

RELATIVISM, VALUE-FREEDOM AND THE SOCIOLOGY OF SCIENCE*

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I

Since its inception in the work of the classical social theorists, the sociology of knowledge—or as some would prefer, of belief—has generated an extensive array of debates about relativism. A central question has been whether the very existence of social determinants for beliefs makes it impossible to regard them as true or rational in any absolute sense, and this has proved especially contentious in the case of scientific beliefs, at least for the many who, whilst happily conceding some form of relativism for moral and political beliefs, are loath to accept a relativist view of science. Hence, if the social determination of beliefs does entail relativism, and scientific or cognitive relativism is to be avoided, definite limits must be placed upon the legitimate domain of inquiry for a sociology of belief. It must confine itself to the study of *non*scientific beliefs, among which may perhaps also be included the results of quite clearly defective or spurious attempts at science.

In rejecting this “limitation thesis” (LT) a number of different arguments have been invoked by those who insist on pursuing the project of an unrestricted sociology of scientific knowledge. Some have declared the question of relativism an exclusively philosophical one: if the discovery of social determinants for scientific beliefs causes difficulties for (nonrelativist) philosophers, so be it—let them deal with their own problems. Others have argued that if the social determination of scientific beliefs entails cognitive relativism, then, since such determinants can in fact be identified, this should simply be taken to provide support for relativism—“philosophical” objections notwithstanding. Others again have similarly been unconcerned by the relativistic implications of their work, but for the additional reason that they believe cognitive relativism to be justified anyway on independent grounds.

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Just such a variety of attitudes can be discerned among advocates of the “strong programme” (SP) for the sociology of science, initially developed in the work of David Bloor and Barry Barnes in the mid-1970s and subsequently providing the methodological rationale for the empirical research of the so-called “Edinburgh School”.¹ Opposed to the LT in any form, the SP’s advocates have often focused their criticisms upon one especially influential version of it. According to this no sociological (causal) explanations are permissible for *rational* scientific beliefs, which are instead to be noncausally “explained” by the procedure of rational reconstruction: by demonstrating how their acceptance satisfies the requirements of some philosophically preferred conception of rationality.² By contrast, proponents of the SP have tended to regard the concept of rationality as having no place in a properly scientific study of science. Their program commits them, instead, to identifying the social determinants of all “scientific” beliefs—defined merely as whichever are regarded as such by relevant social groups—irrespective of their truth or falsity, rationality or irrationality, or conformity to any philosophical prescriptions for genuine scientificity; and for good measure the SP asserts that sociological enquiries conducted in its name are likewise subject to social determination.

Both critics and advocates of the SP have largely agreed that the program entails some form of cognitive relativism.³ For some critics, admittedly, it is the substantive character of the research performed under the SP’s auspices that has chiefly been faulted, the program’s relativism being merely noted *en passant*. For others, however, it is precisely the SP’s relativism that has been seen as providing the main grounds for rejecting it; and among these are some who have viewed the program as especially vulnerable to a version of the self-refutation argument against relativism. In response to the charge of relativism the SP’s advocates have displayed either mild embarrassment, studied indifference, or open celebration—so that by some, indeed, it has actually been called “The Empirical Programme of Relativism.”⁴

It is this widely shared belief in the SP’s relativism that I shall contest. The program, I shall argue, neither implies nor necessarily assumes any form of cognitive relativism. Undeniably, most of its proponents in fact subscribe to some such view. But this is due either to their regarding it as entailed by the SP itself or to their being convinced of it on independent grounds. I shall thus be providing a defense of the program against criticisms based on its supposed relativism—though this defense will hardly be welcomed by many of its proponents. Nor will they be likely to welcome my further claim that, partly through misconceptions about the SP’s relativism and what this would imply, the research inspired by the program is subjected to unfortunate and unnecessary theoretical limitations.

Ultimately, I shall suggest, these involve a failure to explore just those issues that would be most relevant to a properly, sociologically, informed discussion of cognitive relativism itself. More obviously, they remove from the program’s agenda a range of politically significant questions about the social organization of scientific practice—such as what, if any, are the cultural conditions which

support or undermine the process of rational scientific enquiry. Such questions, according to the SP's proponents, cannot be addressed by a strictly scientific sociology of scientific beliefs, since they employ normative concepts of "rationality" and hence require the sociologist to make a certain kind of (intrinsically unscientific) "value-judgment."

It is by exploring this (at least implicit) idea of a value-free sociology of science that I shall criticize the view that cognitive relativism is entailed by the SP and attempt to free it from its advocates' self-imposed limitations. Working from a general consideration of value-free social science, in which the relevant normative concepts are traditionally of a political or moral nature, I shall outline a conception of value-freedom according to which the claims of such a science can and should be assessed independently of value-judgments employing the concepts of "justice," "freedom," and the like. But, I shall argue, this doctrine of value-freedom need not rely upon a relativist view of values, and neither does it require the elimination of such concepts in framing or answering legitimate theoretical questions. Analogously, I shall suggest, the descriptive and explanatory claims of a value-free sociology of science, for which the relevant normative concepts are those of "rationality," "scientificity," and so on, are independent of value-judgments employing these concepts. But this does not require the assumption of cognitive relativism, nor does it rule out the use of specific conceptions of rationality or scientificity in identifying and resolving theoretical problems.⁵

More generally, I shall suggest, the question of cognitive relativism should be seen to bear a similar, and similarly complex, relationship to the assumptions and findings of the sociology of science as that of moral relativism does to sociological investigations that deal with moral and political beliefs. In both cases a properly value-free sociology may well produce results that are relevant in various ways to the philosophical issue of relativism. But relativism is neither directly implied nor necessarily assumed by the possibility of a scientific sociology of beliefs.

I shall proceed in the following manner. After some preliminary remarks in section II about the formulation of the SP and the concepts of relativism and scientific rationality, I turn in section III to examine the idea of value-freedom in the social sciences and to show that certain further doctrines often associated with this are not in fact entailed by it. In section IV I present the main features of a value-free sociology of science, giving particular attention to the place within it of conceptions of scientific rationality; and in section V I offer some possible diagnoses for the SP's having been thought to entail certain views which—understood as (cognitively) value-free sociology of science—it does not, including cognitive relativism. I conclude, in section VI, by indicating how the possible results of sociological inquiries about scientific beliefs might bear upon the problem of cognitive relativism.

II

The basic features of the SP are normally taken to be defined by the following four tenets:⁶

1. *Causality*: It would be causal, that is, concerned with the conditions which bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief.
2. *Impartiality*: 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. *Symmetry*: It would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs.
4. *Reflexivity*: It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. Like the requirement of symmetry, this is a response to the need to seek for general explanations. It is an obvious requirement of principle because otherwise sociology would be a standing refutation of its own theories.

Critical discussions of the program have identified a number of issues concerning the precise sense of each tenet and the logical relationships between them.⁷ I shall comment briefly on some that will prove relevant at various points later on.

