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Will the Real Scientists Please Stand Up? Dead Ends and Live Issues in the Explanation of Scientific Knowledge

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Reflection on the method of science has become increasingly thinner since Kant. If there's any upshot of that part of modern philosophy, it's that the scientists didn't have a secret. There isn't something there that's either effable or ineffable. To understand how they do what they do is pretty much like understanding how any other bunch of skilled craftsmen do what they do. Kuhn's reduction of philosophy of science to sociology of science doesn't point to an ineffable secret of success; it leaves us without the notion of the secret of success.

Relativism is the view that every belief on a certain topic, or perhaps, about any topic, is as good as every other. No one holds this view. Except for the occasional co-operative freshman, one cannot find anybody who says that two incompatible opinions on an important topic are equally good. The philosophers who get called 'relativists' are those who say that the grounds for choosing between such opinions are less algorithmic than had been thought.

Richard Rorty[1,2]

What role do norms—roughly, principles prescribing the conduct and evaluation of research—play in accounting for belief acceptance and change in scientific disciplines? Norms, in this sense, are the central ingredients of scientific method. Scientific method, whatever its details, functions ideally as an epistemic screen which, properly applied, sifts beliefs that are rationally warranted from the rest. Questions of whether or how appeals to notions of such a method suffice to account for observed discriminations made among beliefs in a scientific community arise both synchronically and diachronically. Considered diachronically, e.g. cases where one theory

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[1]'Reply to Dreyfus and Taylor', *Review of Metaphysics* **24** (1980), p. 55.
[2]'Pragmatism, Relativism, Irrationalism', in *Consequences of Pragmatism* (Minneapolis, MN: University of Minnesota Press, 1982), p. 166.

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displaces another in a specific science, the issue concerns changes in theoretical fashion. Are these explicable based on evidence available in the particular subject area, or are the engines of change driven by extra-scientific dynamics? Synchronically, questions arise regarding the actual role norms play in explaining the views scientists currently entertain about the subject matter in their field. Quarrels develop between, on the one hand, those who maintain that scientific method accounts for the fine-grained discriminations scientists make and, on the other, those who see any such method as far too coarse a device for the purposes ascribed to it. In all of these cases, contrasting practices of explanation confront and challenge one another, characteristically denying legitimacy to alternative explanatory approaches.[3]

Yet the persistence of these debates should not lead to the conclusion that the issues ostensibly separating the disputants are, in fact, well or even rightly drawn. For example, Steven Shapin, after tracing the trajectory of the externalism/internalism debate (in science studies) over the last fifty years, concludes that the primary terms of the debate were never well-defined.[4] 'The history of e/i [externalism/internalism] discourse is one of insufficient scepticism about the sense and application of those locutions'.[5] Shapin's point is well-taken, but his warning against taking these debates at face value has been neither fully appreciated nor systematically applied.

In this regard, the epigrams from Rorty suggest two cautions with regard to the ongoing and seemingly irresolvable disputes among the various proponents of

[3]The 'contrasting practices of evaluation' alluded to in the opening paragraph includes a wholesale disagreement between philosophers and sociologists of science with regard to the analysis of norms. On the one hand, philosophers concern themselves with the norms *qua* norms, i.e. as genuine methodological 'oughts' prescribing the course of inquiry. Sociologists of science, on the other hand, take the stated norms of science descriptively, as part of a repertoire of rationalizations that can be variously deployed as the situation warrants. As Michael Mulkay put it in one early formulation of the sociological challenge, 'the standardized verbal formulations to be found in the scientific community provide a repertoire which can be used flexibly to categorise the professional actions differently in various social contexts and, presumably, in accordance with varying social interests. It seems to me misleading to refer to this diffuse repertoire of verbal formulations as the normative structure of science or to maintain that it contributes in any obvious way to the advance of scientific knowledge.' ('Norms and ideology in science', *Social Science Information* **15** (1976), 637–656, p. 645.) What one might call the 'full sociological turn' in science studies comes just here, with the claim that Merton's 'original functional analysis did identify a genuine social reality, but one better conceived as an ideology than as a normative structure' (*ibid.*, p. 646) and the attendant call for social scientists to no longer accept this ideology at face value (*ibid.*, p. 654).

In this regard, the first part of this paper examines how philosophers have reacted to the sociological turn, while the second part chronicles reactions within social studies of science to some of the directions taken in this field. However, the fixed point of concern remains understanding how norms are invoked by either philosophers or sociologists by way of justifying the picture of science *they* produce.

There are two excellent survey articles on these general issues by Steven Shapin. See his 'History of Science and Its Sociological Reconstructions', *History of Science* **20** (1982), 157–211, and 'Discipline and Bounding: The History and Sociology of Science as Seen Through the Externalism–Internalism Debate', *History of Science* **30** (1992), 333–369.

A case study challenging the role of scientific method in both synchronic and diachronic accounts of belief maintenance and change is Andrew Pickering's *Constructing Quarks* (Chicago: University of Chicago Press, 1984). Pickering denies an explanatory role to scientific method along both axes, preferring to account for belief maintenance synchronically by the 'symbiotic' relation of theoreticians and experimenters in physics, and for change diachronically by what he calls there 'opportunism in context'.

[4]Steven Shapin, 'Discipline and Bounding', *op. cit.*, note 3.

[5]*Ibid.*, p. 360.

differing explanatory strategies in science studies. One concerns whether the acceptance or rejection of relativism identifies a major point of difference in competing explanatory strategies. In this context, I argue that the term functions in these debates to divert attention from the main point of epistemological disagreement. On examination, relativism stands revealed as a non-issue in controversies that appear to feature it. A second lesson to be gleaned from these debates is that, *contra* appearances, the principles in them remain wedded to a very conventional notion of science. Disputants remain caught in the allures of thinking they have something to gain by arguing over the nature of science despite the compelling evidence available from research and practice from all sides that science has no nature.

In the first section, I show the irrelevance of relativism in many debates about norms and their role in explanation. Sections II and III identify some implications of taking seriously critiques of the notion of a scientific method.

I. Will the Real Relativists Please Stand Up?

Sociologists of scientific knowledge [SSK], e.g. Barnes, Bloor, Pickering, Pinch, and Collins, often use the term 'relativist' and its cognates when characterizing their intellectual position. For example, in their polemical and much cited essay, 'Relativism, Rationalism, and the Sociology of Knowledge',[6] as well as in writings of more recent vintage,[7] Barnes and Bloor use this label for the brands of sociology of science they prefer (whether straight strong programme or related flavors).[8]

> There are many forms of relativism and it is essential to make clear the precise form in which we advocate it. The simple starting-point of relativist doctrines is (i) the observation that beliefs on a certain topic vary, and (ii) the conviction that which of these beliefs is found in a given context depends on, or is relative to, the circumstances of the users. But there is always a third feature of relativism. It requires what may be called a 'symmetry' or an 'equivalence' postulate....
>
> ... Our equivalence postulate is that all beliefs are on a par with one another with respect to the causes of their credibility....[That is] regardless of truth and falsity the fact of their credibility is to be seen as equally problematic.[9]

[6]In M. Hollis and S. Lukes (eds), *Rationality and Relativism* (Cambridge, MA: MIT Press, 1982), pp. 21–47.

[7]See Barry Barnes, 'How Not to Do the Sociology of Knowledge', *Annals of Scholarship* **8** (1991), 321–335.

[8]This characterization of their own position can be found in even early writings, such as Bloor's *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976), and Barnes' *Interests and the Growth of Knowledge* (London: Routledge and Kegan Paul, 1977). Harry Collins embraces this characterization as well: see 'What is TRASP? The Radical Programme as a Methodological Imperative', *Philosophy of the Social Sciences* **11** (1981), 215–224, esp. p. 216.

[9]Barnes and Bloor, *op. cit.*, note 6, pp. 22–23.

These sociologists term themselves 'relativists', taunting critics who believe it to be a badge of shame. The appropriateness of this characterization remains widely accepted.[10]

Note, however, that (i) and (ii) above—the observation that beliefs vary, and that this variation is ascribed to the contingencies of circumstance—are idle with respect to establishing relativism. For it is of no epistemological consequence if beliefs, as a matter of fact, vary. An issue only exists, for purposes of setting up a 'competition' between philosophical and sociological explanations, on the assumption that variations in belief cannot be accounted for by the sort of factors one group is prone to emphasize, e.g. in the case of philosophers, rationally persuasive reasons. Conversely, the motivation for the sociological turn in science studies is the perceived failure of the factors philosophers favor to explain variations of beliefs among scientists in historically interesting cases. That is, (ii) needs to be read as claiming that variation persists because those factors that, on standard philosophical accounts, ought to settle such disagreements in belief generally prove to be incapable of doing so. Without this reading of (ii), there is no basis for quarrel in the academy.

