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SOCIOLOGY OF SCIENCE, RULE FOLLOWING AND FORMS OF LIFE *

1. WITTGENSTEIN ON SCIENCE:
"THE POINT IS THAT '"OBEYING A RULE" IS A PRACTICE'"

Ludwig Wittgenstein was trained as a scientist and an engineer. He received a diploma in mechanical engineering from the Technische Hochschule in Charlottenburg, Berlin, in 1906, after which he did several years of research on aeronautics before turning to the full-time study of logic and philosophy. Hertz, Boltzmann, Mach, Weininger, and William James, all important influences on Wittgenstein, are authors whose work was both philosophical and scientific. The relationship between everyday life, science, and philosophy, is a central concern throughout the course of his writing. He regarded philosophy, properly conducted, as an autonomous activity, a matter of clarifying our understanding of language, or investigating grammar. Wittgenstein thought philosophy should state the obvious as a way of disabusing us of the desire to formulate philosophical theories of meaning, knowledge, language, or science, and was deeply opposed to the naturalist view that philosophy is a form of science. In his later work, Wittgenstein rejected systematic approaches to understanding language and knowledge. Wittgenstein's answer to the Socratic question about the nature of knowledge is that it has no nature, no essence, and so it is a mistake to think one can give a single systematic answer:

If I was asked what knowledge is, I would list items of knowledge and add "and such-like." There is no common element to be found in all of them, because there isn't one. (Wittgenstein, MS 302, "Diktat für Schlick" 1931-33.)

In the *Philosophical Investigations*, one of the principal reasons for Wittgenstein's opposition to systematic philosophical theorizing is that our use of language, our grasp of its meaning, depends on a background of common behaviour and shared practices – not on agreement in opinions but in "form of life" (Wittgenstein 1953, §242.)

Despite these far-reaching links between Wittgenstein's writings and the study of science, the relationship between the two is poorly understood. In part, this is because Wittgenstein wrote relatively little about science *per se*. His explicit remarks on science are for the most part quite short, dispersed widely

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throughout his writing, and often focus on the work of figures such as Einstein, Goethe, Frazer, Freud, Hertz, James, Newton, Köhler, Mach, or Weininger. In any case, most interpretations of the implications of his philosophy for the natural or social sciences have paid little, if any, attention to what Wittgenstein had to say about particular scientists, or even his more general views about specific natural or social sciences. Instead, the starting point is usually an exposition of Wittgenstein on rule-following in the *Philosophical Investigations*, and the insights into the nature of science, knowledge and society that are supposed to follow from the "rule-following considerations."

In "Extending Wittgenstein: The Pivotal Move from Epistemology to the Sociology of Science," a contribution to the debate over Wittgenstein's significance for the sociology of science in *Science as Practice and Culture* (Pickering 1992), Michael Lynch states that "Wittgenstein is widely regarded as the *pivotal* figure for a "sociological turn" in epistemology" (Lynch 1992, 218). But it is Peter Winch's interpretation of Wittgenstein that first brought him into the sociological limelight, and David Bloor's critique of Winch that made Wittgenstein into a canonical source for the philosophical agenda of recent sociology of scientific knowledge, commonly known as SSK. The existence of this line of influence is hardly controversial. Bloor's first paper on Wittgenstein makes clear his debt to Winch, and his reading of Wittgenstein has become a point of reference for subsequent debates within the sociology of knowledge over methodology and theory. But the role of Winch and Bloor in the reception of Wittgenstein by sociologists of science is often overlooked. This is, perhaps, because the appeal to Wittgenstein by sociologists of science has for the most part become formulaic and routine, and because it is often taken for granted that the sociology of science's Wittgenstein is taken from Kuhn, not Winch and Bloor. Wittgenstein has joined the august company of philosophical figures such as Aristotle, Hume, or Kant: all convenient historical antecedents, when an intellectual lineage is needed, but rarely read in any detail by those who invoke them in a programmatic way. In the earliest stages of Wittgenstein's reception within the sociology of science – Winch's and Bloor's early interpretations of Wittgenstein – interest in Wittgenstein had to do with the way in which he seemed to open up the possibility of appropriating and transforming the claims of classical epistemology. In more recent work, Wittgenstein has come to play two distinct but interrelated roles for those working in SSK. First, his talk of "forms of life" and the primacy of practice is invoked in order to provide philosophical legitimation for the way in which practitioners of SSK study scientific communities. Second, he has become a focal point for debates about the methodology and philosophical agenda of SSK (Bloor 1973, 1983, 1997; Pickering 1992; Lynch 1993; Friedman 1998.)