The first set of problems concern tenet 1. From this it seems clear that the SP does not assert that all scientific beliefs are determined exclusively by social factors: within its overall commitment to complete causal determination some possible explanatory power is allowed to nonsocial factors. This raises the question of how “strong” the SP actually is,⁸ and indeed of whether the program is genuinely testable without further specification of the relative significance to be ascribed to social and nonsocial determinants. More important, for my purposes, is the question of how the distinction between these is itself to be drawn.

In implicit answer to this, advocates of the SP have cited some or all of the following as paradigmatically nonsocial determinants: biological, or biologically grounded, factors such as innate features of the human species’ sensory mechanisms, or forms of “natural rationality”—that is, any genetically programmed capacity for, or tendency toward, certain modes of reasoning; psychological characteristics of individuals which are not themselves amenable to sociological explanation; and “external reality” itself. I shall focus on just one question of the many raised by this: Are what may be

termed “accepted norms of scientific rationality” to be regarded as social or as nonsocial determinants?

At least *prima facie*, members of scientific communities typically endorse various principles of argumentation and reasoning as properly governing the acceptance or rejection of one another’s scientific claims. To what extent, if any, these norms of reasoning actually influence their beliefs is an empirically open question. But whether these are deemed “social” or “nonsocial” will significantly affect both the content and the plausibility of specific SP-explanations for scientific beliefs. Unlike, it seems, many proponents of the SP, I can see little reason to deny a social status to such norms of rationality—unless, of course, they are largely explicable in biological terms, and hence belong to the category of “natural” rationality.⁹

The second set of problems concern tenets 2 and 3. In support of the former, proponents of the SP have sometimes claimed that it is impossible to distinguish, for example, rational from irrational (scientific) beliefs. But I shall take it that this is not entailed by tenet 2 itself, which commits one only to regarding such distinctions as *irrelevant* to the explanation of beliefs—the precise nature of this (ir)relevance can be left unspecified for the moment. In the case of tenet 3 two related problems may be noted. First, there is an apparent ambiguity in the sense to be given to “the same types of cause.” Does the “sameness” here consist in there being *social* determination in both cases, or in there being the same *types* of social determinants for both? Whichever answer is given, a second problem arises: Should tenet 3 be taken to assert that there are *in fact* “the same types of cause” for, for example, both rational and irrational beliefs; or only that one should not, in advance of empirical research, assume that the two necessarily differ in this respect? The former is perhaps the more plausible reading of tenet 3; the latter, I shall argue later, is the more plausible view.¹⁰

Finally, some remarks about tenet 4. To those who are impressed by self-refutation arguments against relativism it might seem that this assertion of “reflexivity” makes the SP peculiarly vulnerable. But as some commentators have noted, tenet 4 is effectively entailed by tenet 1, so that if there are difficulties of this kind for the SP, they are not generated by tenet 4 itself. However, since I shall be arguing that the program (with or without tenet 4) does not entail relativism, such issues will not require attention.

What now needs clarification is the nature of the SP’s supposed relativism. I shall focus upon what has been the main concern of both advocates and critics, namely, relativism with respect to scientific rationality. Adopting the customary (though by no means unproblematic) distinctions between moral and cognitive relativism,¹¹ and within the latter between ontological and epistemological relativism, this can be located as a particular form of the last of these. I shall ignore the question of whether

epistemological relativism can be defended without commitment to ontological relativism, merely noting that at least some proponents of the SP are apparently happy to espouse the former whilst continuing to talk of “external reality”—for instance, as a non-social (though only partial) determinant of scientific beliefs.

Relativism concerning scientific rationality can be understood in the following manner. The work of, for example, inductivist or hypothetico-deductivist philosophers of science may be regarded as an attempt to specify certain rules or standards by which the truth or falsity of scientific theories, and hence the rationality or otherwise of their acceptance, are to be judged. Such proposals thus involve both the articulation of a specific conception of scientific rationality and the claim that this is the proper basis upon which scientific beliefs should be assessed.¹² By contrast the relativist maintains that there are a number—perhaps an indefinite number—of actual and possible conceptions of rationality, but that no one of these can be shown to be superior to all others. Hence, although one can determine whether, and to what extent, the acceptance of a scientific belief is “rational” with respect to some particular such conception, one cannot show that it is rational in any absolute sense, since no one conception of rationality can itself be justified and thereby shown to be correct. (For convenience I shall from now on refer to this specific form of epistemological relativism simply as “cognitive relativism.”)

A similar characterization can be given for the kind of *moral* relativism which will figure significantly in what follows. Assuming such relativism is applicable to political as well as moral values, I shall indicate its nature in the case of justice—in particular, justice concerning the distribution of social goods. One can find within political philosophy a number of distinct and competing conceptions of justice: egalitarian principles; desert-based principles invoking criteria such as skill, output, or effort; need-based principles; Rawls’ difference principle; Nozick’s non-coercive transfers; and so on. Defenders of such conceptions typically maintain that a society’s actual distribution of social goods should be judged by reference to their preferred criteria: “justice requires that ...,” and hence the distribution is legitimately criticized to the extent that these requirements are not met. By contrast, then, the moral relativist will assert that although one can show that a society’s distribution of social goods is just or unjust relative to a particular conception of justice, there is no way of showing that one such conception is itself uniquely preferable or correct. Hence, no absolute judgments of the justice or injustice of particular distributions can be rationally justified: nothing is just or unjust *tout court*.

Two further points about both forms of relativism can be noted here. The first concerns the fact that in making judgments such as “X is rational” or “Y is unjust” one is normally taken to be (among other things) expressing some kind of approval or disapproval, support or criticism, praise or condemnation,

of the item concerned. This can be termed the “appraising force” of the judgment, and its presence is at least partly constitutive of something’s being a *value-judgment*. (To talk of value-judgments in this way does not, of course, commit one to an expressivist meta-ethics.) I shall assume that this feature of such judgments is preserved in a relativist account, but I shall leave open the question whether the relativist’s rejection of “absolute” justifications undermines also any justification for a value-judgment’s appraising force.

The second point is this. In support of these forms of relativism, claims of one or both of the following kinds may sometimes be made: (i) that different conceptions of rationality/justice have in fact been endorsed by different scientific communities/social groups at different times; (ii) that sociological explanations can be provided for such variations. What degree of support is provided for relativism by (i) and (ii)—themselves often, though misleadingly, termed “sociological relativism”—will be addressed briefly in section VI: they will not be taken as constitutive elements of either cognitive or moral relativism in the ensuing discussion.

III

The central thesis involved in the idea of a value-free social science can be presented as follows: The truth or falsity of descriptive and explanatory claims in the social sciences is logically independent of the acceptance or rejection of any value-judgments about what is referred to in such claims.

I shall not here attempt to defend this thesis—henceforth, VF—since my main concern is to show that certain further theses often associated with VF are not entailed by it, and the implications of this for the nature of a value-free sociology of science.¹³ But before doing so, some remarks are necessary about this formulation of the doctrine of value-freedom.

First, although VF is presented here in a realist vocabulary, both for convenience and from conviction, it could easily be translated into a form compatible with at least several versions of instrumentalism or pragmatism. Second, the formulation is somewhat narrower in scope than is usual, in that the range of value-judgments is restricted to those directed at items referred to in the relevant descriptive and explanatory claims. This is a convenient limitation that could be removed if necessary. Third, VF does not straightforwardly support a practical maxim of the form “Do not allow your assessment of a (social) scientific claim to be influenced by your commitment to any value-judgments”. Such an implication would obtain only if the maxim were modified so as to read “*improperly* influenced,” as a result of which it might become vacuous.