What distinguishes the form of relativism defined by (i)–(iii) is (iii)—the symmetry principle. Symmetry holds that all beliefs, whatever their epistemic or semantic status, are equally in need of explanation. The type of explanation, as another tenet of the Strong Programme (SP) stipulates, is to be causal. Yet the symmetry principle, for its part, derives whatever plausibility it has from its conjunction with standard skeptical and critical arguments concerning the failures of idealized standards of reason to account for actual cases of belief change in the natural sciences.[11] So, the reading suggested above of (ii) is critical not only to making sense of the claim that sociologists better explain conclusions drawn in practice by scientific communities than do philosophers, but also to motivating (iii). For it is the alleged inability of evidence to decide when experiments end that makes the actual epistemic status of a belief appear irrelevant for purposes of explaining observed variation.

But is it the case that the primary forms of SSK constitute, by virtue of (i)–(iii), some interesting form of relativism? The line of allegedly relativist reasoning here goes as follows. Since the factors favored by the friends of impartial reason are inadequate to account for why some beliefs are accepted and others not, a belief's actual epistemic or semantic status is of no explanatory utility. The plausible further inference is that what will be of explanatory utility are other sorts of considerations, primarily those generally held not to be relevant to underwriting the rationality of a belief. Put another way, ideals of reasoning characteristically portray the process of belief evaluation as something an individual could do if alert to the 'correct' standards. However, the more contextually embedded the decision process turns out to be, i.e.

[10]Evidence for this is the existence of the volume in which the Barnes and Bloor essay appears, *op. cit.*, note 6.

[11]This is explicit in Barnes and Bloor, *op. cit.*, note 6, p. 22.

the more evaluation responds to contingent local conditions, the less it conforms to, and so is explicable by, such ideals of evaluation. In short, appeals to objective standards of rationality presumed available to agents are found to be idle for purposes of explanation. The focus on local practices of reasoning and the eschewal of the utility of universal standards suggests the label 'relativist' for this approach to explanation.

In this regard, the proverbial strength of the strong programme is taken to be the numerous historical and case studies claiming to show the comparative fruitfulness (for purposes of explanation) of contextually embedded accounts of belief evaluation. Causal explanations (of the sociological variety) explain by showing how such circumstances impinge on the individuals to engender the beliefs they have. Hence, a neat contrast between individual-based rationality accounts of belief evaluation and sociologically oriented causal explanations.[12]

Putting matters this way helps highlight the fact that the challenge actually posed by the symmetry principle is *not* directed at the notion of a scientific explanation as such, but at the role of human agency in belief evaluation. Indeed, while the claim to explanatory relativism is tied, within SSK, to its stress on the explanatory importance of so-called local factors,[13] complaints about their relativism focus rather on their slighting of a role for agency in explanation.

For example, Martin Hollis, in his criticisms of the strong programme's rejection of rationality-based explanations, clearly identifies their eschewal of such explanations as the basis for their alleged relativism. Agency, Hollis dourly observes, can only be preserved by maintaining that people have the capacity to choose, based on independent standards, what to believe.[14]

> That the final determinants of belief are human, yet not intellectual, is an idea as old as skepticism....[Hume] was skeptical about Reason but confident about science and the tracing of our beliefs to custom and imagination was to be a scientific project. Philosophers have not, on the whole, taken kindly to his proposal..., but sociologists of knowledge have carried it a great way. Of late a 'strong programme' has crystallized, intended to deprive Reason of all her traditional autonomy and to place the study of the social world on a thoroughly scientific footing.[15]

Hollis rightly appreciates the Humean/skeptical roots of the strong programme

[12]As will become evident later, this project is closely allied to Quine's proposal to naturalize epistemology. Collins, *op. cit.*, note 8, explicitly contrasts what he sees as the weaknesses of rationality explanations of scientific beliefs and the merits 'hidden hand' accounts of same.

[13]The reason for speaking of 'so-called local factors' will become evident below. For the moment, however, the point is to emphasize that the choice of the term 'local' in the context of this dispute is to underline that the appeal, for purposes of explanation, is not to some universally applicable standard for the having of good reasons. What makes for the goodness of reasons, on the sociological account, is purely a contextual matter. If all politics is local, so too, on this view, is all acceptable reasoning.

[14]See Martin Hollis, 'The Social Destruction of Reality', in Hollis and Lukes, *op. cit.*, note 6, pp. 67–86. Hollis repeats this argument in 'Social Thought and Social Action', in E. McMullin (ed.), *The Social Dimension of Science* (Notre Dame, IN: University of Notre Dame Press, 1992), pp. 68–84.

[15]Hollis, 'The Social Destruction of Reality', *op. cit.*, note 14, p. 67.

proposal. Yet, somewhat oddly given his acknowledgment that the tension is between causal and rational explanations, Hollis insists, *on the basis of that conflict*, that the sociologists are relativists. That is, it is by virtue of insisting on causal explanations of beliefs that, according to Hollis, the 'programme is meant to rot away the props of a familiar notion of objectivity'.[16] Yet the 'familiar notion of objectivity', which falls away on this account, is one, as Hollis elsewhere acknowledges, that is replaced in its turn by 'a determinist picture of the shaping of beliefs by social conditions ... [and so] leaves small place for human agency as a motor concept'.[17] But what makes a determinist account, *ipso facto*, a relativist one?

Close attention to Hollis' remarks suggest that his complaints about relativism simply mask a rather different point of contention between Hollis and SSK, namely whether, or under what circumstances, causal explanations of human behavior are to be preferred over ones grounded in agents' rationales for their actions. More generally, the following logical slide takes place. Hollis correctly notes that (a) the sort of causal accounts promoted by SSK emphasize contextual and contingent factors influencing the evaluation of beliefs, and (b) that norms, and so agents' reasons citing those norms, are taken to have no explanatory utility. But, from (a) and (b), Hollis concludes that (c) the emphasis on local as opposed to general standards of reasoning means that sociological explanations are commuted to relativism.

'Relativism' here means beliefs are to be causally explained by contingent local factors, not by individuals' use of norms. But in order to obtain a sort of relativism which is philosophically interesting, one must ignore the call for causal explanations embedded in (a). For otherwise doing away with 'a familiar notion of objectivity'—the idea of reasons as logically determinative—is not to do away with objectivity—a standard which provides a general criterion for evaluating the 'goodness' of an explanation. The demand that explanations must be causal ones provides a non-relativist standard of evaluation. Hence (c) cannot be inferred from (a), i.e. one cannot legitimately slide from the charge that SSK accounts ignore 'a familiar notion of objectivity' to the conclusion that they invoke no independent standard.

Interestingly enough, Hollis is far from alone in making this slide. One finds similar reasoning in complaints against the SSK entered by Ronald Giere,[18] Philip Kitcher,[19] Thomas Kuhn,[20] and Larry Laudan.[21] SSK is identified as holding three theses: (a) advocacy of causal explanations of beliefs; (b) rejection of reason explanations; and

[16] *Ibid.*, p. 69.

[17] *Ibid.*

[18] Ronald Giere, *Explaining Science* (Chicago, IL: University of Chicago Press, 1988).

[19] Philip Kitcher, *The Advancement of Science* (Oxford: Oxford University Press, 1993), and 'Contrasting Conceptions of Social Epistemology', in F. Schmitt (ed.) *Socializing Epistemology* (Lanham, MD: Rowman & Littlefield, 1994), pp. 111–134.

[20] Thomas S. Kuhn, 'The Trouble with the Historical Philosophy of Science', Robert and Maurine Rothschild Distinguished Lecture (Cambridge, MA: Department of the History of Science, Harvard University, 1992).

[21] Larry Laudan, 'The Pseudo-Science of Science', *Philosophy of the Social Sciences* **11** (1981), 173–198.

(c) emphasis on the importance of contextual (local) factors for purposes of explanation. Symmetry becomes associated with (c) inasmuch as (c) is given as a (the?) reason for treating all beliefs as plausible candidates for causal explanation. Moreover, (c) is also the SSK's basis for claiming to be relativists.

However, as in the case of Hollis, the actual dispute between them and the sociologists centers on the fact that, for purposes of accounting for the role of norms in belief evaluation, sociologists affirm (a) and affirm (b), while the aforementioned philosophers all deny (b).[22] Yet the argument slips into suggesting that the conflict is whether or not one accepts (c). Yet while (c), taken in isolation, may be given a relativist reading, it may also consistently be conjoined with either (a) or the denial of (b). Linked with (a), local factors may be understood as instantiating the antecedent condition for some causal law. If good reasoning is taken as explanatory, i.e. (b) is denied, then one may construe the historically contingent factors as still providing such reasons. Examples of this are found in the historical accounts advanced by Giere[23] and Kitcher[24] against the sociological claims that reasons cannot be assumed to be explanatory of how beliefs come to be evaluated.

Kuhn, for his part, while acknowledging that the difference between SSK-style accounts and the style he favors is in terms of the *types* of explanations each side offers, nevertheless also characterizes his disagreement with the SSK as if they took an anti-science stance.[25] However, the issue here, *contra* Kuhn, is not *whether* science produces knowledge, but *which* science produces it. Likewise, Laudan recognizes that the challenge posed by SSK is to the type of explanation he favors, namely one in terms of the norm-bound rationality of the scientists.[26] Despite this, he too succumbs to identifying the problem as the relativism implied by (c), and the associated symmetry principle.[27] Even Giere, although offering an even-handed summary of the sociological position that explicitly recognizes that the sociologists favor different factors than he does for purposes of explanation,[28] nonetheless goes on to write as if the incompatibility is between attributing rationality to agents and some type of relativism stemming from (c).[29]

Kitcher, like Kuhn and Giere (and unlike Hollis and Laudan), maintains that philosophers have much to learn from sociological studies of laboratory life. He appreciates, as well, that the battle lines are drawn with regard to the issue of rationality versus causal explanations of belief maintenance and change within

[22]Differences exist among philosophers regarding the role causal accounts—(a)—may play.