It will be helpful to begin by considering some textbook examples of the recent use of Wittgenstein within SSK as providing philosophical legitimation for the turn toward social practice. Jan Golinski's *Making Natural Knowledge: Constructivism and the History of Science* (1998), a book on SSK's methodo-

logical implications for the history of science, sums up Wittgenstein's role as follows:

> the social collectivity, ignored in the classical model of subject and object, has come to be regarded as critical for the production of knowledge. One source of this is the later philosophy of Ludwig Wittgenstein, with its claim that language finds meaning by virtue of its use in specific "forms of life" (Bloor 1983) ... Analysts who have applied Wittgenstein's notion of "forms of life" have portrayed social formations ([Kuhn's] "paradigms" or [Collins'] "core sets," for example) that are defined by particular configurations of scientific practice (Golinski 1998, 7, 47.)

The master argument for this appeal to Wittgenstein can be summarized very briefly: application of any concept is always indeterminate, and it is only the social collectivity, as studied by SSK, that can resolve this indeterminacy. Here is a representative exposition of this argument from *Science in Context: Readings in the Sociology of Science* (Barnes and Edge, 1982), a set text for Britain's Open University. The quotation is taken from an editorial introduction to Part Two of the reader, on "The culture of science", which contains readings by Kuhn, Collins, Bloor and Pickering.

> Every particular case differs in detail from every other and can never be conclusively pronounced identical to any other, or identical in any attribute to any other. Hence, nothing can be unproblematically deduced from a rule or law, concerning any particular case, because there is always the undetermined matter of whether the case falls under the rule or the law – that is, whether it is *the same as* or *different to* those instances which have already been labelled as falling under the rule or law. Formally, this matter of similarity or difference arises at every point of use of a concept, and has to be settled at every point by the using community. Concept application is inherently open-ended (Wittgenstein, 1953; Bloor, 1973; Hesse, 1974). (Barnes and Edge 1982, 70.)

A closely related exposition can be found in Pickering's introduction to *Science as Practice and Culture* (1992), although Pickering adopts a more moderate formulation, on which the master argument is not applied to every case, only new ones:

> SSK's perspective on knowledge is, however, typically underwritten by a particular vision of scientific practice that goes broadly as follows ... Since the central problematic of SSK is that of knowledge, the first move is to characterize the technical culture of science as a single conceptual network ... an image of scientific practice follows: practice is the creative extension of the conceptual net to fit new circumstances. And here SSK, following Ludwig Wittgenstein (1953) and Thomas Kuhn (1962), insists on two points. First, that extension of the net is accomplished through a process of modeling or analogy: the production of new scientific knowledge entails seeing new situations as being relatively like old ones. And second, that modeling is an open-ended process: the extension of scientific culture, understood as a single conceptual net, can plausibly proceed in an indefinite number of directions; nothing within the net fixes its future development. (Pickering 1992, 4)

Of course, this conception of scientific practice as indeterminate naturally leads to the question: "what then does produce closure in particular cases?" (Barnes and Edge 1982, 73), or as Pickering puts it, "how is closure – the achievement of consensus on particular extensions of culture – to be understood?" (1992, 4) Pickering provides the following answer: SSK provides a distinctly sociological account by appealing to the instrumental aspect of knowledge, and the interests of scientific actors.