Finally, whilst VF clearly depends upon some version of the dichotomy between “facts and values” (or better, perhaps, between “the world and its evaluation”), it is compatible with a considerable variety of meta-ethical positions concerning the cognitive status of value-judgments, including both those which allow, and those which deny, the ascription of truth-values to such judgments. (My use, in VF, of the phrase “acceptance or rejection” in relation to value-judgments is intended to mark this neutrality) Hence, as will now be briefly argued, VF does not entail the following:

NVF (1): no rational justification can be provided for value-judgments.

Undeniably, NVF (1) can be used to support an argument for VF, and indeed many proponents of VF, such as Max Weber, have done just this. The argument runs along the following lines: social science must produce rationally justifiable knowledge-claims; value-judgments are not rationally justifiable; *ergo* social science must be value-free. But the question remains of whether VF *requires* the assumption of NVF (1)— of whether there are alternative arguments for VF which do not rely on such scepticism about value-judgments.

That there are is supported by the following considerations. All that the advocates of VF need maintain is that the acceptance or rejection of value-judgments about the objects of inquiry are *irrelevant* to the truth or falsity of its descriptive and explanatory claims. The criteria of validity for these claims must be logically distinct from those governing value-judgments. But there is no need to assert that value-judgments lack any such criteria, and are hence not rationally justifiable. (To put this point another way: VF does not require the assumption of scientism, that is, of the view that science is the only genuine form of human knowledge.) Hence, for example, both Kantian and subjectivist views of the cognitive status of value-judgments are equally consistent with the doctrine of value-freedom. *A fortiori*, VF does not require the assumption of moral relativism, and there is no need to spell out here the relativist’s somewhat complex attitude toward NVF (1) itself.

Similarly brief consideration can now be given to a further thesis, sometimes thought to be implied by the doctrine of value-freedom, namely:

NVF (2): no reference can be made in a social science to the (moral and political) values of those whom it investigates.

It is rare to find explicit arguments aimed at showing that NVF (2) is implied by VF: more often the claim that it is seems due to simple misunderstanding. It may, perhaps, be thought that value-free descriptions of the values which other people accept—that is, of their moral and political beliefs—are particularly difficult to achieve. But it is far from clear why this should be so, and to support such

optimism, one might reasonably note that the truth or falsity of statements reporting beliefs is independent of the truth or falsity of the reported beliefs. Hence, for example, the claim that members of a certain social group endorse some particular view of justice is itself neutral with respect to the merits of that view.¹⁴

More worthy of attention, however—since it raises issues that will prove important later on—are the relationships between both VF and NVF (2), and the substantive claims of what may broadly be termed *materialist* social theories, such as Marxism. One can define such theories as maintaining both

- (i) that the values accepted by social groups have little if any explanatory power, and
- (ii) that the acceptance of these values can itself be explained primarily by reference to “material” conditions—”material” being taken to exclude, at the very least, any processes of a ratiocinative or reflective nature.

Clearly, the truth of these claims neither implies nor is implied by NVF (2): indeed they could not be established without rejecting this thesis, since the ascription of what is basically an epiphenomenal status to “accepted values” requires that descriptive statements can be made about them. But whether these materialist claims are correct is itself a substantive theoretical question, contested for example in Weber’s disagreement with Marx in his (non-materialist) account of the Protestant ethic’s role in the historical emergence of modern capitalism. What the doctrine of value-freedom maintains is that the significant differences between Marx’s and Weber’s political values are irrelevant to the resolution of this explanatory dispute, like any others.

More complex issues, however, are raised by a further thesis often thought to be implied by VF, namely:

- NVF (3): social science must not employ any normative concepts in its descriptive and explanatory claims.

Without attempting a strict definition of “normative concept,” I shall take it that justice, freedom, alienation, exploitation, and so on are paradigmatic instances of this kind, and that this is so by virtue of their typically being understood to (at least partly) indicate the appraising force expressed in the value-judgments which employ them.

To show that NVF (3) does not follow from VF it will be convenient to proceed by considering some hypothetical examples of social scientific claims involving the concept of justice. The first involves its

possible use in providing explanations for some social phenomenon. Suppose that the collapse of a political regime is said to have been due to its injustice, in particular the injustice of its pattern of economic distribution. (This explanation is, of course, to be distinguished from one which refers instead to the distributive pattern's being perceived or regarded as unjust by certain social groups.) Such an explanation, I shall argue, is compatible with the doctrine of value-freedom provided that the concept of justice is being employed in what may be termed a *purely characterizing* manner: namely, one in which both the "correctness" or justifiability of the concept(ion) of justice involved, and the appraising force of its normal use, are put aside or "suspended."¹⁵

For example, suppose that the specific conception of justice involved in the claim "the regime fell because of its injustice" is egalitarian. Then the truth of this explanatory claim can be assessed independently of whether economic equality *is* "what justice requires," that is, independently of whether the egalitarian conception of justice is correct or rationally justified, and the question of whether the unequal pattern of distribution caused the regime's downfall can be answered without either endorsing or rejecting the *appraising force* normally expressed by statements employing the concept of justice.

Some further comments need now to be made about this. First, in using the concept of justice in this purely characterizing manner it is essential that one indicates clearly the specific conception of justice involved; otherwise fruitless debates may be generated through mere ambiguity. Second, any such conception must be operationalizable, that is, it must meet the standard requirements imposed upon scientific concepts with respect to the testability of statements which employ them. (To accept such requirements does not commit one to a verificationist or operationalist theory of meaning, however.) The egalitarian conception of justice, fairly obviously, meets this condition; so too, it could be argued, does Nozick's transfer principle, Rawls's difference principle, and many others. It will, of course, be no easy matter to determine whether a pattern of economic distribution meets, for example, the difference principle, since this will depend on answering counterfactual questions about the effects of alternative distributions on the amounts of goods possessed by various social groups. But such epistemic difficulties are hardly peculiar to conceptions of justice: they are a commonplace of scientific enquiry.

Third, this characterizing use of normative concepts is compatible not only with the doctrine of value-freedom but also with moral relativism. Indeed, what the moral relativist says about concepts such as justice fits very well with the account so far given of their characterizing use. For the relativist will maintain that while one cannot rationally justify any particular conception of justice, and hence cannot show that a state of affairs is just or unjust *tout court*, one *can* determine its justice or injustice relative to the criteria specified in some particular such conception. But it is precisely this possibility which

the purely characterizing use of normative concepts depends upon, together with the suspension of the appraising force typically implied by their employment in making value-judgments. The characterizing claims of a value-free science can thus be assessed independently of the correctness of, for example, the specific conception of justice involved, and, hence, also independently of whether rational justifications of such conceptions are possible—that is, of the question of relativism itself. So value-freedom does not entail either NVF (3) or moral relativism, and moral relativism is, like value-freedom, consistent with the denial of NVF (3).