[23]See Section II below.

[24]See, in particular, Kitcher, *op. cit.*, note 19, chs. 2, 3, 6 and 7. In addition to the works of Giere and Kitcher, Peter Galison offers a case study approach that favors reason explanations: see his *How Experiments End* (Chicago, IL: University of Chicago Press, 1987).

[25]Kuhn, *op. cit.*, note 20, pp. 7–9.

[26]Laudan, *op. cit.*, note 21, p. 176.

[27]*Ibid.*, p. 184.

[28]Giere, *op. cit.*, note 18, pp. 52–61, esp. pp. 52 and 56.

[29]*Ibid.*, pp. 107–108.

scientific communities.[30] In this regard, Kitcher seeks to meet the SSK challenge regarding explanation head on, i.e. he argues directly against (b), the alleged inadequacy of norm-based reason explanations.[31] This strategy allows him to agree with Bloor *et al.* that any account of reasoning needs to take account of psychological factors—the localism mandated by (c)—and yet resist (a).

Kitcher, interestingly enough, also denies that the symmetry principle leads to relativism. 'The popular belief that acceptance of the Symmetry Principle thus leads to relativism seems to me to be quite mistaken'.[32] However, his reasons for maintaining this are quite different than those rehearsed above and illustrate the misdirection of issues that plague these debates. For Kitcher distinguishes between what I shall term 'strong' and 'weak' readings of the symmetry. The 'weak reading' of symmetry says only that epistemology cannot be apsychologistic, i.e., it must take account of actual mechanisms relevant to epistemic evaluation. This Kitcher finds unobjectionable, since he takes it as compatible with traditional sorts of epistemological projects, e.g. some form of reliablism.

> Thus, in a certain sense, all the conceptions of social epistemology so far envisaged—even the most traditional—honor the Symmetry Principle. All suppose that 'the same types of causes' must be invoked to explain both true and false beliefs. For, if we are out to explain X's belief that *p*, we shall surely do so by identifying X's cognitive capacities, X's interactions with reality, and X's social background. Even in cases of perception, X's socialization will be relevant....Hence, *at a very general level*, the same types of causes will be invoked to understand any belief, irrespective of its truth value.[33]

'Weak' symmetry is simply a truism to the effect that facts about a person—training, education, etc.—will be relevant to any epistemic evaluation of whether or not X knows that *p*.

For Kitcher, in order to have symmetry 'imply more exciting, relativistic, conclusions, it is necessary to interpret it in a much stronger—and quite controversial—way'.[34] Kitcher's tack is then to suggest that any reading strong enough to license relativism is simply implausible. In particular, he develops what he acknowledges is a very narrow reading to the notion of 'type' when speaking of using 'the same type of causes' to explain both true and false beliefs. Thus, if one 'type' of belief forming process is becoming inebriated and forming beliefs in this condition, he reads 'strong symmetry' as mandating that this process 'should be invoked to explain the presence of both true and false beliefs *and indeed that each should be invoked equally often in this enterprise*'.[35] The same type of process—getting drunk and then seeing what one believes—must not only account for true and

[30]Kitcher, *op. cit.*, note 19, pp. 184–185.
[31]Kitcher, *op. cit.*, note 19, pp. 184–188.
[32]Kitcher, 'Contrasting Conceptions', *op. cit.*, note 19, p. 122.
[33]*Ibid.*, p. 120.
[34]*Ibid.*
[35]*Ibid.*, p. 121.

false beliefs on this reading, but account for them equally often (otherwise, no symmetry). This is relativism with a vengeance, since symmetry is taken to license the explanatory goodness of any belief forming process, and demanding it be used to explain equal numbers of true and false beliefs. Since these consequences are 'intuitively absurd',[36] so much the worse for the troublesome reading of symmetry.

However, there are problems both with Kitcher's effort to meet the arguments for (b) directly and for his conclusion that symmetry is either truistic or absurd. The first, which I mention only in passing here, is that Kitcher's strategy against (b) leads him to maintain that there is a fact of the matter regarding how to interpret texts (and presumably, determine which interpretation an author intended). 'But there is a fact of the matter: either Galileo's reasoning exemplified a strategy likely to promote cognitive goals or it did not'.[37] I have elsewhere argued against the plausibility and utility of any general assumption that there is such a fact of the matter to what someone means.[38] In addition, as emerges in the discussion in Section II, I am quite skeptical about uncovering a fact of the matter with regard to the correct interpretation of historical incidents.

Concerning his claim that the symmetry principle is either a truism or an absurdity, I have already adduced arguments to the effect that symmetry is compatible with either agency-oriented or causal analyses. Does this draw the teeth from the principle for purposes of debate about norms and explanation? I think not. On the one hand, the 'symmetry' called for is not so flat-footed a principle as to require parity of causal processes or types, as Kitcher's 'strong' reading insists. The symmetry demanded is in treating all beliefs, regardless of epistemic status, as causally explicable.[39] Symmetry just says no belief is exempt from causal explanation. Asymmetries arise if one insists that, e.g. only false beliefs, or the persistence of weak evaluative strategies, call for causal explanations. Asymmetric positions regard the rationality of a belief as its presumptive explanation. In this regard, despite Kitcher's otherwise quite sophisticated appreciation and treatment of the issues, his strong reading is just off the mark. On the other hand, Kitcher's truistic reading of symmetry is not innocuous. For it cedes to the sociologists all they ever demanded. This reading just underscores the central issue, namely whether explanations of belief should go fully causal.[40]

[36] *Ibid.*

[37] Kitcher, *op. cit.*, note 19, p. 186.

[38] Paul Roth, *Meaning and Method in the Social Sciences* (Ithaca, NY: Cornell University Press, 1987), chs 1–4, and Paul Roth, 'Politics and Epistemology: Rorty, MacIntyre, and the Ends of Philosophy', *History of the Human Sciences* **2** (1989), 171–191.

[39] 'The position [regarding symmetry] we shall defend is that the incidence of all beliefs without exception calls for empirical investigation and must be accounted for by finding the specific, local causes of this credibility. This means that regardless of whether the sociologist evaluates a belief as true or rational, or as false and irrational, he must search for the causes of its credibility'. Barnes and Bloor, *op. cit.*, note 6, p. 23.

[40] Questions understandably arise at this point with regard to how the philosophers just discussed manage to go, at one point or another, so awry in their characterization of the issues. My suggestion is that the

In any case, if relativism involves denying the universal applicability of some standard of belief evaluation, then most sociological accounts, insofar as they claim to be causal, are clearly not relativistic. For causality is the standard against which all candidate explanations must be evaluated. Thus, debates over relativism are besides the point with regard to the explanatory strategies in dispute.

Belief relativity, in the sociologically relevant sense, is not the endorsement of any position plausibly identified with epistemological relativism. Rather, agents' announced reasons for beliefs are just taken to be part of the *explanandum*—that which is to be explained—in the context of sociological explanations. What one finds in the *explanans*—the factors cited by way of accounting for the explanandum—consists of laws and statements of relevant conditions. Indeed, as James Bohman shows quite nicely, the account of science found in the writings of Barnes and Bloor looks pretty much like the standard hypothetico-deductive account.[41] In short, SSK takes its model of scientific explanation whole cloth from positivist philosophy of science. I conclude, then, that there are no relativists in these debates in any interesting sense of the term.[42]

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[40] *continued*
various mischaracterizations are symptomatic of a strong antipathy, certainly within philosophy of science, with respect to fully naturalizing epistemology. For, in its full-blooded Quinean form, naturalized epistemology just does replace explanations of belief in terms of agent evaluation by ones in terms of causes. It is neither an accident nor a mistake that Barnes, *op. cit.*, note 7, recognizes Quine *et al.* as kindred spirits. Indeed, debates *within* philosophy reproduce disputes concerning the role of norms found in the debates chronicled so far. See, for example, Hilary Kornblith, 'Introduction: What is Naturalistic Epistemology?', and Jaegwon Kim, 'What is "Naturalized Epistemology"?', both in H. Kornblith (ed.), *Naturalizing Epistemology*, 2nd edn (Cambridge, MA: MIT Press, 1994), pp. 1–13 and 33–56, respectively. Although the central debate about naturalizing epistemology focuses on the citing of norms vs the citing of causes, there are naturalizing accounts that stress the role of norms. See, e.g. Larry Laudan, 'Progress or Rationality? The Prospects for a Normative Naturalism', *American Philosophical Quarterly* **24** (1987), 19–31, or the recent book by Susan Haack, *Evidence and Inquiry* (Cambridge, MA: Basil Blackwell, 1993).