Introduction of the distinctively sociological concept of interest serves to solve the problem of closure in two ways. On the one hand, actors can be seen as tentatively seeking to extend culture in ways that might serve their interests rather than in ways that might not. And on the other hand, interests serve as standards against which the products of such extensions, new conceptual nets, can be assessed. A good extension of the net is one that serves the interest of the relevant scientific community best. Here, then, is the basic SSK account of practice, and with this in hand we can return to the starting point – the problematic of science-as-knowledge – and articulate a position: scientific knowledge has to be seen, not as the transparent representation of nature, but rather as knowledge relative to a particular culture, with this relativity specified through a sociological concept of interest. (Pickering 1992, 4-5)

In both cases of change and continuity, the distinctively sociological feature is the turn to the interests of the people involved as the factor that takes up the slack in the network model. The interest model is not particularly prominent in Bloor's early work. However, the principal intellectual legacy of the approach he pioneered is the writing of history of science in which group interests take center stage, exemplified in Shapin's (1982) review of the early literature in this area, and Shapin and Schaffer (1985).

Interests are not just supposed to play a part in explaining the particular directions in which actors seek to extend, or modify, existing scientific knowledge, and the standards that are used in choosing between the different choices that are available. Interests are also allocated a comparable role in the maintenance of ordinary, uncontested, practices, in going on in the same way. "The suggestion is that goals and interests are associated with scientific research in all actual situations, and operate as contributory causes of the actions or series of actions which constitute the research. The causes help to explain the problem of the next case, of why a term is applied, or an exemplar extended in that particular way that time." (Barnes, Bloor and Henry 1996, 120) The appeal to interests as an explanatory principle has been irresistibly attractive to many within and beyond SSK, despite forceful objection from both exponents (Woolgar 1981, 1983, 1992) and opponents (Roth 1987, 1996, 1998).

Wittgenstein's term "form of life" has been taken over within SSK as a term for the culture of a specific scientific community, which in turn comprises its practices, interests, and ways of going on. *Scientific Knowledge: A Sociological Analysis*, a recent textbook authored by members of the Edinburgh Science Studies Program, highlights this aspect of Wittgenstein's influence in SSK.

Setting out different ways of presenting the individual as an active agent within sociology of science, they say that one leading alternative is to characterize him or her as a participant in a "form of life":

The term is Wittgenstein's, and its use here is testimony to the relevance of Wittgenstein's work, directly or indirectly, to the work of many sociologists. Those who have taken up the work of Thomas Kuhn have thereby linked themselves to Wittgenstein; so have those who have extended ethnomethods into sociology of science. Harry Collins, who makes the most frequent explicit references to forms of life in science, has used the work of the philosopher Peter Winch as a line of access to Wittgenstein's ideas. [Bloor's] finitist account of the use of scientific knowledge in this book is another version of the same position. (Barnes, Bloor, & Henry 1996, 116.)

This use of Wittgenstein is exemplified in Shapin and Schaffer's widely-read *Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life* (1985), which makes liberal use of the terms "form of life" and "language-game". In the first chapter of the book, they justify this use in the following terms:

We mean to approach scientific method as integrated into *patterns of activity*. Just as for Wittgenstein "the term 'language-*game*' is meant to bring into prominence the fact that the *speaking* of a language is part of an activity or a form of life," [Wittgenstein 1953, §23] so we shall treat controversies over scientific method as disputes over different patterns of doing things and of organizing men to practical ends. [A footnote here refers the reader to Bloor 1983, ch. 3] We shall suggest that solutions to the problem of knowledge are embedded within practical solutions to the problem of social order, and that different practical solutions to the problem of social order encapsulate contrasting practical solutions to the problem of knowledge. (Shapin and Schaffer 1985, 15)

The principal critique of the appeal to Wittgenstein's philosophy in the sociology of science is Michael Friedman's "On the Sociology of Scientific Knowledge and its Philosophical Agenda" (1998). The central theme of the paper is the tension between "the idea that SSK is an empirical scientific discipline, on the one hand, and its claim to solve the traditional problems of philosophy" (1998, 241). Friedman sums up SSK's use of Wittgenstein as follows:

This philosophical agenda of SSK, in both its theoretical and its applied versions, is explicitly traced to the work of one of the giants of twentieth century philosophy, namely Ludwig Wittgenstein. In particular, the concepts of 'language-game' and 'form of life,' which are central to Wittgenstein's *Philosophical Investigations*, are here interpreted as referring to particular socio-linguistic activities associated with particular socio-cultural groups – where the practices in question are regulated by socio-cultural norms conventionally adopted by the relevant groups. Wittgenstein's insistence on the need for renouncing traditional philosophy in favor of the careful description of particular 'language-games' expressing particular 'forms of life' is then read as the call for an empirical sociological investigation of the way in which the traditional categories of knowledge, objectivity, and truth are socially constituted and determined by the norms, needs, and interests of particular socio-cultural groups. (Friedman 1998, 240-1)

In his critique of this sociological turn, Friedman highlights four leading points on which Bloor and his colleagues misread Wittgenstein. First, SSK aims to be an empirical scientific discipline, while Wittgenstein always said that his philosophy was entirely distinct from natural science (1998, 252-3). Second, Wittgenstein often considers imaginary uses of language, not real ones, which indicates the deeply non-empirical character of his work (1998, 253-4. For an excellent discussion of the weaknesses of Bloor's interpretation of Wittgenstein's use of imaginary examples, see Cerbone 1993). Third, Wittgenstein "shows very little interest in the kind of historical and cross-cultural variation in human linguistic and cultural practices that is the basis and starting-point for the empirically oriented enterprise of SSK" (1998, 254). While he is interested in alternatives to 'our' ordinary practices, and in showing that there is no absolute necessity to those practices, the alternatives are usually imaginary and far more radical than those encountered by ethnologists or historians of science. Fourth, Wittgenstein shows no sign of interest in socio-cultural relativism. While the reference of his "we" is rarely specified – one of the ways in which his writing makes it easy for a socio-cultural relativist to find a foothold – the term is often used interchangably with "humanity" or "all human beings" (1998, 254).

However, Friedman has little to say about how or why this misreading came about, or the role of the master argument as a philosophical argument for the primacy of "forms of life" and a sociological epistemology. But the idea of reading Wittgenstein's philosophy as sociology of knowledge is an audacious one, and the story of how it came about is well worth telling. It is a story of a succession of theories, each claiming to get at what was right about the previous theory, but turning it on its head in order to do so. And it is just the kind of demystifying history of disciplinary myth-making that sociologists of science have so assiduously pursued when studying the sciences, but have for the most part been reluctant to undertake on their own behalf.

At the end of *Wittgenstein: A Social Theory of Knowledge*, David Bloor sums up the relationship between the programme of research he advocated in the sociology of science and Wittgenstein's work in the following terms:

Wittgenstein referred to his work as one of "the heirs to the subject which could be called philosophy" (Blue Book, p. 28). My whole thesis could be summed up as the claim to have revealed the true identity of these heirs: they belong to the family of activities called the sociology of knowledge. ... The point is that "'obeying a rule' is a practice" (Wittgenstein 1953, §202)

In other words, Bloor contends that his sociology of knowledge is a replacement for philosophy, as traditionally conceived. Wittgenstein provides the point of departure for this revolution, a revolution that begins with Wittgenstein's treatment of practice and ends with Bloor's sociology of knowledge. It is in this connection that Bloor cites *Investigations* §199 – "the point is that "'obeying a rule" is a practice'" – as a summary of the main point he takes from Wittgenstein. Certainly, Wittgenstein emphatically insists that obeying a rule is a prac-

tice; the significance of practice in the *Investigations* can hardly be overemphasized. But what did Wittgenstein mean by his talk of rule-following and practice, and what were his reasons for insisting on their importance? As there are deep and far-reaching disagreements among Wittgenstein interpreters on just this issue, Bloor's interpretation can best be appreciated if we first review the alternatives. This is the business of section two of this paper. In section three, I look at Winch's reading of Wittgenstein on rule-following, the epistemology he attributes to Wittgenstein, and his claim that "sociology is really misbegotten epistemology." Section four concerns Bloor's reading of Wittgenstein on rule-following, the epistemology he attributes to Wittgenstein, and his reply to Winch: "epistemology is misbegotten sociology." Section five considers what is at stake in this debate about how to read Wittgenstein and how to do philosophy and sociology of knowledge.