Similar points apply to the use of normative concepts in specifying a domain of inquiry or in posing explanatory questions, as distinct from their use in making explanatory claims of the kind indicated earlier. Thus the doctrine of value-freedom does not rule out posing such questions as: What explains the injustice (or the unjust character) of some society's pattern of economic distribution; to what extent was this determined by the nature of its political system; and so on? Likewise, a value-free social science may legitimately address more general theoretical questions such as: What, if any, are the necessary and/or sufficient conditions (political, cultural, economic, and so on) for distributive justice; is the capitalist mode of production intrinsically incompatible with distributive justice—and if so, what exactly is it about capitalism that explains this; and so on? These questions can be both asked and answered consistently with VF, provided that the conditions noted above for the characterizing use of normative concepts are fully met.

The view of value-freedom and its implications that is being defended here may be illuminated by contrasting it with two mutually opposing attitudes often adopted toward Marxist social theory. According to one, Marxism is to be rejected as unscientific because it addresses such issues as: What are the determinants of alienated human activity, or what are the effects upon political systems of exploitative relations of production? The very terms in which these questions are posed, it is argued, show that Marxist theory is not properly value-free. According to the other, however, it is the doctrine of value-freedom itself that is at fault, since it prohibits investigation of precisely those issues which Marxism rightly addresses. Marxist social theory is thus to be applauded for rejecting the straightjacket of value-free “scientificity”.

But both these attitudes should themselves be rejected, since they assume that VF entails NVF (3). A social theory which refused to ask and answer the kinds of questions which Marxism addresses would indeed be politically impoverished. But there is no need for a value-free social theory to suffer this. Nonetheless, as will now be argued, in refusing the unnecessary limitations which would be imposed by NVF (3), the work of social theorists becomes vulnerable to a certain kind of normative criticism which cannot straightforwardly be dismissed by invoking the doctrine of value-freedom.

Suppose that one has put forward some theoretical explanation of why it is that capitalism necessarily generates, or tends to generate, distributive injustice. Two kinds of objection may be raised: first, that the proposed explanation is false, or at least insufficiently well supported to render even its provisional acceptance rational; and second, that the conception of justice involved is itself objectionable—that there is no good reason to employ this particular conception since, for example, it gives insufficient attention to the requirements of desert, property rights, or such like.

The two kinds of criticism are, of course, entirely distinct: the theory's explanatory claims may perfectly well be true, despite the failings of its conception of justice; and vice versa. This much is ensured by the idea of value-freedom. But value-freedom does not protect social theorists from the latter kind of criticism, which concerns, one might say, the normative significance of their theoretical work rather than its scientific credibility. An analogy may be helpful here. One can—and should—distinguish between the adequacy of a solution and the significance of the problem it is intended to resolve. There can be good solutions to insignificant problems and bad solutions to significant ones. The doctrine of value-freedom requires one to recognize this distinction, but it does not absolve one from normative criticism of the concepts employed in specifying theoretical problems.¹⁶

IV

The preceding account of value-freedom in the social sciences can now be used to articulate the nature of a value-free sociology of science. The basic move is to apply what has been said about moral and political values, such as justice, to cognitive “values”: in particular, to (scientific) rationality. By doing so, I will argue, one can see that a value-free sociology of science does not require the assumption of cognitive relativism with respect to rationality; that it does not rule out the possibility of non-materialist explanations which refer to the conception(s) of rationality accepted by members of a scientific community; and that it allows the characterizing use of specific conceptions of rationality in the identification of legitimate theoretical questions about its domain of enquiry.

First, then, how will the doctrine of value-freedom itself apply in the case of a value-free sociology of science? VF will now read as follows:

The truth or falsity of descriptive and explanatory claims about scientific beliefs is logically independent of the acceptance or rejection of any value-judgments concerning the rationality of those beliefs.

Drawing upon what has been said earlier about the use of normative concepts in making value-judgments, one can regard such judgments about the rationality of a scientific belief as expressing some favourable or unfavourable appraisal of the belief by virtue of its meeting or failing to meet some preferred conception of rationality. So the descriptive and explanatory claims of a value-free sociology of science are to be independent both of the endorsement or rejection of such appraisals and of the correctness or justifiability of the conception(s) of rationality they employ.

One can now go on to consider in turn the implications for a value-free sociology of science of VF's not entailing any of NVF(1) - (3). First, since VF does not entail that value-judgments cannot be rationally justified (NVF (1)), it does not entail that no justification can be provided for value-judgments about the rationality of scientific beliefs. That such judgments cannot be rationally justified would, of course, provide a possible reason for insisting upon value-freedom in the sociology of science. But it is not a necessary one: all that VF requires to be assumed is that these judgments are irrelevant in assessing the sociology of science's claims about scientific beliefs. Further, since VF does not entail NVF (1), it is perfectly compatible with both relativist and non-relativist views of scientific rationality. Thus a value-free sociology of science need not, at least on these grounds, involve commitment to cognitive relativism.

Next, since VF does not imply NVF (2), a value-free sociology of science is not barred from making descriptive and explanatory claims about the conceptions of rationality that may actually be accepted by the members of a scientific community or of other social groups. The truth or falsity of claims made about these "accepted values" is to be assessed independently of endorsing or rejecting these values. Further, what degree of explanatory power such accepted values may have, and what are the determinants of their *being* accepted, are to be regarded as essentially open substantive theoretical questions to be answered in a value-free manner. Hence, for example, it might be the case that a broadly materialist social theory of science is correct. But the basic project of a sociology of science cannot assume this *a priori*, and certainly it is not implied by its proper commitment to value-freedom. (The question of whether a scientific community's accepted conception of rationality is rightly regarded as a *social* determinant will be discussed later.)

Finally, since VF does not imply NVF (3), a value-free sociology of science is not prohibited from employing the normative concept of scientific rationality, provided that this is used in a purely characterizing manner. This means that the particular conception of rationality involved, which should itself be appropriately operationalizable, must be clearly specified; that the appraising force of the concept's use in (cognitive) value-judgments be suspended; and that likewise suspended is any claim to the effect that this particular conception of scientific rationality is itself correct or justifiable. These characterizing uses of the concept of rationality are thus compatible with both relativist and non-

relativist views about scientific rationality. Further, the possibility of such uses is itself supported by the relativist's claim that while no scientific belief can be shown to be rational *tout court*, what *can* be determined is whether a belief is rational with respect to some specified conception of rationality.¹⁷

Thus a value-free sociology of science may legitimately address questions such as the following: What are the effects of different forms of organization within scientific communities on their ability to generate rational scientific beliefs; what kinds of political and economic systems either favor or undermine these forms of organization, and hence their respective likelihoods of success in producing such beliefs; what, if any, are the cultural preconditions for the practice of rational scientific belief-formation; and so on?¹⁸ Such questions—like their counterparts concerning the relationships between economic systems and distributive justice—are clearly of some political significance. A sociology of science which refused to address them would be considerably impoverished, but fortunately the doctrine of value-freedom does not impose this self-denying ordinance.