Likewise, the hostile reception of Richard Rorty's work, from the publication of his *Philosophy and the Mirror of Nature* (Princeton, NJ: Princeton University Press, 1979), is indicative of the problem. An appreciation of how philosophers shy away from the implications of naturalizing projects is found in Steve Fuller, 'Epistemology Radically Naturalized: Recovering the Normative, the Experimental, and the Social', in R. Giere (ed.), *Cognitive Models of Science* (Minneapolis, MN: University of Minnesota Press, 1992), pp. 427–459.

[41] James Bohman, *The New Philosophy of Social Science* (Cambridge: Polity Press, 1991), ch. 1. See also W. Newton-Smith, *The Rationality of Science* (London: Routledge and Kegan Paul, 1981), ch. X, esp. section 7, for similar conclusions on the strong programme conception of science and, especially, causality.

[42] Indeed, despite their regular recourse to and dependence on skeptical arguments, the sociologists in question hardly count as skeptics either. They are not skeptics inasmuch as the skeptical lessons about reasoning are quite general, applying to efforts to justify all purported first principles, including imputations of causal connections. The SSK ignores the reach of skepticism altogether.

II. The Myths of Sociological Method; or Two Dogmas of SSK[43]

Sociologists favoring both symmetry and causality might claim that, epistemology notwithstanding, the strength of the strong programme (and its variants) resides in the actual explanations produced. The 'relativist' label is just besides the point. This would, at least, focus discussion on the merits of actual explanations. Of course, to judge explanations by their merits here would require some explication of the notion of causality. Otherwise, there is no way to determine whether or not the sociologists achieve explanatory success more often than the friends of reason explanations are alleged to achieve theirs.

A minimal and charitable reading of the notion of causality to which the SSK analyses advert would be just Humean constant conjunction.[44] But this minimal reading does not help save their account from a fatal difficulty. For Hume was, quite famously, as skeptical about beliefs in causal connection as he was regarding logical proof. Indeed, the cognitive status of belief not only about particular causal connections, but also about causality, are as suspect as those beliefs imagined to be made obvious or certain by virtue of reason or experience. All such accounts turn out to be circular; they appeal to the regularity of experience to justify beliefs in regularity.[45] As a result, causal explanations do not provide a way past skeptical challenges to rationally grounding beliefs.

Yet, within the framework of SSK, causal explanation is, without argument, presumed to have some *better* epistemological standing than the account of explanation by appeal to rationality that it is to replace. But that assumption is false.[46] It is false, moreover, not only as a general epistemological principle, but also as a practical matter. For to imagine that causal modelling, even when sophisticated mathematical techniques are used, somehow sidesteps the problems of when to infer causal connections is to be mistaken about the sort of difficulties that beset attempts to wring causal inferences from even the most sophisticated statistical work.[47]

Bloor does not help the sociological case here in a recent effort to respond to challenges to establish that causal accounts are somehow better certified than the

[43]This section expands upon and revises some material from my 'What Does the Sociology of Scientific Knowledge Explain? or, When Epistemological Chickens Come Home to Roost,' *History of the Human Sciences* 7 (1994), 95–108.

[44]Giere, *op. cit.*, note 18, p. 109, notes, like Hollis, the Humean propensities of the sociologists. While I discuss this more below, one more irony in this debate is the extent to which writers in the SSK tradition, imagining themselves to represent the 'overcoming' of positivism, in actuality attempt to redo strict empiricism in sociological dress.

[45]Oddly, Barnes and Bloor use Humean critiques to justify preferring causal explanation over ones in terms of justification by reasons, without appearing to recognize that the same problems reappear for their own favored strategy. Barnes and Bloor, *op. cit.*, note 6, pp. 40–47.

[46]In ch. 8 of my *Meaning and Method in the Social Sciences*, *op. cit.*, note 38, I also critique the fast and easy way that SSK has with its account of causality, but from a different perspective than that urged here.

[47]These issues are well canvassed in Stephen Turner (ed.), *Causality in Crisis* (Proceedings of Conference on 'Causality in Crisis', Notre Dame, Indiana, 15–17 October 1993), forthcoming. See, in particular, the papers by Stephen Turner and Daniel Freedman.

accounts given in terms of reasons that he finds so unsatisfactory.[48] Bloor insists, on the one hand, that the social component is always present but that, on the other hand, it may or may not be causally significant.

> But doesn't the strong programme say that knowledge is purely social. Isn't that what the epithet 'strong' means? No. The strong programme says that the social component is always present and always constitutive of knowledge. It does not say that it is the *only* component, or that it is the component that must necessarily be located as the trigger of any and every change; it can be a background condition. Apparent exceptions to covariance and causality may be merely the result of the operation of other natural causes apart from social ones.[49]

So, social factors determine what people believe except when they do not. While this principle is impossible to refute, it is not very helpful either.

What one wants to know is how to identify the relevant engines of belief change. For this, one needs laws (or close analogs) specifying what to expect under certain conditions. To this demand, Bloor responds as follows. 'What does this say about the search for "laws" in the sociology of knowledge? It means that any such laws will exist, not on the surface of phenomena, but interwoven into a complex reality. In this respect they will be no different from the laws of physics'.[50] Well, of course, there is at least one difference. We seem to have some laws in physics. We have none in the sociology of knowledge.[51] Note here the point emphasized at the end of the first section, namely that however radical the SSK critique of the self descriptions of scientists may be, and despite the recurrent criticisms of what they perceive as inappropriate philosophical valorization of the natural sciences, what Bloor's remarks reveal is that he retains the conventional understanding with respect to key concepts such as those of law or of explanation. In this important respect, either the sociological challenge to philosophy of science is much less sweeping than might at first appear to be the case, or they have not absorbed the methodological lessons of their own studies.

Let me hasten to emphasize here that this is not to suggest that causal explanations of behavior are therefore necessarily *worse* off than the reason explanations they are intended to replace. The problem is that such explanations appear to have no better epistemological or practical warrant (or prospect for such being obtained) than those that the sociologists find defective.

But if the claims to provide an explanatory strategy that slips the problems of their rivals cannot be substantiated, what then to make of claims, within SSK, to provide

[48]See the 'Afterword' to David Bloor, *Knowledge and Social Imagery*, 2nd edn (Chicago, IL: University of Chicago Press, 1991), pp. 163–185.

[49]*Ibid.*, p. 166.

[50]*Ibid.*, p. 167.

[51]Rather oddly, Bloor offers, as a counter example to this well-known philosophical scoff, the following: 'All concept application is contestable and negotiable, and all accepted applications have the character of social institutions' (*ibid.*, p. 167). The problem here, apart from quibbling over what counts as a scientific law, is that this does not appear to go anywhere to providing the causal explanations, underwritten by laws, which are called for by the strong programme.

scientific explanations?[52] On this issue, clear divisions have emerged in SSK between those who take their claim to be non-relativist scientists most seriously and those (such as Steve Woolgar or Bruno Latour) who take their role as skeptics most seriously.[53] Given the intellectual fault lines just scouted, the split occurs just where one would expect it to, namely over the claim of SSK accounts to provide, if not causes, then, at least, some explanation satisfying scientific standards. Further, and again as prefigured in the debate as sketched so far, views on agency are also in dispute.[54] The present debate positions Woolgar, Latour and Callon as the heirs to Hume's skeptical and reflexivist musing on the limits of justifying accounts of human doings.[55] This skepticism and reflexivism has elicited strong anti-skeptical, anti-relativist responses from others in the field.

The claimed anti-relativist status of sociological explanations is not tied exclusively to the standard provided by a demand for causality. For example, an attitude of social realism, Harry Collins and Steve Yearley assert, can serve that purpose.[56] Social realism permits the sociologist, in that mood, to provide close descriptions of the world in which the scientist operates. 'Detailed description dissolves epistemological mystery and wonder'.[57] The method of close description, under the assumption of social realism, yields the signature SSK conclusion. 'The methodological prescription that emerges ... is that explanations should be developed within the assumption that the real world does not affect what the scientist believes about it'.[58] Descriptions done in the SSK mode are imagined to license their so-called relativism, namely that 'all cultural enterprises ... [have] roughly the same

[52]Discussions of relativism has too often sidetracked debate away from the issue of whether the explanations offered by the numerous SSK studies deliver the particular type of explanation promised. As Bloor insists, sociologists offer explanations 'in the same causal idiom as those of any other scientist. Their concern will be to locate the regularities and general principles or processes which appear to be at work within the field of their data. The aim will be to build theories to explain these regularities.' Bloor, *op. cit.*, note 48, p. 5. The aim, in short, is to be scientists.

[53]This dispute has smoldered for a number of years, flaring up now and again in journal articles and conferences. A recent, sustained flare-up can be found in various essays in Andrew Pickering (ed.), *Science as Practice and Culture* (Chicago, IL: University of Chicago Press, 1992).

[54]See, in particular, the essays by Woolgar, Latour and Callon, and Collins and Yearley in Pickering, *op. cit.*, note 53.