2. WITTGENSTEIN ON RULE-FOLLOWING: SCEPTICISM, ANTI-SCEPTICISM, AND QUIETISM

What is it to follow a rule correctly? Taken by itself, the verbal formulation of a rule does not determine its next application, for it is always possible that it will be misunderstood, and any attempt to drag in more rules to determine how to apply the original rule only leads to a vicious regress. Of course, a great deal depends on how one frames and approaches the question about rule-following that I have just sketched so quickly. The standard point of reference in discussions of this issue since 1982 has been Saul Kripke's *Wittgenstein on Rules and Private Language*. Kripke reads Wittgenstein as raising, and attempting to answer, a scepticism about rule following, that is, as replying to someone who holds that we cannot satisfactorily answer the question about what it is to follow a rule. In a sense, Kripke takes Wittgenstein's arguments to lead to scepticism, for he argues that the solution he attributes to Wittgenstein doesn't work, and does nothing to show that a better answer is possible. A few interpreters have actually read Wittgenstein as such a sceptic himself. Michael Dummett's (1959) review of Wittgenstein's *Remarks on the Foundations of Mathematics*, describes Wittgenstein as a "full-blooded" conventionalist who held that every single case of rule-following involves an element of decision, and so it is never necessary to follow a rule one way rather than another. Henry Staten's (1984) reading of the rule-following discussion in the *Investigations* also stresses the role of decision, interpreting Wittgenstein as a sceptic and deconstructionist avant la lettre. On this Derridean reading of Wittgenstein, there is an unbridgeable gap between a rule and its application, an abyss that makes any positive theory about what it is to follow a rule, or a theory of meaning, an impossibility.

While it has few supporters, the sceptical position is important because it is the point of departure for the standard approach to rule-following. Most readers agree with Kripke that Wittgenstein is replying to scepticism about rule-follow-

ing, but disagree over the right answer. The two main camps are known as "individualists" and "communitarians." "Individualists," such as Colin McGinn (1984) and Simon Blackburn (1984, 1984a) maintain that the resources for a solution can be provided by a single individual. In other words, the practices involved in following a rule may be the practices of an isolated individual. "Communitarians" such as Peter Winch (1958) or David Bloor (1973, 1997), hold that answering the sceptical problem is only possible if one is a member of a community – a group of a certain kind – and so the practices in question must be social, if not community-wide. Before the publication of Kripke's book, it was usually taken for granted that Wittgenstein was offering a communitarian solution. While Kripke himself endorses this reading, he highlighted the importance of the distinction between individualists and communitarians, and the differences between them became a leading issue in the resulting controversy. For instance, Bloor's first book on Wittgenstein (1983) takes it for granted that he was a communitarian, and argues for a sociological construal of "community" and "practice"; his second (1997) is an extended defence of communitarianism.

Kripke and Bloor agree that Wittgenstein begins by arguing that meaning is underdetermined by the available evidence and then provides a community-based solution – meaning is determined by the community's social practices, or "form of life". But Kripke argues that this is only a second-best, 'sceptical' solution, one which does not meet the standards set by the sceptical problem about meaning, while Bloor (1973, 1997) maintains that appealing to a community is a 'straight' solution, one that really does solve the sceptical problem. In other words, Bloor accepts Kripke's starting point, an argument that there is a gap between a rule and its application, but holds that social practices, the forms of human activity studied by the sociologist, provide the answers.