Yet neither does it protect the sociologist of science from criticism which is directed against the conception of scientific rationality that has been employed in asking such questions, and which thereby also challenges whatever political conclusions might otherwise be drawn from the suggested answers. If the theory proposed has used a conception of rationality that the critic regards as defective or unjustifiable, then the significance of its results may legitimately be challenged. The sociology of science is as vulnerable as any other area of social theory to the charge of providing excellent solutions to pseudo-problems or poor solutions to genuine ones. One can construct a successful science of pseudoscience or an unsuccessful science of genuine science. What the doctrine of value-freedom insists is only that the two kinds of failure be kept distinct.

However the reference here to the ideas of genuine and pseudoscience raises two further issues about value-freedom in the sociology of science which somewhat complicate the preceding account. The first is this. It might be argued that the concept of rationality is so central to what is meant by calling something a (genuine) science, or by terming a belief (genuinely) scientific, that sociologists of science must employ some reasonably plausible conception of rationality in identifying their domain of inquiry. Otherwise, their work simply would not deserve the title of a sociology of "science." So the characterizing use of conceptions of rationality is not merely permissible, as has so far been maintained; rather it is essential.

This line of argument has sometimes been offered as a criticism of the SP, whose proponents have generally claimed that the program *must* define its object-domain only as consisting of whatever has been *regarded as* scientific by the members of relevant social groups—apparently on the grounds that any "epistemologically" based definition would fall foul of the program's own commitment both to

(cognitive) value-neutrality and to relativism. I have already argued that neither of these provides grounds for prohibiting the characterizing use of conceptions of rationality, and hence that sociologists of science are perfectly at liberty to define their object-domain in ways that make use of such conceptions. However, what of the claim that this is not merely optional but required?

While this view has certain merits, its critical implications are less far-reaching than may initially appear. First, those who are disinclined—for whatever reason—to accept this requirement can respond by allowing that their work is to be taken “only” as a sociology of what are regarded as scientific beliefs by certain social groups, and by retitling their inquirers accordingly. Next, it may in any case turn out that the set of beliefs they thereby investigate more or less coincides extensionally with those that satisfy the conception of genuine scientificity insisted upon by their critics. Finally, the actual explanatory claims made by the sociologist concerning “beliefs that are merely regarded as scientific” are not themselves rendered defective by the possible failure of those beliefs to satisfy an epistemologically preferred criterion of scientificity

The second, and partly related, issue is this. I have so far assumed, for the sake of convenience, that conceptions of scientific rationality involve only the specification of what standards have to be met by a belief for its possible acceptance to be rational. But it may be argued that for someone’s actual acceptance of a belief to be rational it is not sufficient that the belief meets the specified standards. What is also necessary is that the person’s acceptance of the belief is actually due to, or determined by, their regarding it as meeting those standards. Likewise, “genuinely scientific beliefs” are those which not only meet such criteria but are accepted because of their being thought to do so. That there are good reasons for a belief does not by itself make its acceptance by someone rational; the reasons must, as it were, be causally operative.

I shall call a conception of scientific rationality which includes this additional causal requirement, a “type-(2)” conception, to distinguish it from the “type-(1)” conceptions so far considered. There is no question here of debating their respective merits: they are designed for different purposes and are not in competition with one another. But it is worth noting briefly some implications of employing type-(2) conceptions of rationality in the kinds of characterizing use of normative concepts previously discussed. In particular, suppose that such a conception were employed in specifying the overall object-domain for a sociology of science. Then, in effect, a good deal of empirical investigation would be necessary before such a sociology could even begin: enough would have to be known about the determinants of “scientific” beliefs to ensure that they qualified as type-(2) rational beliefs.

There is nothing intrinsically objectionable in this—in adopting, that is, a theory-loaded criterion for domain-specification. Nonetheless, there are some persuasive pragmatic reasons against such use of

type-(2) conceptions of rationality in the sociology of science. In particular, an unnecessarily cumbersome terminology would be required in asking, as a substantive theoretical question, whether a scientific community's acceptance of type-(1) conceptions of rationality was in fact causally operative in determining their "scientific" beliefs, as distinct from other possible determinants. It is surely more convenient to be able to ask, as a question *within* the sociology of *science*, to what extent beliefs that are type-(1) rational have also been type-(2) rational. In what follows, therefore, I shall continue to use "rational" to mean type-(1) rational unless otherwise indicated.

V

It was noted at the outset that both advocates and critics of the SP generally agree that the programme entails cognitive relativism. By contrast I have been arguing that to the extent at least that the program is taken simply to specify the basic features of a properly scientific, and hence (cognitively) value-free, sociology of science there are no good grounds for thinking this. I have also argued that neither value-freedom nor cognitive relativism would justify the exclusion of conceptions of rationality from the theoretical vocabulary of such a "science of science"; nor would it rule out in principle the ascription of explanatory power to the conceptions of rationality accepted by members of a scientific community.

I shall now suggest some possible reasons which might lead an advocate of the SP to make these (in my view) mistaken claims, and thereby also potentially to confuse its critics. In doing so I shall return at times to various problems in the formulation of the program noted earlier. I begin by considering the program's "origins" in its opposition to the limitation thesis (LT), and suggest how mistakes about the SP's commitment to relativism could be generated by this.

According to what is in this context the relevant version of the LT, neither causal explanations in general, nor sociological explanations in particular, are to be provided for rational scientific beliefs, which are instead to be non-causally explained by the procedure of rational reconstruction. This basically consists in showing how they satisfy the requirements of some conception of rationality, and are hence such that it would be rational for members of the relevant scientific community to accept them. (Clearly, only a type-(1) conception of rationality would be suitable for this task.) For reasons by now familiar, the operation of this procedure requires at least that the conception of rationality be suitably operationalizable, so that it can be used to make purely characterizing claims about whether a particular belief is or is not rational. Such characterizing claims do not, of themselves, involve commitment to either a relativist or a non-relativist view of scientific rationality. But it can plausibly be argued that any advocate of the LT in this rational reconstructionist version is committed, at least

for all practical purposes, to rejecting cognitive relativism concerning scientific rationality.

This can be seen from the following considerations. Advocates of the LT are concerned to put boundaries upon the legitimate object-domain for a sociology of belief—to identify what can and cannot be subject to sociological explanation. But if a relativist view of scientific rationality were accepted, then either the boundaries drawn would, as it were, be constantly shifting in relation to each “equally (un)justifiable” conception of rationality; or it would have to be admitted that, in drawing a single determinate boundary by reference to just one such conception, a purely arbitrary decision was being made. Clearly, the central purpose of the LT would be undermined by these consequences of cognitive relativism.

Thus, while the procedure of rational reconstruction can be operated without commitment to non-relativism, its employment in the service of the LT does involve such commitment. Imagine, now, someone considering the merits of the LT, who is already convinced of the virtues of cognitive relativism—a person, say, who had been favorably impressed by the various broadly relativist views which emerged in the post-positivist epistemology of the 1960s and 70s, or who had indeed contributed to these. Such a person would legitimately regard the LT as absurd—as presupposing an archaically objectivist philosophy of science. Their opposition to the LT—and hence, also, their potential support for the SP—would thus be “based” upon their cognitive relativism, and quite reasonably so, in that relativism would indeed provide good grounds for rejecting the LT.

