exactly to say that there is a sense in wich the strong programme 'privileges' science (Restivo, 1980: 67; Hesse, 1981: 290; Rudwick, 1981: 243).

PHILOSOPHY SELF REFLEXIVITY AND THE PRIVILEGING OF SCIENCE

What does it mean to 'privilege' science? It seems to us that there may be (at least!) two distinguishable questions: (1) Does the theorist want to argue that only science provides knowledge, that other actual and possible practices for fixing belief are not knowledge producing? (2) Are there any good reasons for taking science (or any other practice) as the preferred mode? It seems to us that one could offer an emphatic 'no' to the first question and yet adopt scientific modes as preferred (for at least answers to *some* questions). Indeed, for a writer trained at a British University, these answers would seem to be the predictable outcomes of a reflexive inquiry! In reflecting on what had preceded the epilogue of his early (1974) *Scientific Knowledge and Sociological Theory,* Barnes wrote:

> The epistemological message of the work could be sceptical, or relativistic. It is sceptical since it suggests that no arguments will ever be available which could establish a particular epistemology or ontology as ultimately correct. It is relativistic because it suggests that belief systems cannot be objectively ranked in terms of their proximity to reality or their rationality.

This formulation is important. It is sceptical *not* in the (very strong) sense that we can rationally believe nothing, *nor* in the (very weak, fallibilist) sense that this or that belief is defeasible, lacking certainty. It is sceptical in the sense that no arguments could establish that some belief-fixing practice (or philosophical articulation of it) could be shown to be 'ultimately correct', and thus that others arc not 'correct'. The relativism follows. Alternative practices generating alternative 'belief-systems' can not be ranked *absolutely*.

But, Barnes goes on to say,

> This is not to say that practical choices between belief systems are not at all difficult to make, or that I myself am not clear as to my preferences. It is merely that the extent to which such preferences can be justified, or made compelling to others, is limited *(1954:* 154).

There is nothing paradoxical in this. On the other hand, this is hardly the end of the matter, since if there are limits to the extent to which such preferences can be justified, there remains the question of *how one might even begin to make this effort.*

It is now possible to discern a range of views among SSK writers which runs from the view that the problem of justifying one's preference is insoluble to the view that it is a non-problem with, perhaps, some writers holding to both views! Indeed, except for those 'radicals' who have been critical of the suggestive but incomplete formulations of Barnes, Bloor and Shapin, we believe that among SSK writers, the issue has not been systematically addressed (Gruenberg, 1978; Roth, 1987),

The critics of Barnes and Bloor from within SSK (Steve Woolgar (1976, 1983, 1987), Malcolm Ashmore (1985, 1987), and Michael Mulkay (1985) among others), find themselves agreeing with rationalist critics of the strong program that one cannot apply sociological constructivism to the work of natural science and then hope to deny its *destructive* relevance to the work of the social study of science itself. Thus, Woolgar charges that Bloor's willingness to turn 'constructivist irony…back on itself…but to seek to avoid the infinite regress by declarations that irony is not intended as critical (the "impartiality" tenet)' is no more than programmatic since it 'fails to take notice of the ways in which irony is heard in practice' (255). The move is, thus, 'disingenuous'. But instead of seeing the problem of the absence of a ground for claims about 'reality' (including claims about what scientists 'really do') as a *reductio ad absurdum* of SSK, it is seen by Woolgar as something which, if taken seriously, might be genuinely celebrated!

The problem is a real problem and as Woolgar says, it will not go away. On the other hand, we are not convinced that celebration is yet in order, The line of argument (if one can use that term!) is much too large a topic to take on here, Still, a few remarks may suggest what these writers are up to.