[55]Consider, in this regard, the following remark: '... the only observables are the traces left by objects, arguments, skills, and tokens circulating through the collective. We never see either social relations or things. We may only document the circulation of network-tracing tokens, statements, and skills. This is so important that one of us made it the first principle of science studies [reference to Latour]. Although we have not yet fully articulated this argument, it is the basis of our empirical methods.' M. Callon and B. Latour, 'Don't Throw the Baby out with the Bath School!: A Reply to Collins and Yearley', in Pickering, *op. cit.*, note 41, p. 351. The 'more-empirical-than-thou' tone to much of the controversy here is philosophically fascinating. It suggests a strangely unreflexive (extremely odd, in fact, given that one of the authors here is Latour) invoking of some form of the theory/observation distinction. In this respect, Steve Woolgar takes a much more philosophically/skeptically consistent line in his, 'Some Remarks about Positionism: A Reply to Collins and Yearley', in Pickering, *op. cit.*, note 53, pp. 327–342.

[56]Harry Collins and Steve Yearley, 'Epistemological Chicken', in Pickering, *op. cit.*, note 53, p. 309.

[57]*Ibid.*

[58]Harry Collins and Steve Yearley, 'Journey into Space', in Pickering, *op. cit.*, note 53, p. 372.

epistemological warrant'.[59] The legitimacy of their descriptions is taken to underwrite, then, both their particular explanations and their general epistemological evaluation of the natural sciences.

In order to see why this is just dogmatic scientism[60] dressed up in skeptical clothing, consider the parallel between reflexive critique here and the role of such critique in the overthrow of positivism. The methodological linchpin of positivism was the verifiability criterion of meaning. This held that for any sentence to be cognitively significant/meaningful, it must either be so by virtue of its logical form (either analytic or self-contradictory) or, if contingent, have empirically specifiable truth conditions. However, when the question was raised whether this principle itself is believed true either by virtue of its logical form or because of evidence, no good answer could be given.[61] The positivists did not want to contend that they had simply stipulated a definition of meaningfulness; that would have taken any critical edge off of their critiques of other views on meaning. Yet they could not maintain that the account was an empirical hypothesis, because the criterion delimited the realm of empirically meaningful sentences; hence, any 'test' would be question-begging.

The reflexive equivalent in the context of Collins and Yearley, in turn, would demand a naturalistic justification of claims made by them for close description. But then a paradox analogous to the one that plagued the verifiability criterion arises for sociologists committed to social realism. On the one hand, the method of close description *per se* does not require excluding the sort of descriptions—by Woolgar, Latour, and Callon—that Collins *et al.* find offensive. On the other hand, social realism is not an empirical hypothesis sustained by sociological practice using the method of close description, since such descriptions can be used to precisely the opposite effect. In short, the Collins and Yearley position is neither true by definition nor a hypothesis that can be sustained without begging the question of which accounts 'make sense'.

Faced with a demand to substantiate the legitimacy of their approach, Collins and Yearley turn from bold challengers of scientific orthodoxy into curmudgeonly reactionaries, much in the manner of positivists who could neither justify the verifiability principle nor bring themselves to give it up. In particular, they can neither legitimate the elevated status given descriptions in the mode of social realism nor give them up. For example, the criticism of Woolgar and other reflexivists turns out to be *not* that they fail to provide close description, but that they provide the 'wrong kind'

[59] *Ibid.*, p. 384.

[60] By 'scientism' I mean the view that all and only the methods of natural science are appropriate to the study of all subject matters. What is scientistic, in this respect, about the position Collins and Yearley defend is the assumption that only the 'method of science'—however they would spell that out—is appropriate to the study of human social behavior.

[61] For a brief consideration of the problems here, see the 'Introduction', in A. J. Ayer, *Language, Truth and Logic*, 2nd edn (New York: Dover Publications, 1952). This edition was originally published in 1946.

of close description, namely descriptions that make problematic the sort of claims to objectivity of the accounts Collins and Yearley favor.[62]

But the problem goes deeper than a stubborn preference for one mode of description over another. For avoided entirely is the crucial question of what makes one description, from any perspective, either worthy of attention in its own right or preferable to a competitor in the same style. It just will not do to insist, however repeatedly, that the favored descriptions 'work'.[63] The issue is *not* how SSK can make scientists 'look'; the challenge concerns what makes these descriptions (however 'close') worth taking seriously. Otherwise, there is no reason whatsoever to credit the rhetoric that now, thanks to SSK, we have learned that all disciplines have the same epistemological warrant etc.[64]

Having, like their positivist forebears, glimpsed the fact that their method of explanation has no immediate or automatic standing given their own principles, Collins and Yearley dig in their heels and declare their faith.

> We have our methods; they include participation in forms of life. To deny this on grounds of unobservability would be to resurrect the scientific sociology of the early sixties.[65]

'We have our methods' indeed![66] 'Participation in forms of life' is not a method, but a claim. Questions of how to substantiate this sort of claim have never been resolved. The suggestion that one empirically unjustified account of scientific belief

[62]Collins and Yearley, *op. cit.*, note 53, pp. 308–309.

[63]*Ibid.*, p. 309.

[64]There is a further deep puzzle here. Collins and Yearley speak of three methods for making progress with regard to our relation to 'machines and other artifacts'. These are the methods of modeling, natural science, and that of the SSK. 'What sociology of scientific knowledge provides is a third method, no longer subservient to accounts of the work of the scientists and technologists and the stories of philosophers but rooted in special understanding of social life' (*ibid.*, p. 321). What is puzzling is that this method is explicitly contrasted with 'the false ally of the counterfactual method' (*ibid.*). Yet causal explanations are taken to support counterfactuals—if being exposed to a virus caused Jones' disease, then, presumably, if not exposed, Jones would not have had the disease. More generally, in non-laboratory sciences, explanations that could not support counterfactuals would not usually be thought to be providing causal explanations at all. If, as Collins and Yearley complain, in 'their emphasis on form, the reflexivity and actor-network theory approaches both exclude explanation', (*ibid.*, p. 323) it is difficult to see where Collins and Yearley imagine their account provides for it.

[65]Collins and Yearley, *op. cit.*, note 58, p. 381.

[66]These comments also exhibits the favorite fallacious argument form of the SSK—*argumentum ad hominem*. No one should be cowed by this SSK version of guilt by association. Find an aspect of an opponent's position that is also some view that any positivist ever held and, *voilà*, the argument is refuted. In this case, the fact that positivists made light of claims to understanding does not show, now that we are all properly post-positivist, that suddenly all is well with the so-called method of empathetic identification. Collins and Yearley are correct to maintain that there is nothing particularly methodologically *outré* about an appeal to unobservables. However, when the appeal is for the purpose of justifying one's entire way of proceeding, without additional empirical check, then some more compelling reasons for accepting the posit is needed. Use of this argument form is also rampant in Pickering's work. See, for example, Andrew Pickering, 'Philosophy Naturalized a Bit', *Social Studies of Science* **21** (1991), 575–585. Setting up straw man positivists and knocking them down occupies most of Pickering's 'Knowledge, Practice and Mere Construction', *Social Studies of Science* **20** (1990), 682–729.

formation—the positivists'—be replaced by an empirically unjustifiable one is, for all its delightful irony, still quite unsatisfactory.[67]

Consistent with the thought that their favorite close descriptions are somehow self-substantiating, one also finds appeals to historicism as the method of choice for underpinning the explanations of science which the SSK prefer. By 'historicism' is meant 'the programme dedicated to analysing historical action in historical actors' terms'.[68] Shapin, whose characterization this is, acknowledges that 'historicism is accompanied by its own methodological baggage', most infamously 'atomizing particularism'.[69] Traditionally, of course, this generates charges that historicism is *anti-scientific*, inasmuch as particularism precludes any generalizations, and is unverifiable, insofar as there is no good test of whether or not one has reconstructed the categories particular to participants correctly. As with Collins, Shapin appeals to the most localist of all localist methods in order to sustain the general sociological thesis that certain factors—the ones sociologists favor—are appropriate for explanation to the exclusion of rivals.[70]

[67]Of course, there is a method textbooks call 'participant observation', and a vast literature on qualitative research generally. The sense in which invoking 'participation in forms of life' represents a *claim* and *not* a *method* is the one most directly relevant to the debate, i.e. the question of the purported objectivity/reliability/validity of these forms of study. There is no need to rehearse the *Methodenstreit* to make the point that there exists no consensus on the validity of such methods. The crisis in anthropology induced by post-modern critiques of ethnographies concerns precisely this point, i.e. the 'scientific standing' of ethnographic methods such as participant observation. Nice historical work on the development of this style of research method in British and American anthropology and sociology can be found in the writings of Jennifer Platt, e.g. 'The Development of the "Participant Observation" Method in Sociology', *Journal of the History of the Behavioural Sciences* **19** (1983), 379–393, and her 'The Chicago School and Firsthand Data', *History of the Human Sciences* **7** (1994), 57–80. The claim that appeal to these methods is the basis of a crisis in social science, not its resolution, is generally accepted. See, e.g. the section on the 'Crisis of Representation' in the 'Introduction', *Handbook of Qualitative Research*, N. K. Denzin and Y. S. Lincoln (eds) (London: Sage, 1994), pp. 9–11. This essay also contains an extensive bibliography to the relevant literature. See also, 'Qualitative Methods: Their History in Sociology and Anthropology', A. J. Vidich and S. M. Lyman, *ibid.*, pp. 23–49, but especially p. 41 ff. Relevant also are sources cited in note 70 below.