It might then be tempting for such a person to conclude that the SP itself requires the assumption of cognitive relativism and that the LT can only be rejected on this basis. But this conclusion could only be drawn if there were no other grounds for rejecting the LT—grounds which did not themselves entail cognitive relativism.

That there are such alternative grounds might plausibly be argued in the following way. The procedures of rational reconstruction and causal explanation are not mutually exclusive. They can both be applied to the same set of beliefs, since they are concerned with distinct questions—the former with justification, the latter with explanation. To show that a belief is justifiable one must show (at least) that it is reconstructible by reference to some particular conception of rationality. But success in so doing does not entail that the belief’s acceptance has no causal determinants. Nor, conversely, does the existence of such determinants entail that the belief is not justifiable, let alone that the very question of its justifiability is misplaced or illegitimate.

In other words, the LT could be rejected on the grounds that it misconceived the relationships between justification and explanation, and hence without assuming cognitive relativism. So although cognitive

relativists may reasonably deploy their relativism in supporting the SP against the LT, they would be wrong to conclude from this that support for the program requires commitment to relativism. And the same would hold for the program's espousal of (cognitive) value-freedom, expressed—albeit ambiguously—in the tenets of impartiality and symmetry (that is, tenets 2 and 3).¹⁹

How far this account of the hypothetical relativist's support for the SP is true of the program's *actual* proponents I shall not discuss here: its main purpose is heuristic, in clarifying the relationships between relativism, the LT, and the SP.²⁰ A similar point now applies in considering how it might be that advocates of the SP have mistakenly taken it to rule out any significant use of conceptions of rationality. I shall do this by commenting on the following passage from one of its actual proponents, but without claiming that this is strictly representative:

“The programme's endorsement of relativism means that it must seek to explain the content of scientific knowledge as far as possible in social terms. *Rationality* (whatever that means) must play little part in explaining how the world comes to appear as it does. Thus beliefs that seem less rational should be explained in the same way as those that seem more rational. Relativism is thus translated into symmetry and impartiality, in Bloor's terms.”²¹

The first question to raise is what exactly is meant by saying that “rationality” has little explanatory power with respect to scientific beliefs. If the rationality of a belief consists in its meeting the requirements of some (type-1) conception of rationality, then quite clearly this feature of the belief cannot (causally) explain its acceptance by anyone: to think that it could, would involve confusing the task of justification with that of causal explanation. But this cannot be the point being made here since what is claimed is that rationality has little explanatory power, not that it (necessarily) has none at all.

Perhaps, then, what is meant by “rationality” is instead the conception(s) of rationality actually accepted by the members of a scientific community, and it is these that are being said to play a minor role in explaining scientific beliefs. But if so, it is difficult to understand why this should be thought either to follow from, or to be necessarily assumed by, the SP—nor would the program's supposed relativism add plausibility to the thought. For the claim that “accepted values” have *little* explanatory power is basically an assertion of sociological materialism, and there seems nothing in the definition of the SP that would commit its proponents to this substantive theoretical position.

But there is a complication here. Perhaps it is being assumed in this passage that “accepted conceptions of rationality” are not properly categorized as *social* determinants, and that therefore the SP can ascribe little explanatory power to them simply because of its exclusive concern with the *social* determination of scientific beliefs. But there are two difficulties with this. First, it seems both

highly implausible and unduly restrictive to adopt a definition of ‘sociality’ according to which accepted values turn out to be non-social.²² If one considers what has been suggested throughout to be the analogous case of “accepted moral and political values,” then such a definition would entail that significant modes of “social” theory are not *social* at all—for instance, the Weberian theory of the origins of capitalism. It would surely be preferable to regard the disputes between Marxists and Weberians as substantive matters *within* social theory rather than between social and non-social theories.

The second difficulty is this. Even if so unduly restrictive a definition of sociality were accepted, the minor explanatory role of accepted conceptions of rationality would be entailed by the SP only if it were itself formulated in a very “strong” manner. It was noted earlier (in II above) that the program’s first tenet does not specify the relative importance of social, as compared with non-social, determinants of beliefs. If tenet 1 is taken to claim only that social factors always play at least *some* significant part in the determination of scientific beliefs, then the question of whether this is a predominant part is left open. If, alternatively, tenet 1 is taken as claiming that non-social determinants never exercise more than a minor influence, then certainly it would follow that accepted conceptions of rationality (if deemed to be non-social) have little explanatory power. It should be noted, however, that several critics of the empirical research conducted in the name of the SP have argued that there is little if any evidence to support the substantive truth of tenet 1, interpreted in this way.²³

Hence, the claim that accepted conceptions of rationality have little explanatory power is entailed by the SP only if one adopts both a strong reading of tenet 1 and a restrictive definition of sociality, neither of which are especially plausible. But this is not the only point at which lack of clarity or actual ambiguity in the SP’s tenets may give rise to the illusion that certain substantive theoretical claims about the determinants of scientific beliefs can be established merely by the methodological requirements of a scientific sociology of science. Similar problems may afflict tenets 2 and 3, especially the latter—the symmetry thesis. Understood as an attempt to express the requirement of (cognitive) value-freedom, tenet 3 should be read in the following way: the question of what determines the acceptance of scientific beliefs is to be answered independently of any judgments about their rationality, that is, independently of any favorable or unfavorable appraisals of those beliefs based upon their meeting or not meeting the requirements of some preferred conception of scientific rationality.²⁴ And this, of course, rightly makes the assessment of explanatory claims about the social determinants of scientific beliefs independent of the question of cognitive relativism, and more generally of what if any justification can be provided for such conceptions of rationality.

It is, nonetheless, quite tempting for the symmetry thesis to be understood in a significantly different way, namely, as asserting that the causal determinants of “rational” beliefs are in fact precisely the same as those of “non-rational” beliefs. Further, suppose that what is meant by “the same types of cause” is “in both cases predominantly social,” and that the concept of sociality is taken to exclude accepted conceptions of rationality. Then tenet 3 would be claiming that both rational and non-rational scientific beliefs have in common the absence of any major degree of causal determination by a scientific community’s accepted cognitive values. No doubt it is just conceivable that this is true. But it seems strange to insist that anyone endorsing the legitimacy of a scientific sociology of science must thereby assent to this apparently contentious substantive hypothesis.

Of course, most proponents of the SP would, in any case, not allow the formulation of any hypothesis of this kind, since they would regard its use of the concept of rationality as offending their, or their program’s, commitment to cognitive relativism and/or value-freedom. I have already argued that these objections are misconceived. But in the concluding remarks which follow I shall suggest that unless the SP is understood so as to legitimate the investigation of such hypotheses, the potential contribution of its empirical research to the issue of cognitive relativism will be greatly impoverished.

VI

To deny, as I have, that the SP either implies or necessarily assumes relativism may seem to presuppose a certain view of the relationship between the philosophy and the sociology of science, namely, that the former is a strictly autonomous enterprise to which the latter is entirely irrelevant. More specifically, according to this view the question of cognitive relativism (with respect to scientific rationality) is a purely philosophical one, to the resolution of which nothing can be contributed by the empirical results of a sociology of science. But this view is not thus presupposed, nor is it correct. For, although the philosophy and sociology of science are indeed distinct and mutually irreducible forms of inquiry,²⁵ neither are likely to be well conducted in isolation from the other—any more than are moral philosophy and the sociology of moral beliefs and practices. I shall now indicate briefly some of the possible, and complex, relationships between the two in the case of cognitive relativism.