Following in deconstructionist tracks, enormous emphasis is put on reflexivity, including the attempt to employ new literary forms. In this context, Woolgar and Ashmore's 'The Next Step: An Introduction to the Reflexive Project' (1987) does \what it promises; Mulkay's interesting *The Word and the World* (1985) and Ashmore's 'The Critical Problem of Writing the Problem: A Double Text' (1987) are pertinent recent examples of this form of response. Perhaps Woolgar's gloss on Garfinkel is the most economical way to suggest the reason for celebration. He considers Garfinkel's diclum: 'Ethnomethodological studies are not directed to formulating or arging correctives. They are useless when they are done as ironies' (Garfinkel, 1967). He offers as his preferred reading that 'the reader is encouraged actively to undermine the proffered interpretation and to experience some of the deeper implications of continued undermining' (Woolgar, 1983: 261).

Similarly, a double text treatment within SSK literature by Trevor Pinch and Trevor Pinch, 'Reservations about Reflexivity and New Literary Forms: Or Why Let the Devil Have All the Good Tunes' (1987) neatly summarizes what seems to us to be the critical problem:

> In summary, in order for any claim to be made some areas of discourse must be privileged. As we have seen Bloor in effect privileges his own discourse, Collins privileges social science discourse, and Mulkay, Woolgar and Ashmore claim to privilege nothing at all, and thereby as far as I can see claim nothing at all. But as I have argued, in deconstructing others's discourse these latter authors must privilege their own discourse and thereby are caught in the very same trap. Thus, the proponents of the strong version of reflexivity such as Ashmore, Mulkay and Wooigar, appear to want to have their cake and eat it. They critique the sort of argumentative moves made in which they themselves inevitably must engage. By deconstructing SSK they undermine their own premisses.

Trevor Pinch #2. commenting on this passage, declines its deconstruction, offering that 'their' shared aim is to be provocative.

We think that other solutions are available. We can, perhaps, begin with the obvious: The 'others' addressed, like the person addressing those others, are not free-floating rational beings. They are historically rooted beings operating with humanly constructed legacies -now including for us (that is, for readers of this Journal), both what we call 'philosophy' and what we call 'science'.[6] Indeed, it is just (and only!) those 'philosophers' who fully acknowledge this who can enter into a fruitful criticism of strong programme theory. We have remarked on Hekman's work in this regard and have touched on Harré. We can now include, if all too briefly, Margolis's recent *Pragmatism Without Foundations* (1987) and, along with this, some additional aspects of Harré's book.

We judged that Harre's defense of a single-barreled realism was consistent with the widely misunderstood realism of the strong programme, even if Harré himself disavows relativism. Margolis's book, subtitled, 'reconciling realism and relativism' takes another tack. We believe that as regards realism, his 'minimal realism' arrives at much the same conclusions. But as regards the present concern, we believe that the most interesting aspect of his treatment is his explicit attention to the problem of articulating a conception of philosophy which is constrained by the following conclusions:[7]

1 denial of the cognitive transparency or nature --hence of universalism, essentialism, and related doctrines;
2 construal of science as the work and achievement or historical communities;
3 acknowledgement or incommensurability with the diachronic movement or science;
4 rejection of skepticism [that is, 'S *can* know that p'];
5 rejection of objectivism --that is, of any formulable universal framework of commensuration;
6 disqualification of any fallibilist or linear theory of scientific progress or linear theory of verisimilitude;
7 adherence to holism --that, is legitimation without reference to general cognitive privilege or to privileged first-order truths (1987: 116).[8]

It is clear that 1, 2, 3,5, and 6, are features of the SSK inquiry. Indeed, we can say that 7 is the particular constraint (consistent with the strong programme's rejection of traditional epistemology) which, *at the present time,* defines the philosophical task as a second-order inquiry (and which, ultimately, gives concrete sense to 4).[9] But as Margolis writes:

> Here we must be careful. For the sense in which legitimation is a second-order question signifies that it is *not* a "naturalistic" question, in the first-order sense of an empirical science, a matter inadequately addressed by those who assimilate epistemology to psychology (as does Quine [1969]) or to sociology (as Barnes and Bloor do [1982]) or, in general, by those who advocate straightforwardly casual theories of knowledge (1987: 253).