A third alternative is to hold that the debates between sceptics and anti-sceptics, individualists and communitarians, miss the point, which is that Wittgenstein aims to show us that there is no philosophical problem about rule-following, or determinate meaning. On this approach, Wittgenstein aims to get rid of arguments about meaning, not provide a theory of it. Problems about rule-following only lead to scepticism if one approaches understanding a sentence or a rule wrongly, such as thinking of understanding as consisting in giving an interpretation. But Wittgenstein considers such views of rule-following to be mistaken, and without them, the paradox does not get started. There is no "gap" of the kind that concerns both sceptic and anti-sceptics. Admittedly, there is considerable *prima facie* textual support for each of those readings – passages which certainly look as if they formulate and defend sceptical and anti-sceptical theories of meaning – both within the *Philosophical Investigations* and Wittgenstein's other writings. One can read Wittgenstein as either a sceptic or an anti-sceptic, depending on which of those passages one plays up and which one plays down. But neither reading can explain why so much of the material on rule-following, and rule-scepticism, is in the form of a dialogue, a dialogue in which the two positions are played out against each other in such detail. Scepticism and

anti-scepticism about meaning fascinated Wittgenstein, but he was always concerned with finding a way out of it, not in defending one side or the other. (For further discussion of this approach, see Stern 1995.)

This reading is often known as "quietism," for its denial that Wittgenstein has anything to say on the subject of grand philosophical theories about the relation between language and world. According to the quietist, Wittgenstein's invocation of forms of life is not the beginning of a positive theory of practice, or a pragmatist theory of meaning, but rather is meant to help his readers get over their addiction to theorizing about mind and world, language and reality. Hilary Putnam, Cora Diamond, and John McDowell are among the leading advocates of this approach. McDowell observes that Wittgenstein's readers often take his talk of "customs, practices, institutions," and "forms of life" as the first steps towards a positive philosophy. The point of the positive views would be to give a non-intentional, or non-normative, justification of our talk of meaning and understanding, by placing it in a broader context of human interaction, interaction which can be described in non-intentional terms.

But there is no reason to credit Wittgenstein with any sympathy for this style of philosophy. When he says "What has to be accepted, the given, is – so one could say – forms of life" ([*Philosophical Investigations*] p. 226) his point is not to adumbrate a philosophical response, on such lines, to supposedly good questions about the possibility of meaning and understanding, or intentionality generally, but to remind us of something we can take in the proper way only after we are equipped to see that such questions are based on a mistake. His point is to remind us that the natural phenomenon that is normal human life is itself already shaped by meaning and understanding (McDowell 1993, 50-51).

Within the sociology of science, one can find echoes of this view in Michael Lynch's (1993) and Jeff Coulter's (1993) ethnomethodological readings of Wittgenstein. Lynch, Coulter and their fellow ethnomethodologists still see room for a scientific study of science that would take its cue from Wittgenstein's insistence on the interwovenness of language, activity, and practice, aiming at "thick description" rather than a formal theory. The debate between Bloor (1992) and Lynch (1992, 1992a) in *Science as Practice and Culture* (Pickering 1992) is over whether to read Wittgenstein with Bloor as a communitarian who provides the basis for a sociological theory of practice, or to follow Lynch in reading Wittgenstein as a quietest precursor of ethnomethodology. Bloor insists that a sociological theory of practice is necessary to show how a rule and its application are tied together; Lynch, following Baker and Hacker, replies that there is a firm logical connection between a rule and its application, and so no need for Bloor's theory. But the best place to begin, if we are to see how Wittgenstein's discussion of rule-following came to be taken up within SSK, is with Peter Winch's *The Idea of a Social Science* (1958).