First, sociological investigations might reveal one or both of the following: that the theoretical beliefs of various scientific communities do not conform to the conception(s) of rationality prescribed by certain philosophers of science, or that there is considerable diversity in the conceptions of rationality endorsed by members of different scientific communities. Concentrating here on the latter possibility, what would be the significance of such diversity for the preferred conception of rationality of a non-

relativist philosophy of science? Clearly this cannot be a matter of simple refutation: the actual diversity of accepted cognitive values no more entails cognitive relativism than the diversity of accepted moral values entails moral relativism. (It is, incidentally, curious how willing some cognitive relativists are to invoke an inappropriately refutationist logic in this context, while elsewhere deriding the absolutist pretensions of hypothetic-deductivist philosophy of science.)

Hence, there is nothing logically to prevent the non-relativist philosopher of science simply proceeding to criticize those scientific communities for endorsing defective cognitive values. Yet, to do this without pause for thought would be unwise. For, surely, these alternative conceptions of rationality should be treated with some respect—with at least as much respect, indeed, as the views of other philosophers of science whose conceptions of scientific rationality likewise differ from that which is being defended. The cognitive values endorsed by scientists are not *ipso facto* philosophically insignificant: one may, as it were, learn as much from arguing with Newton or Einstein about scientific rationality as from arguing with Mill or Popper.

Second, the sociology of science might not only discover the existence of such diversity but also propose certain explanations for this, for why it is that these varying conceptions of rationality have in fact been accepted by different scientific communities. But the bearing of such explanations on the question of cognitive relativism will depend at least partly on their actual character—on the kinds of (social) determinants supposedly identified. In particular, as argued earlier, one should not assume that any sociological explanation is necessarily materialist. Hence, for example, it might be that a scientific community's acceptance of certain cognitive values was largely due to the influence of broader philosophical or intellectual movements or traditions. If so, the problems thereby posed for the non-relativist philosopher of science would be no different from those posed more generally for philosophy by the historical and cultural diversity of such forms of thought.

But suppose instead that the proposed explanations were of a materialist nature. The analogy with moral relativism may again be helpful here. Consider the case of Marx's materialist explanations for the acceptance of dominant moral and political values. It is at least arguable that the truth of these explanations is consistent with a non-relativist position, and that Marx was himself, therefore, consistent in distinguishing between ideological and non-ideological values and in adopting a normatively critical standpoint toward the former. If so, the same might be true in the case of cognitive relativism and materialist explanations of accepted cognitive values. The significance of such explanations would not be that they directly establish relativism, but that they should make the non-relativist philosopher of science suspicious of the apparent attractiveness and superiority of their preferred conception of scientific rationality—suspicious that its till-now unacknowledged material determinants have imbued it with various elements of ideological distortion. But to be on one's guard

is one thing; to think there is nothing to be guarded is quite another.

Finally, sociological studies of science might reveal that while the conception(s) of rationality endorsed by particular scientific communities generally conformed to the preferred conception(s) of non-relativist philosophers of science, the theoretical beliefs held by members of those communities cannot be fully explained by their acceptance of those cognitive values. It might further be argued that had the scientists concerned relied exclusively on the considerations dictated by those standards of rationality, they would have been unable to make the necessary—albeit provisional—decisions that faced them in assessing the merits of possible theoretical beliefs: they would, as it were, have been forced into a situation of immobilizing agnosticism.

Now this possible conclusion of sociological inquiries would not straightforwardly support cognitive relativism. The non-relativist could still maintain that there were universally applicable “absolute” standards of scientific rationality. But what might have to be accepted is that by themselves these do not provide a complete basis for the kinds of cognitive decisions that have to be made in scientific practice, and that such decisions will always also require various informal, and in certain ways context-specific, modes of argumentation.²⁶ This does not, of course, imply that the philosophy of science is at this point simply replaceable by the sociology of science; rather it suggests that philosophical reflection on the concept of rationality must be informed by the practices of reasoning.

But what is also apparent, both from this and the previous cases considered, is that the significance of sociological investigations for the problems of relativism depends, not upon the mere existence of social determinants for scientific beliefs, but upon the specific character of those determinants; and that a sociology of science, whose results might contribute to the resolution of these problems, must not inhibit itself from deploying conceptions of scientific rationality in its theoretical work.

Notes and references

(I am grateful to my colleague John O’Neill for discussion of the issues addressed in this paper.)

1. See David Bloor, *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976); Barry Barnes, *Scientific Knowledge and Sociological Theory* (London: Routledge and Kegan Paul, 1974), and *Interests and the Growth of Knowledge* (London: Routledge and Kegan Paul, 1977). For a review of the Edinburgh School’s research see S. Shapin, “History of Science and Its Sociological Reconstruction,” *History of Science* 20 (1982). The Edinburgh School’s approach can be broadly distinguished from that of the “social constructionists” such as Bruno Latour and Steven Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (London: Sage Publications, 1979): see Paul Tibbetts, “The Sociology of Scientific Knowledge: The Constructivist Thesis and Relativism,” *Philosophy of the Social Sciences* 16 (1986). The disagreements emerge strongly in the exchanges between Woolgar and Barnes in *Social Studies of Science* 11 (1981).

2. As, e.g., in Larry Laudan, *Progress and Its Problems* (Berkeley: University of California Press, 1977), and Imre Lakatos, *The Methodology Scientific Research Programmes*, ed. J. Worrall and G. Currie (Cambridge:

Cambridge University Press, 1978); but I shall not restrict the concept of rational reconstruction to the particular standards of rationality endorsed by Laudan or Lakatos.

3. For criticisms of the SP see, e.g., W. H. Newton-Smith, *The Rationality of Science* (London: Routledge and Kegan Paul, 1981), ch. 10; Larry Laudan, "The Pseudo-Science of Science?" *Philosophy of the Social Sciences* 11 (1981); Roger Trigg, *Reality at Risk* (Brighton: Harvester Press, 1980), ch. 5; Martin Hollis, "The Social Destruction of Reality," in *Rationality and Relativism*, ed. M. Hollis and S. Lukes (Oxford: Basil Blackwell, 1982); Ray Pawson and Nicholas Tilley, "Monstrous Thoughts: Weaknesses in the Strong Programme of the Sociology of Science," *Occasional Papers in Sociology* no. 14 (Department of Sociology, University of Leeds, 1982); Ernan McMullin, "The Rational and the Social in the History of Science," in *Scientific Rationality: The Sociological Turn*, ed. R. Brown (Dordrecht: D. Reidel, 1984). For defenses see, e.g., Mary Hesse, "The Strong Thesis of the Sociology of Science," in *Revolutions and Reconstructions in the Philosophy of Science* (Brighton: Harvester Press, 1980); David Bloor, "The Strengths of the Strong Programme," *Philosophy of the Social Sciences* 11 (1981); Barry Barnes and David Bloor, "Relativism, Rationalism and the Sociology of Knowledge," in *Rationality and Relativism*; Peter T. Manicas and Alan Rosenberg, "Naturalism, Epistemological Individualism and 'The Strong Programme' in the Sociology of Knowledge," *Journal for the Theory of Social Behaviour* 15 (1985). (The voluminous literature on the SP can usefully be approached through the pages of *Social Studies of Science* and *Philosophy of the Social Sciences* from the late 1970s.)