Put crudely, as there are first-order practices which produce belief, philosophy as a second inquiry is the critique of practices. We believe that Margolis is correct in saying that the matter is 'inadequately addressed' by Barnes and Bloor as, indeed, it was inadequately addressed by us in our first review. As regards Barnes and Bloor, this is hardly said in criticism, since not only has their primary effort been in doing and promoting (more) sociology of scientific knowledge—a first order practice, but, as well, it is only in the very recent past that conventionally sustained assumptions about both 'science' and 'philosophy' have collapsed. Of course, the attack on the strong programme has been mostly motivated by 'philosophers' and social scientists who remain with older assumptions about both. (Briefly they take opposite positions on one or more of 1 through 7 above). We believe that Margolis's formulation allows us to see more clearly what leads some to 'deconstructionist' paradox, and others to those forms of dogmatism which are characteristic of mainstream philosophy. Similarly, it may allow us to see similarities among other philosophers who, we think, should be sympathetic to the strong programme --including here Margolis.

Margolis is correct, we believe, in arguing that traditional pragmatisms attempted to justify particular beliefs pragmatically, but that this will not suffice. Margolis's pragmatism is 'global'. We would offer that instead of attempting to justifty beliefs pragmatically, we need instead to seek pragmatic justification of *practices,* including then, knowledge producing practices. That is, the issue is not over the corrigibility of this or that belief, since all are *equally* corrigible. The question rather is, can we discriminate between historically viable knowledge-producing practices and make a judgment that of these some, at least, are to be preferred? Again, the idea is not, as in Peirce, for example, that some mode gives us assurance that we asymptotically approach truth. Rather, the idea is that some mode may have historically vindicated superiorities defined pragmatically, beginning with its capacity to generate life-sustaining and life- enhancing consensus, by its capacity to prevent permanent closure (Manicas, 1987: 263-65), and perhaps teven by its superiority regarding the development of technologies (Taylor, 1982).

It seems to us that this route requires a single-barreled realism exactly because as Barnes puts it, 'if the world were just what it is believed to be, then what can threaten that world other than other believers'? Indeed, 'idealists have no ready way of rationalizing a sense of failure' (1987: 10). While, as we have argued, a single-barreled realism *in and of itself* cannot be used to discriminate between truth-claims, it is, we believe, the first premise in any attempt to vindicate a knowledge-producing practice.

PROBLEMS AND DIFFERENCES

On the other hand, as Margolis writes, even 'assuming minimal realism to be consensually favored at the present time, more complex and pointed forms of realism are bound to be more controversial' and that, for good or for ill, it will not be easy to be content with minimal realism itself (1986: 116-117).

It is just here that we can see potential division between strong programme theorists, other advocates of SSK including, for example, Collins, Latour, Woolgar and others, and 'philosophers', e.g., Bhaskar, Harré, Margolis and others who are 'single-barrelled' realists. The second-order critique of any practice demands an articulation of that practice; and no doubt, there will be important differences in how these are theorized,

One critical difference already alluded to is the question of how the *object* of *social* scientific inquiry, 'society', is to be theorized. Again, the topic is huge, but perhaps suggestively, we can contrast Latour and Woolgar, but especially, Latour's very recent *Science in Action,* with the theorization (with differences) of Bourdieu (1977), Bhaskar (1979), Giddens (1983), Manicas (1987) and Porpora (1987). Very roughly, there would seem to be three main questions: First, is some conception of social structure needed? Second, assuming that it is, do we need to distinguish 'non-scientific' practices (structures) from 'scientific' practices (even if they are or may be ‘internally' and/or 'externally', related)? Finally, and already mentioned, there is the question of agency/structure but especially as that bears on the problem of causal explanation. Latour sometimes writes as if 'society' is an ephemeral homogeneous blur and as if causal explanation is either impossible 'or unnecessary; Giddens and Bhaskar offer that social structures 'exist in time' and that society is a 'system' of these. Morever, they attempt theorizations of causal explanation in terms of agency/structure duality.