For balanced general historical overviews of this particular controversy, insofar as it concerns the status and the nature of the distinction between the *Geisteswissenschaften* and the *Naturwissenschaften*, see J. Habermas, *On the Logic of the Social Sciences* (Cambridge, MA: MIT Press, 1988), or K.-O. Apel, *Understanding and Explanation* (Cambridge, MA: MIT Press, 1984).

[68]Shapin, 'Discipline and Bounding,' *op. cit.*, note 3, p. 354.

[69]*Ibid.*

[70]*Ibid.*, p. 353. This despite Shapin's characteristically candid acknowledgement of the shortcomings of his preferred approach on the critical issue of causality: see especially pp. 345–351). While Shapin cites as exemplary here S. Schaffer and S. Shapin's *Leviathan and the Air Pump* (Princeton, NJ: Princeton University Press, 1985), it is instructive to note how their account can be challenged by another self-professed naturalist and read to the advantage of a more philosophical account, i.e. one that emphasizes just the factors as determinative that Shapin would discount. See the discussion in P. Kitcher's *The Advancement of Science*, *op. cit.*, note 19, pp. 294–302. The point here is that the historical data too are underdetermined and so can be read to many different effects. If the issue is who has the best explanation of the shape which scientific practice takes, appeals to historicist matters hardly strengthens the SSK position.

The problems with a historicist methodology are well canvassed by historians. See, for example, the magisterial work by G. Iggers, *The German Conception of History* (Middletown, CT.: Wesleyan University Press, 1968). One might say that the time has come for the SSK to stop worrying about Karl Popper

Further muddying the explanatory waters is a recent effort by Pickering to combine historicism and an account of science that brings the world back in as an explicit player in the sociological story, what Pickering calls the 'mangle of practice'.[71] For Pickering, being a historicist means eschewing appeal (for purposes of explanation) to a 'realm of regulatory or guiding principles—standards or interests—that endure through particular acts of knowledge production and evaluation....Traditional thought in philosophy and the social sciences on human action and cognition in general begins from just this assumption. Nevertheless, the historicist view that I advocate ... suggests that science can, and indeed does, function in the absence of these....'[72] Rather, he sees inquiry as a matter of *resistance*—'emergence of obstacles on the path to some goal'—and *accommodation*—'the revision of open-ended modeling sequences'.[73] The interplay of resistance and accommodation comprise what he terms 'the mangle of practice'.[74]

The puzzle here is how this account of the mangle extends, or rather fails to extend, to those accounts on which Pickering does not smile. For while, on Pickering's account, resistances can lead the scientists he discusses to modify their practice, Pickering vigorously chastises philosopher of science Ron Giere for suggesting that experimental resistance can make one theory appear strongly preferable to another.

The problem, I suggest, lies not in any deep methodological divide between Pickering and Giere but consists, rather, in the sort of description of the case that Giere favors. For when Giere describes matters in a way that suggests that experiments provide evidence for rationally preferring one view to another, Pickering argues *ad hominem* by attempting to tar Giere's position with some tinge of positivism: 'here we are, back in a good old-fashioned context of justification in which the data tell scientists what to believe'.[75] The reason that Giere, but not Pickering, commits the

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[70] *continued*
et al. and start thinking seriously about their relation to Hayden White. Although not directly tied to the issues of this paper, I remain fascinated how the two most influential schools pursuing social constructivism—those who, like the SSK, claim to follow Kuhn and those who take their lead, rather, from Hayden White and various schools of semiotics (see, e.g. work by James Clifford in anthropology)—ply their trades in apparent ignorance of one another. Moreover, the types of relativism to which each school subscribes show important differences as well. For an account of how the notion of historical objectivity has played out on the American scene, told in a way in which the SSK ought to be sympathetic, see P. Novick, *That Noble Dream: The 'Objectivity Question' and the American Historical Profession* (Cambridge: Cambridge University Press, 1988). Novick's book should convince them that attempts to found their account of scientific objectivity on historicist notions is building on quicksand.

[71] Andrew Pickering, 'Objectivity and the Mangle of Practice', *Annals of Scholarship* **8** (1991), 409–425.

[72] *Ibid.*, p. 411.

[73] *Ibid.*, p. 412.

[74] The notion of mangle is to emphasize the contingent nature of scientific productions, how they are jury-rigged rather than constructed by some cook-book of scientific method. 'The particular resistances and accommodations that give content to *this* new instrument, fact or theory, arise unpredictably in the real-time of scientific practice and cannot be explained by reference to any catalog of enduring regulatory principles. What emerges from the mangle has therefore a truly historical character, and this is what I mean by describing the appreciation of knowledge outlined here as a historicist one' (*ibid.*). 'Historicist', for Pickering, connects then to the gloss by Shapin cited above; the point is to emphasize the localism and particularity of the object or theory under discussion.

[75] Pickering, 'Philosophy Naturalized a Bit', *op. cit.*, note 66, p. 578.

sin of letting nature guide the scientist is that, on Pickering's view, Giere does not see the process as sufficiently open-ended. Nature can guide a scientist's hand, but not when described, Pickering believes, in Giere's way.

To this, Giere could and does reply that it is strange indeed for a sociologist to insist that all logical possibilities must count, since, in actual situations, scientists will have before them a rather limited repertoire of theories by which to accommodate various resistances.[76] Furthermore, Pickering, in his own work, employs a distinction between 'free' and 'forced' moves that seems not at all unlike the kind of description he finds so offensive in Giere.[77] Indeed, by emphasizing the historically situated options confronting the scientists he discusses, Giere appears the better historicist and naturalist than does Pickering.

What divides naturalizing sociologists from naturalizing philosophers, once the red-herring charge of positivism is seen for what it is? The issue, it now appears, turns *not* on competing methodologies, but on *competing descriptions* of ethnographic or historical material. As one would expect from the skeptical arguments underlying so much of the debate both in and between the sociology and philosophy of science, attending to the details of a case does not preclude the formulation of incompatible accounts of the data based on different descriptions of what investigators observe.

One of the morals to be drawn here, I suggest, is the rock upon which SSK is founded, namely the symmetry principle, can only bear the weight of the program if the causality principle—or some other objectivist basis for explanation—can. For, as argued above, the motivation for treating beliefs symmetrically comes from the putative inadequacy of accounts of rationality for deciding, in historical cases, why people chose as they did. But since it turns out that the notions of causality, causal explanation, and close description suffer from infirmities quite equal and parallel to the difficulties of those explanatory strategies that the SSK disdains, the promised explanatory gain disappears. With it goes any reason to assume that sociological/causal explanations of belief provide any antidote to skeptical worries used, in the first place, to motivate the sociological turn in science studies.

My second moral, and the one of greater moment, is that beliefs in the relatively unproblematic nature of causal explanation and their own status as scientists are two dogmas of the 'new' sociology of knowledge. Indeed, to follow the Quinean analogy through, the dogma of the superiority of causal explanations of beliefs and the dogma of the scientific status of SSK are basically one in the same. For an abiding faith in scientism, in the ability of some one method to deliver the truth about reality, connects Bloor, Collins, and others, whatever their other differences. All subscribe to the presumption that events stand connected in a particular way, and that certain

[76]R. Giere, 'The Cognitive Construction of Scientific Knowledge: Response to Pickering', *Social Studies of Science* **22** (1992), 100–101.

[77]See A. Pickering and A. Stephanides, 'Constructing Quaternions: On the Analysis of Conceptual Practice', in Pickering, *op. cit.*, note 53, pp. 139–167; see particularly p. 163, fns. 23 and 24.

methodologically informed individuals can ascertain what this connection really is. Yet these were just the assumptions made by the philosophies of science that the SSK rejects. The arguments swirling about over which sort of factors receive explanatory pride of place is one, as Hollis partially appreciated, between two sets of dogmatists. What separates these schools is the faith one puts in discovering good reasons for scientific beliefs and the opposed faith the others place in finding causal explanations. Neither has justified any claim to being the *sole* authority for the explanation of human behavior.[78]

III. On (Finally) Taking the Disunity of Science Seriously

My argument so far has been that efforts to promote one style of explanation of scientific knowledge and exclude others invariably engenders dialectical dead ends. In the debate canvassed in Section I—reasons versus causes—an impasse is reached because the competing approaches turn out to be afflicted with parallel and offsetting infirmities. The debates within SSK chronicled in Section II simply recapitulate nineteenth-century arguments that aimed to establish the ability of the *Geisteswissenschaften* to recount an event *wie es eigentlich gewesen*. The persistence of these disputes within SSK only testifies to the truth of the adage that those ignorant of history are condemned to repeat it.