4. For example, H. M. Collins, "Stages in the Empirical Programme of Relativism," *Social Studies of Science* 11 (1981).

5. There may be grounds for believing that the SP entails a relativism which would remain unaffected by my arguments concerning value-freedom. Nonetheless, confusion about the issues I address here underlies a good deal of the debate about the program's supposed relativism.

6. As presented in Bloor, *Knowledge and Social Imagery* pp. 4-5.

7. See especially Laudan, "The Pseudo-Science of Science?" and Bloor's reply, "The Strengths of the Strong Programme."

8. On this see Hesse, "The Strong Thesis," and G. Freudenthal, "How Strong Is Dr. Bloor's Strong Programme?" *Studies in the History and Philosophy of Science* 10 (1979).

9. On Barnes' use of the concept of natural rationality see Manicas and Rosenberg, "Naturalism, Epistemological Individualism and 'The Strong Programme'."

10. On the interpretation of tenets 2 and 3, and for a revised formulation offered later by Bloor, see Newton-Smith, *The Rationality of Science*, pp. 247-252.

11. As, e.g., in the Introduction to Jack W. Meiland and Michael Krausz, eds., *Relativism: Cognitive and Moral* (Notre Dame and London: University of Notre Dame Press, 1982). I make no attempt to justify the specific formulations of relativism adopted in what follows, assuming merely that they are well recognizable as such: for instance, my account of "relativism with respect to scientific rationality" corresponds closely to the way Hollis and Lukes define "relativism of reason" in their Introduction to *Rationality and Relativism*. But my later use of the phrases 'cognitive values' and 'cognitive value-freedom' suggests that at least the terminology of the "cognitive v. moral" distinction is misleading.

12. Many conceptions of scientific rationality are, of course, a good deal more complex than the two noted here—e.g., Lakatos' sophisticated methodological falsification. Most involve rules relating "evidence" to "theory," and some also "theory" to "theory"; and distinctions may be drawn between "ideal" and "available" evidence. Those which refer to "empirical data" may be subject to problems connected with other forms of epistemological relativism, but I shall not consider these here. I shall also ignore distinctions between conceptions which allow for "degrees of rationality" and those which do not.

13. For an attempt to support the claims about value-freedom made here see the opening chapters of Russell Keat, *The Politics of Social Theory* (Chicago: Chicago University Press, 1981), which develops further the position defended in chapter 9 of Russell Keat and John Urry, *Social Theory as Science* (London: Routledge and Kegan Paul, second edition, 1982).

14. Here as elsewhere in the paper I leave aside problems concerning the criteria for correct interpretations of beliefs, which raise issues quite distinct from those with which I am directly concerned.
15. The distinction here between characterization and appraisal derives partly from E. Nagel, *The Structure of Science* (London: Routledge and Kegan Paul, 1961), pp. 491-494.
16. Nor is this the only respect in which scientific work is always necessarily more than an exclusively cognitive enterprise and must therefore be judged accordingly
17. Indeed, proponents of the SP sometimes defend themselves against the charge that their relativism undermines their own scientific claims by saying that these meet the requirements of “local” standards of rationality. In doing so, they implicitly accept what they elsewhere deny, namely, that scientific beliefs can be “characterized” as rational or irrational.
18. These are the kinds of questions addressed by “Mertonian” sociology of science—see, e.g., Robert K. Merton, *The Sociology of Science* (Chicago: University of Chicago Press, 1973). Proponents of the SP, in rightly asserting the possibility of social determination for the specific content of scientific beliefs as distinct from the institutional requirements for the practice of “good science,” have wrongly concluded that questions about the latter must be excluded. On the opposition of the Edinburgh School to the Mertonians see H. M. Collins, “The Sociology of Scientific Knowledge: Studies of Contemporary Science,” *Annual Review of Sociology* 9 (1983).
19. Here, as throughout, I assume that tenets 2 and 3 are best understood as an (admittedly confused) attempt to express the idea of cognitive value-freedom. Certainly I can see no other legitimate rationale for them, and that Barnes, at least, has this idea in mind is indicated by his comment that “as a methodological principle we must not allow the evaluation of beliefs to determine what form of sociological account we put forward to explain them” (*Interests and the Growth of Knowledge*, p. 25).
20. See Collins, “The Sociology of Scientific Knowledge”, for useful information on the prior intellectual commitments of various proponents of the SP. In the case of Bloor the influence of a certain interpretation of Wittgenstein and Durkheim is evident and further developed in his later *Wittgenstein: A Social Theory of Knowledge* (New York: Columbia University Press, 1983). It is, I believe, unfortunate that the SP has been so closely associated by both advocates and critics with the broadly relativistic tendencies in recent philosophy of science, since neither of the two actually has much to do with the other.
21. Collins, “The Sociology of Scientific Knowledge,” p. 272: while not altogether representative, this passage illustrates some quite widespread confusions in the literature on the SP.
22. Hence I am unhappy with the terms used by McMullin, in “The Rational and the Social,” to distinguish what he calls “epistemic” and “social” factors: the former correspond closely to my “accepted (cognitive) values.” But the difference is mainly terminological, since he regards both as legitimately occurring in causal explanations of scientific beliefs and rightly criticizes proponents of the SP for what is, in my terms, their *a priori* materialism.
23. See, e.g., McMullin, “The Rational and the Social,” and Pawson and Tilley, “Monstrous Thoughts”.
24. Here as throughout I consider tenet 3 in relation only to rationality/irrationality, and not to truth/falsity: but similar arguments could be developed to deal with the latter also. I therefore disagree with, e.g., Newton-Smith, who argues in *The Rationality of Science*, pp. 252-253, that in the case of simple perceptual beliefs the explanation of true beliefs necessarily differs from that of false ones. Whether there are such differences is, in my view, an empirically open question, and hence not one whose answer depends on prior assessments of truth and falsity.
25. Much of the SP’s supposed relativism is due to some of its proponents’ commitment to the reduction of philosophical to sociological enquiry. But no such reduction is required by the program itself: nor do I find attractive the revival of nineteenth-century positivistic scientism which this reductionism often implicitly involves (see *The Politics of Social Theory* ch. 1). For example, when Barnes and Bloor proclaim that “Far from being a threat to the scientific understanding of forms of knowledge, relativism is required by it,” they seem to assume that science provides the only kind of knowledge one could have about science: see “Relativism, Rationalism and the Sociology of Knowledge.” p. 21.

26. And hence, also, that scientific rationality is not algorithmic. The apparent relativism of “social constructionists” such as Woolgar and Latour (see note 1 above) seems often to rely on the mistaken assumption that scientific rationality must be algorithmic, together with a curious espousal of Bridgman-esque operationalism in a new, sociological guise.