Clearly related and pertinent is the interpretation of ethnomethodology and other 'micro-sociologies' and their relation to sociology. As Woolgar has rightly noted (1981 b), Garfinkel can be read as either 'reformist', and thus as altogether incorporable into Barnes's causal version of sociology of science, or as 'radical', Woolgar's preferred reading.[10]

But prior to this, even, is the question: How is *science* to be theorized? Whitley (1983) is correct, we think, in charging that strong programme theorists have been insufficiently clear on the conception of science which is assumed in their (scientific) sociology of (scientific) knowledge. We think that he is mistaken in charging that they 'seek to explain scientists' behaviors and decisions solely in terms of general causal laws by means of hypothetical-deductive reasoning' and thus are 'following a philosophically and sociologically inadequate model of explanation which implies a monolithic and unified conception of scientific knowledge' (1983: 692) . Indeed, this issue is at the core of Paul Roth's very recent extended critique of the strong programme, what he provocatively calls 'Voodoo Epistemology' (Roth, 1987: chs 7 and 8).

Roth's M*eaning and Method in the Social Sciences*, a spirited defense of 'methodological pluralism', attacks the strong programme for failing to see through the implications of their post-empiricist philosophy.’[11]

He holds that, finally, their program is incoherent (183). The incoherence results from 'the strong programmers' abortive attempt to marry their doctrine of holism [finitism] and a claim to be able to identify necessary and sufficient [causal] conditions (192). The problem, is clear enough: Finitism must reject the analytic/svnthetic distinction, but 'what is it to give necessary and sufficient conditions except to say that a certain relation is analytic?' *(ibid.)*. Roth sees the incoherence popping up in many places. For example, if the extension of a term is open-ended, then there is no 'program' to be learned which the learner can apply); but if so, then, 'the causal principle cannot he satisfied. Causality requires specifying necessary and sufficient conditions of social conditioning' (194). As Roth makes clear, the view of causality which this presupposes is 'Humean concon-comitant coincidence' (184) *and* it just this view which he imputes to strong programmers. Roth is quite correct that *if* this is the notion of causality assumed, then strong programme views arc incoherent. But we are not surprised that he does not find any Humean causal laws at work in concrete SSK inquiry; it is informed by more realist strands of thought. Roth's argument nevertheless highlights the centrality of a firmly realist theory of science to strong programme scientific practice. Indeed, we would argue that lacking this, it is hard to see how it could be sustained in second-order critique--a point perhaps vaguely seen by mainstream writers (Laudan, 1981), revisionists like Roth, and by idealist critics of the strong programme (including those within SSK who tend in this direction). Some sort of realist theory of science as an alternative to 'nomothetic empiricism' is (with differences) a common theme of a growing number of writers, but it is not yet widely appreciated how different this alternative is. Nor, as in Roth's account, is there yet an appreciation that the implications for social science are monumental. On the other hand, the fact that the empiricist charge against SSK recurs suggests that much more clarity is needed.

There are not only differences between the 'realist' theories, there are also many problems. For example, all have presumed that their articulation of science is a better approximation of the actual practice of some paradigmatic science. But the question then is, what counts as the paradigmatic science whose practice is being theorized (Gieryn, 1983; Fuller, 1985; Rudwick, 1981)? Consider here among the (countless?) possibilities: physicists at Princeton's Advanced Institute, physicists at Rome AR&D, evolutionary biologists at Harvard, comparative taxonomists at the Museum of Natural History, population geneticists at Cambridge, recombinant DNA researchers at Cold Spring Harbor, hematologists at Bronx Veterans Hospital, psychologists at Cornell working on perception, psychologists working at MIT in AI, psychologists at NIMH, economists at the University of Chicago, or at the Bureau of Labour, etc. etc.

If once we believed that these shared in something called 'scientific method', it is by now *empirically* clear that they may share in nothing but a name!