These various impasses result, I suggest, because the disputants cling to a common but erroneous assumption. Overcoming the present sorry state of debate depends, or so I argue, on rejecting this shared false premise. This is the belief that there is a scientific method—some set of methods and norms that can be used to demarcate legitimate practices from the rest. For without some such assumption regarding some way of defining what scientific method is, why would anyone indulge in disputes about whether or not a particular mode of investigation is scientific? The argument, by and large, is *not* over whether there are explanations of the beliefs scientists hold which accord with some notion or other of scientific method, but with regard to whose explanations in fact instantiate the proper form of scientific reasoning. Yet the lesson repeatedly taught by case studies and philosophical inquiry—that there are no general, perduring norms that define what it is to be a *science*—is precisely the moral invariably ignored in the disputes noted so far. In what follows, I examine how this shared presumption regarding the existence of a scientific method continues to work into debates regarding the explanation of scientific knowledge. I conclude by

[78]Alan Nelson suggests a similar conclusion, although his argument for it travels a path very different from the one sketched here. Nelson notes, regarding the arguments for explanatory priority waged between social constructivists and scientific rationalists, that the dispute may well be rationally irresolvable. 'Perhaps it would be better to say that there should not really be a dispute. Rationalists are determined to interpret the history of science such that the really rational choices always prevail in the long run, and since some of them are ingenious, they will succeed by their own lights. Constructivists are just as determined and ingenious, and will enjoy equal success.' Alan Nelson, 'How *Could* Scientific Facts be Socially Constructed?', *Studies in History and Philosophy of Science* **25** (1994), 537–547, p. 546.

sketching how inquiry ought to proceed if one takes seriously the disunity of science.

The use of the term 'science' and cognates such as 'scientist' in the sense tied to an epistemologically significant notion of scientific method is, surely, the usage with which all of us are quite comfortable and familiar. The intimate link presumed between 'science' and the natural sciences, and all the related connotations of experimental method, theoretical entities, and reliance on observation dates, roughly, from the nineteenth century and thinkers such as Whewell.[79] This narrow construal of 'science', tying it just to the natural sciences, is, moreover, primarily an Anglo-American preference.

The philosophy of science, at least or especially in the guise that those in SSK claim to oppose, looked, through the first several decades of this century, to distill those principles taken to inhere in the practices of the natural sciences—the secret of their success. What it was to be scientific was to follow a certain general method—the scientific method.[80] The consistent and persistent failure to uncover what this method happens to be has led some, such as Rorty, to urge abandoning the search as a lost cause, a Lakatosian degenerating research program. It has led others, most notably many practitioners of SSK themselves, to conclude *not* that the search for a method was in vain, but that philosophers of science were looking for the wrong sort of explanations.[81] Yet their confidence that causal explanations of scientific beliefs are there to be had has yet to be justified by their practice.

But if, following Rorty, one holds that there is no secret to the success of science—no method scientists have down pat and the rest should hope to emulate, what then should one conclude? My suggestion is that interesting and important consequences flow from dropping the conceit, indulged up until now, of granting all the disputants their use of the term 'science'. Arguments over the scientific status, or lack thereof, of differing explanations leads only to dialectical dead ends, and should be avoided. The live issue, I contend, is how to analyze disciplinary practices

[79]See the etymological reflections in Sydney Ross, '*Scientist:* The Story of a Word', *Annals of Science* **18** (1962), 65–85, esp. p. 72. Ross's comments in the last three pages of his article make plain how the notion of scientist is parasitic on the belief in a special method owing to the practices of the natural sciences and generalizable to other disciplines as well.

[80]See, for example, Andrew Cunningham and Perry Williams, 'De-centering the "big picture"', *British Journal for the History of Science* **26** (1993), pp. 407–432, esp. p. 411. Philip Kitcher offers a nice characterization of this view in the first chapter, 'Legend's Legacy', of his *The Advancement of Science, op. cit.*, note 19. Kitcher appreciates the extent to which the SSK arguments are skeptical ones in the service of rejecting rationality-based explanations in particular, and progressive evaluations of the scientific enterprise in general.

[81]In this regard, Barnes, *op. cit.*, note 7, correctly links SSK with the naturalizing push in philosophy, especially as found in the writings of Quine. For Quine too endorses the Humean project of explaining what people believe, including all of natural science, by using science to provide causal accounts of these beliefs. Quine, of course, claims that the apparent circularity of this project is benign once one surrenders the hope of doing 'first philosophy', i.e. justifying the practice of science from some standpoint independent of and firmer than our scientific practices themselves. The *locus classicus* here is Quine's essay, 'Epistemology Naturalized', in *Ontological Relativity* (New York: Columbia University Press, 1969), pp. 69–90.

once one acknowledges that we possess no notion of what makes a particular practice a science, in some philosophically interesting sense of 'science'. One should say, not 'So much the worse for science', but 'So much the worse for "science"'.

Properly interpreted, I maintain, 'so much the worse for "science"' importantly differs from slogans such as 'anything goes'. For my suggested moral urges only the enduring skeptical caution against fooling ourselves into thinking that we know more than we in fact do. Slogans such as 'anything goes', however, deign to offer advice, and not very helpful advice at that.

It is easy to establish that the skeptical caution is commonly ignored even by researchers who ought to be alert to it. For example, writers in SSK continue to employ the notion of science, even after providing case studies meant to disabuse us of the efficacy of appeals to a 'scientific method'. Here is a typical instance, from a recent work by Harry Collins and Trevor Pinch, *The Golem*.[82] Collins and Pinch write, '*The Golem* presents a view of science as fallible and untidy, a matter of craft rather than logic. To do this it examines a series of experiments.... In each case it shows that scientific certainties do not come from experimental method, but from the way ambiguous results were interpreted. To explain science the authors display science'.[83] Yet, their display of science is for the purpose of revealing that the notion of a 'scientific method' is a 'misplaced ideal'.[84] incapable of settling 'disagreements through better experimentation, more knowledge, more advanced theories, or clearer thinking'.[85] But why continue to insist, then, that there is something to be called 'science'? The thread that bound together the activities examined by Collins and Pinch was supposedly composed of unifying methodological strands. Having claimed to show that the alleged unity is a chimera, the obvious but undrawn conclusion is that what remains are just activities with no essential tie to one another.

A similar teetering at the brink of recognizing that the term 'science' has outlived the uses for which it was mainly introduced are found in writers of philosophical persuasion as well. For example, Kuhn echoes what is surely now a widely held view that a 'reconceptualization' of science is needed, in particular, one that 'depends on abandoning the view of science as a single monolithic enterprise, bound by a unique method'.[86] Having made this observation, Kuhn then adds that 'it should be seen as a complex but unsystematic structure of distinct specialities or species, each responsible for a different domain of phenomena'.[87] But what, now, answers to the referent of 'it' here? For has not Kuhn just asserted that there is no 'it'?

Likewise, after surveying the state of actual practices in the sciences, both John Dupré and Zuzana Parusnikova urge a conclusion similar to that of Kuhn's. Dupré,

[82](Cambridge: Cambridge University Press, 1993).
[84]From the frontispiece.
[84]*Ibid.*, p. 143.
[85]*Ibid.*, p. 144.
[86]Kuhn, *op. cit.*, note 20, p. 18.
[87]Kuhn, *op. cit.*, note 20, p. 18.

for instance, maintains that '[s]cience, construed simply as the set of knowledge-claiming practices that are accorded that title, is a mixed bag. The role of theory, evidence, and institutional norms will vary greatly from one area of science to the next'.[88] Surveying the sessions at any meeting of the Philosophy of Science Association provides further validation of this characterization. Overwhelmingly, one finds thriving subdisciplines such as philosophy of biology, philosophy of psychology, and philosophy of physics. But precious little answers to some general assessment of issues in philosophy of science. In its own quiet way, the philosophy of science has gone postmodern.

Parusnikova echoes this observation regarding the *de facto* move of science into postmodernism, taking as her focus science writing. Examining book catalogs rather than symposia titles, she finds 'a rather heterogeneous mixture of extremely partial isolated issues'.[89] Yet, like Dupré, who wishes to hold onto a 'family resemblance' account of science and some form of demarcation criterion,[90] Parusnikova cannot take the final step of counseling that the term be dropped. Instead, she is willing only to consider the suggestions either that postmodernism has nothing directly to offer the philosophy of science, or that postmodernism suggests a strategy for examining scientific texts.[91] Yet neither thinker can identify what work the term currently does for us.[92]

No one can provide substantiation for the desired sense to the term 'science', but neither, it seems, can anyone give it up. Reluctance to surrender the term stems from hope that one can still use it to mark out something that is special and right about certain approaches to inquiry. But surely, if the past two or three decades of sociological investigation and philosophical reflection on laboratory life suggest any conclusion, it is that continuing to invoke the term 'science' for purposes of explaining what some do right is to commit a category mistake.

Consider how the disputes reviewed so far neatly illustrate the misstep which Ryle cautioned us against.[93] One of Ryle's examples of this mistake concerns the tourist who, after visiting Christ Church, the Bodleian Library, etc., still wants to see where Oxford University is. Another imagines a case where, after seeing the various brigades march by, someone insists on waiting around until the regiment marches past as well. But, of course, there is no Oxford University to view in the tourist's sense; there is

[88]John Dupré, *The Disorder of Things* (Cambridge, MA: Harvard University Press, 1993), p. 242.