Finally, there are the problems and differences in the second-order attempt at grounding, as preferred, this or that practice. SSK writers are correct, we believe, to reject TRASP (Truth, Rationality, Success or Progress) as available for justificatory purposes (Collins, 1981). Put briefly (if dogmatically), each of these presuppose a philosophy not constrained by the seven 'conclusions' identified by Margolis (above). But if we are obliged to deny 'transparency', acknowledge incommensurability, deny 'objectivism' and 'privileged first order truths'; and if we are obliged to affirm historicism along with some form of realism that precludes 'cognitivism', it does not follow that all justification is precludcd, We conclude tentatively with some brief comparisons between Bhaskar (1978, I979, 1986), a writer rightly linked to strong programme inquiry and to Harré and Margolis, two recent critics of the strong programme.

Bhaskar shares explicitly with Margolis a notion of philosophy as second-order inquiry. One possible difference is their form of argument, whether (to employ a distinction suggested by Taylor (1975)), Bhaskar's form of 'transcendental' argument is intended to be 'weak' (pragmatic, inductive, probabilistic) or 'strong' (apodictic, deductive)? Surely the latter cannot be sustained. Another difference, clearer to us, is the fact that Bhaskar attempts a philosophical defense of a stronger form of (single-barrelled) realism than any of the writers mentioned in this essay. Similarly, Bhaskar writes with a confidence regarding the claims of science which seems, at least, to rule out his explicit relativism. On the other hand, can we suppose that SSK writers do not share in believing that their scientific accounts are to be preferred?[12]

As noted, Harré attempts to foreclose 'the mess of relativism'. On the other hand, he seeks not 'foundations' but grounds and these are found, not in statements, but in material practices: 'We trust beliefs that have been produced by reliable people using reliable methods' (166), As with Bhaskar, it is not anything called 'science' which Harré contends is to be preferred over other possible knowledge-producing practices, but the specific practices of what is perhaps best called the 'theoretical sciences'. Harré has made convincing argument that what he calls 'Realm I' discourse -'the science of objects of common experience' is but proto-science whose instabilities are remedied only by including reference to unobservable things and processes (Realm 2 discourse). Since his brilliant defense of this, what he calls 'policy realism', takes the form a inductive argument which, finally, is a recommendation, it is hard to see how in the broadest outlines, at least, he disagrees with Margolis and Bhaskar about either the role of philosophy or finally, the justifiability of preferring that now better understood mode of fixing belicf which we call 'science'. But this is no 'scientific triumphalism' either. As Rudwick (1981) and Hesse and Arbib (1986) have offered, there is no reasonable way to rule out 'the possible differentiating effect [on belief] of claimed imputs from externality characterized in theistic terms', a conclusion reached seven decades ago by the never intrepid relativist, pragmatist and psychologist, William James.

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NOTES

[I] See also Flew's 'Must Naturalism Discredit Naturalism?' in G. Radnitzky and W.W. Bartley, III (ed..), *Evolutionary Epistemology, Rationality, and the Sociology of Knowledge* (LaSalle: Open Court, 1987). Flew does not hide the motivations for his attack on the strong programme. For him, it is of a piece with 'profoundly obscurantist and educationally subversive doctrines [which] have been preached as revealed truth from the electronic pulpits of the Open University and in the more conventional lecture halls of the University of London Institute of Education' (p. 409). Indeed, in his book, *The Pied Pipers of Education* (1981), he 'argues for a substantial saving of counterproductive public expenditure by excising this cancer from the curriculum' (note 11, p. 409).

[2] Bloor's causality tenet asserts: '[The inquiry] would be causal, that is, concerned with the conditions which bring about belief or slates of knowledge. Naturally, there will be other types of causes apart from social ones which will cooperate in bringing about belief (Bloor, 1976: 4). In addition to the work of Barnes and Bloor, see D. MacKenzie, *Statistics in Britain 1860-1930: Social Construction of Scientific Knowledge* (Edinburgh: Edinburgh University Press, 1981); B. Latour and S. Woolgar, *Laboratory Lifet* (London: Sage, 1979, 2nd Edition, Princeton University Press, 1986; K.D. Knorr-Cetina, *The Maufacture of Knowledge: An Essay* on *the Constructivist and Contextual Nature of Science* (Oxford: Pergamon Press, 1981). H.M. Collins and T. Pinch, *Frames of Meaning: The Social Construction of Extraordinary Science* (London: Routledge and Kegan Paul, 1982), Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Chicago: University of Chicago, 1984); M. Lynch, *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory* (London: Routledge and Kegan Paul, 1985); S. Shapin and S. Schaffer, *Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life.*

(Princeton: Princeton University Press, 1985); Trevor Pinch, *Confronting Nature: The Sociology of Solar Neutrino Detection* (Dordrecht: Reidel, 1986); The journal, *Social Studies in Science*, contains any valuable essays. Also see note 5.

[3] Latour and Woolgar, on the 'left wing' of SSK inquiry, encourage misunderstanding when they assert that 'the social' 'no longer has meaning' (1986: 281). Their point is that in the context or Mertonian sociology or knowledge, 'social' contrasted with 'scientific' and in Barnes and Bloor, initially at least, it contrasted with the attack on 'internalist' history of science, well represented by Laudan. Since for Latour and Woolgar, nothing is not social, the term is vacuous. We would demur on two counts. First, granted that 'social' refers equally to 'a pen's inscription on graph paper, to the construction of a text and to the gradual elaboration of an amino acid chain', there will still be useful contrasts to be drawn between factors which are 'individual' (biographical) and those which are 'social' in the sense that they are 'structural'. Second for the realist, non-social factors—physiological and physical --do enter into all human activities, even if such factors are always socially mediated. We return to these to themes below.

[4] Collins (1985) offers that the symmetry principle should be taken to its conclusion, that 'all description-type language should be treated at the outset as though it did not describe anything real'. This seems mistaken. That is, the terms should be treated as if they desscribed *something* real, even though, of course, *what* that real is will be open and contestable.

[5] There are, by now, an impressive number or empirical studies of experimental situations which show that any sort of Popperian notion cannot be sustained See e.g., H. L. Collins, 'The Seven Sexes: A Study in the Sociology of a Phenomenon or the Replication or Experiments in Physics', *Sociology* 9 (1975); Trevor Pinch, 'What does a Proof Do if it does not Prove? A Study or the Conditions and Metaphysical Division Leading to David Bohm and John von Neuman Failing to Communicate in Quantum Physics', in E. Mendelsohn *et al* (eds.), *The Social Production of Scientific Knowledge, Sociology of Sciences Yearbook,* Vol, 1 (Dordrecht. Reidel, 1977); Bill Harvey, 'Plausibility and Evaluation or Knowledge: A Case-study of Experimental Quantum Mechanics', *Social Studies of Science,* Vol. 11 (1981); A. Pickering, 'Constraints on Controversy: The Case or the Magnetic Monopole' *(ibid).);* G.D.L. Travis, 'Replicating Replication? Aspects or Social Construction or Learning in Planaria Worms *(ibid.);* and Trevor Pinch, 'Theory Testing in Science --The Case or Solar Neutrinos; Do Crucial Experiments Test Theories or Theorists', *Philosophy of the Social Sciences* 15 (1985) and further references therein.

[6] Thomas McCarthy (1987) puts this idea at the center of his critique of relativism. McCarthy cites Barnes and BIoor as recent expositors or the view he rejects, but it is by no means clear to us that they would be uncomfortable with his overall conclusion. They do not hold, e.g., that their descriptive accounts are 'strictly neutral' and they would agree, we believe, with his explication or Horkheimer except that to say that the 'for us'/'for them' relation is a dialectical one does not break one out or the hermeneutic circle. Moreover, they would not deny that 'when we offer an account or their beliefs which differs from our own account, we have *ipso facto* criticized them, implied that that we are right and they are wrong'.