[89]Zuzana Parusnikova, 'Is a Postmodern Philosophy of Science Possible?', *Studies in History and Philosophy of Science* **23** (1992), 21–37, p. 30.

[90]John Dupré, *op. cit.*, note 88, ch. 10.

[91]Parusnikova, *op. cit.*, note 89, pp. 36–37.

[92]Cunningham and Williams, *op. cit.*, note 80, esp. pp. 415–417, and footnotes therein, also comment on how little even the proponents of a unity thesis are able to offer. Somewhat surprisingly, their suggestion to replace a timeless notion of science by a rather thoroughly historicized one (*ibid.*, p. 418) leads them to propose emphasizing questions about explaining the objectivity of science (*ibid.*, p. 419) which would, of course, put them back in the reason vs causes dialectic now fruitlessly occupying the energies of sociologists and philosophers.

[93]Gilbert Ryle, *The Concept of Mind* (New York: Barnes & Noble, 1971), esp. pp. 16–18.

no regiment to witness after the other units have paraded by. Likewise, there are various disciplines that, through the wisdom of deans or assorted other administrators, are classified as sciences. One can, if one is so inclined, list procedures employed in each of them. But to imagine that one ought to keep a weather eye out, after having cataloged the various and disparate practices of the biologists, the chemists, the physicists, etc. until one sees the 'scientific practices' as well, is to just fall prey to a category mistake. If nothing on the original list identifies these areas as linked in a common endeavor by virtue of employing common methods and norms, then there is no explanatory gain to be realized from insisting that the viewing remains incomplete until one observes, as well, the scientific method. What research reveals is that nothing falls under that name which is common to and definitive of whatever else we now call a science. Like the ghost in the machine, appeals to 'scientific method' turn out to be a pseudo-explanatory prop, not an aid to clarifying why things go right when they do.

The conclusion of the dialectic between sociologists and philosophers over who has got the account of the practice of science right is that neither do. 'Science' ain't there to have. Moreover, if the philosophical search for a method is in vain, then neither can sociologists possess a privileged way of discussing what goes on in laboratory life.

Rather than continuing to indulge in either philosophical or sociological versions of familiar dogmas, let us ask, rather, how the terms of this debate regarding methods and norms might be recast. Where does debate and inquiry go if one gives up the notion of science, at least insofar as it is used to suggest something over and above the particular activities in the disciplines that happen to be lumped together at this historical moment?

Having begun by invoking the spirit of one of philosophy's best known advocates of postmodern sensibility, I shall close by recalling the shade of one whom most postmodernists have striven to forget, namely, Rudolf Carnap. Yet, it was Carnap who introduced[94] the distinction between what he termed 'internal' and 'external' questions. Beginning his philosophical career with a belief that scientific method could be given evidentiary and methodological foundations, he later reluctantly came to the conclusion that all that one could do was to use various frameworks for purpose of analysis. There was no way of certifying the 'rational/scientific rightness' of one over others.

> If someone wishes to speak in his language about a new kind of entities, he has to introduce a system of new ways of speaking, subject to new rules; we shall call this procedure the construction of a linguistic *framework* .... And now we must distinguish two kinds of questions of existence; first, questions of the existence of certain entities of the new kind *within the framework*; we call them *internal questions*; and second, questions concerning

[94]'Empiricism, Semantics, and Ontology', in R. Carnap, *Meaning and Necessity*, 2nd edn (Chicago: University of Chicago Press, 1958), pp. 205–221.

> the existence or reality *of the system of entities as a whole*, called *external questions*. Internal questions and possible answers to them are formulated with the help of new forms of expressions....[95]

> From the internal questions we must clearly distinguish external questions, i.e. philosophical questions concerning the existence or reality of the total system of new entities....[W[e take the position that the introduction of the new ways of speaking does not need any theoretical justification because it does not imply any assertion of reality.... An alleged statement of the reality of the system of entities is a pseudo-statement without cognitive content. To be sure, we have to face at this point an important question; but it is a practical, not a theoretical question; it is the question of whether or not to accept the new linguistic form.... It can only be judged as being more or less expedient, fruitful, conducive to the aim for which the language is intended.[96]

In a postmodern light, one might well say that the normative questions that permit of answers will be internal to particular disciplines. There will be, as Carnap insisted, no answer to questions about which language of inquiry one ought to use, or which is really correct.

Asking about methods and norms, in the sense I have termed a category mistake, is a case of mistaking a question without a fixed answer for an internal question. That is, questions such as 'Is X really a science?' are pseudo-questions—ones which have no real answer—masquerading as internal questions. Such questions would be genuine questions if an answer could be read off the list of methods and norms constituting the practice in question. But absent a defining list for what it is to be a science, the best strategy is to reject the question as ill-formed.[97] Properly formed, one might ask after which set of practices, of those available, might prove most useful for doing what one wants done.

Steve Shapin urges a conclusion superficially like this, in conceding that the distinction between the 'social' and the 'external' is purely one made for purposes of conducting a study.[98] Yet Shapin continues to resist the more radical conclusion argued by Bruno Latour against recent trends in science studies that tend to reify distinctions made for analytic purposes. As Latour notes in commenting on the work by Shapin and Schaffer which examines the Boyle–Hobbes dispute as a case study

[95]*Ibid.*, p. 206.

[96]*Ibid.*, p. 214.

[97]The endorsement of Carnap here is not to contravene the sort of criticisms of Carnap's distinction entered by Quine in, e.g. 'Carnap and Logical Truth' and 'On Carnap's View on Ontology'. Quine's criticisms of the distinction between internal and external questions is that he thinks that these distinctions, as Carnap developed them, lead back to a distinction between analytic and synthetic statements and to objectionable ways of discriminating among things which are said to exist within a framework. Quine will have no truck with either consequence, and in this I follow Quine. However, using Carnap's distinction need not lead in this direction. Quine, in the last sentence of 'On Carnap's Views on Ontology,' puts the matter as follows. 'Carnap maintains that ontological questions, and likewise questions of logical or mathematical principle, are questions not of fact but of choosing a convenient conceptual scheme or framework for science: and with this I agree only if the same be conceded for every scientific hypothesis'. Put another way, my call is for explicitness in matters regarding one's analytic framework. None of the objectionable consequences follow from this. I thank James Maffie and Georg Vielmetter for pressing me to clarify this point.

[98]Shapin, 'Discipline and Bounding', *op. cit.*, note 3, p. 367, fn. 64.

in the SSK fashion,[99] '[t]hat there is no Nature "out there" to account for the success of Boyle's programme is obvious to them; but they seem to believe that there is a Society "out there" to account for the failure of Hobbes's programme'.[100] Even worse, as noted in Section II, Shapin pins his hopes of methodological justification on a form of historicism. But, as argued above, such hopes are empty.[101]

No one set of distinctions, Latour insists, has analytic priority. 'For an anthropologist of science, there is no more Force than Reason, no more Society than Nature'.[102] Latour chooses to dress up the view that we have a choice of vocabularies and analytic constructs to use in accounting for how things are in a rhetoric that suggests he moves beyond postmodern debates over one's faith in science. His goal is to be relentlessly reflexive and symmetrical, which is to say not to spare one's favorite science, or one's own practices, from scrutiny as just another imposed set of practices. There is no scientific us versus the benighted them; there are only boundaries drawn and redrawn. Yet Latour, in attempting to move beyond tiresome debates about method, unwittingly embraces the position of late Carnap. Latour's 'amodern' stance represents a Carnapian attitude towards external questions and echoes his sentiments that one's choice of framework is contingent upon its utility for the inquiry in which one is interested. *Plus ça change, plus c'est la même chose.*

As the existentialists insisted, if everything is permitted, nothing is. Responsibility can no longer be avoided. Far from licensing 'anything goes', dropping the term 'science' would demand, at long last, that we explicate and assess each putative explanatory practice in terms other than those drawn from some abstract characterization of what it is to be a science. Gesturing towards an as yet unexplicated but soon to arrive science will not do. Nor can one take refuge in the pretensions of historicism or participant observation to be sciences—objective modes of inquiry—which deliver the situationally specific essences of the groups or era studied. For ethnographic methods provide no better claims to validity or objectivity than the positivist principles they intend to replace. Without methodological self-consciousness, practitioners cannot say what goes, much less if anything does. Once we appreciate that no explanatory gain results by fighting over which disciplines or methods to label as 'science' or from treating the notion of science as explanatory, the more we may actually learn about how inquirers produce desired results.

[99]S. Shapin and S. Schaffer, *op. cit.*, note 70.

[100]Bruno Latour, 'Postmodern? No, Simply Amodern! Steps Towards an Anthropology of Science', *Studies in History and Philosophy of Science* **21** (1990), 145–171, p. 158.

[101]In *Meaning and Method in the Social Sciences*, *op. cit.*, note 38, I argue that there is no way to distinguish between claims that one has discovered what someone else actually meant and the charge that one is imposing a translation.

[102]Latour, *op. cit.*, note 100, p. 159.

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