[7] In Margolis's book, these are conclusions to carefully wrought arguments. To put these most complicated matters as briefly as possible, he argues that there is some mind- independent reality ('ontic externalism'), but that the question, 'the way the world is' makes sense only relative to some conceptual scheme or other ('ontic internalism'). Moreover, while we need to reject both the idea that there is one true and complete description or the way the world is ('alethic externalism') and the idea that the 'real' is 'self identifying' ('epistemic externalism'), there is a perfectly adequate (coherent and anti-Protagorean) sense or 'objectivity for us' (epistemic internalism).

[8] Margolis argues that Rorty's well-known rejection of philosophy depends on overlooking this possibility. Margolis is critical of Habermas and Opel for their universalism,

finds insight *and* excess in Foucault, Derrida and--here departing from Hekman, from Gadamer as well, Margolis favors 'textualism,' the 'theory that our knowledgc of the world is an interpretation of whatever we take ourselves to discriminate within an indissolubly relational condition in which the actual world is cognitively accessible only through the tacitly organizing concepts of a natural and historically changing praxis and language.' (pp. 235-6). Indeed, while there are 'florid' versions or this, in e.g., Derrida, 'textualism is essentially a form of realism that precludes ‘cognitivism' (the view that we possess cognitive powers that self-certifying, self-insuring, self-presenting, or indubitably apt for securing truths about the actual world (p. 144). Textualism is realist, anti-foundationalist, hermeneutic, historicist.

[9] In William James's notes for the 1879 'The Sentiment of Rationality', there is a neat argument for the pragmatic pertinence of the idea of a *knowable* non-experienceable reality. James says:

> Th principlc of "pragmatism", which allows for all assumptions to be of identical value so long as they equally save the appearances will of course be satisfied by this empiricist explanation…[viz, as according to Mill, that no mysterious "outness" needs to be postulated]. But common sense is not assuaged. She says, yes, I get all the particulars, am cheated out of none of my expectations. And yet the principle of *intelligibility* is gone. Real outness makes everything simple as the day, but the troops of ideas marching and falling perpetually into order, which you now ask me to adopt, have no *reason* in them--their whole existence is *de facto* and not *de jure* (*Works of William James: Essays in Philosohy,* Fr. Burkhardt, General Editor, Cambridge, Ma: Harvard University Press. p. 364).

Nevertheless, if British phenomenalism did not suffice, neither could he accept a 'more' beyond the actual as it functioned in Kant and Spencer. The 'unknowable' cannot function to give order since to do this it must be known to have properties which could explain the orderliness or experience (p. 369). See my 'Pragmatic Philosophy of Science and the Charge of Scientism', forthcoming in *Transactions of the Charles S. Peirce Society*..

[10] In addition to Woolgar's essays, see Barnes's 'reformist' reading of Heritage's *Garfinkel and Ethnomethodology* (Cambridge: Polity, 1984) in *Social Studies in Science* 19 (19*85)* and Bloor's critique of Eric Livingston, *The Ethnomethodological Foundations of Mathematics* (London. Routledge and Kegan Paul, 1986) in *Social Studies in Science* 17 (1987).

[11] Roth's book has many praiseworthy features even if we cannot agree with many of his conclusions. He is correct, we believe, in rejecting Quine's version of naturalized epistemology and in arguing that the strong programmers have been somewhat naive (as we were!) as regards, the problems of a post-empiricist epistemology. While it is hardly the place to develop this, we would however, reject his line of solution. He aims to show that disputes over differing standards of rationality are pseudo problems, best conceived as testable translation questions. It seems to us that Roth remains caught in empiricist assumptions about reason and evidence.

[12] Collins is clear: ‘A loss of confidence in the scientific enterprise is a disaster that we cannot afford. For all its fallibility, science is the best institution for generating knowledge about the natural world that we have' (1985: 165) .

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