3. WINCH'S WITTGENSTEIN:
"SOCIOLOGY IS REALLY MISBEGOTTEN EPISTEMOLOGY"

Although nothing could have been further from Winch's intentions, it was his interpretation of Wittgenstein in *The Idea of a Social Science and its Relation to Philosophy* (1958) that proved to be the crucial link in the transformation of Wittgenstein's ideas about rule-following into a new sociology of knowledge. One of the main aims of that very short book was to argue against the view that the method of the social sciences should be the method of the natural sciences, as conceived of by logical positivist and empiricist philosophy of science. It also argued for an interpretive approach to social science that begins from the unreflective understanding of the participants. Winch's presentation of the case for the distinctively interpretive character of social science presupposes a sharp contrast with a positivistic conception of natural science:

> I do not wish to maintain that we must stop at the unreflective kind of understanding ... But I do want to say that any more reflective understanding must necessarily presuppose, if it is to count as genuine understanding at all, the participant's unreflective understanding. And this in itself makes it misleading to compare it with the natural scientist's understanding of his scientific data. (Winch 1958/1991, 89)

Winch maintains that language and action – what people say and do – cannot be understood in isolation from their broader practical and cultural context. Drawing on an expression of Wittgenstein's, he calls this context the "forms of life" of the people in question. Because of the way in which what we say and do is embedded within this broader context, language and world are inextricably intertwined. One consequence that Winch draws is that the realist's conviction that reality is prior to thought, that the world is independent of our ways of representing it, is incoherent:

> Our idea of what belongs to the realm of reality is given for us in the language that we use. The concepts we have settle for us the form of the experience we have of the world. ... The world *is* for us what is presented through those concepts." (Winch 1958, 15)

Because social institutions embody ideas of what is real and how it is to be understood, Winch holds that causal methods will prove utterly inadequate for the task of understanding our social world: "the central concepts which belong to our understanding of social life are incompatible with concepts central to the activity of scientific prediction" (Winch 1958, 94). Winch is not merely making the familiar claim that the methods of the natural sciences will prove unsuccessful when applied to social questions, but that the very attempt to do so is logically flawed, and strictly speaking, nonsense.

Winch's central argument for these far-reaching conclusions is contained in his exposition of Wittgenstein's account of rule-following in the *Investigations*

(Wittgenstein, 1953 §§ 243ff.; Winch 1958, 24-39). Winch begins by pointing out that words do not have meaning in isolation from other words. We may explain what a word means by giving a definition, but then one still has to explain what is involved in following a definition, in using the word in the same way as that laid down in the definition. For in different contexts, "the same" may be understood in different ways: "It is only in terms of a given *rule* that we can attach a specific sense to the words 'the same.'" (Winch 1958, 27) But of course the same question can be raised about a rule, too: how are we to know what is to count as following the rule in the same way? Given sufficient ingenuity, it is always possible to think up new and unexpected ways of applying a rule. However, in practice we all do, for the most part, unreflectively follow a rule in the same way: "given a certain sort of training everybody does, as a matter of course, continue to use these words in the same way as would everybody else. It is this that makes it possible for us to attach a sense to the expression 'the same' in a given context." (Winch 1958, 31)

An essential part of the concept of following a rule, Winch contends, is the notion of making a mistake, for if someone is really following a rule, rather than simply acting on whim, for instance, we must be able to distinguish between getting it right and getting it wrong. Making a mistake is to go against something that "is *established* as correct; as such, it must be *recognizable* as such a contravention. ... Establishing a standard is not an activity which it makes sense to ascribe to any individual in complete isolation from other individuals" (Winch 1958, 32). Rule-following presupposes standards, and standards presuppose a community of rule-followers.

In a section on the relations between philosophy and sociology, where Winch sums up the results of this argument, he describes it as a contribution to "epistemology." Winch makes it clear that epistemology, as he uses the term, has little to do with traditional theories of knowledge, but is instead his preferred name for first philosophy, that part of philosophy which is the basis for all others. For epistemology, as Winch understands it, deals with "the general conditions under which it is possible to speak of understanding" and so aims at elucidating "what is involved in the notion of a form of life as such" (Winch 1958, 40-1). Thus, on Winch's reading, "Wittgenstein's analysis of the concept of following a rule and his account of the peculiar kind of interpersonal agreement which this involves is a contribution to that epistemological elucidation" (Winch 1958, 41).

Like the "private language arguments" that were so much discussed during the 1960s and 1970s, this one turns on the need for a community if one is to follow rules. In retrospect, Winch's argument is extremely compressed, and a full defence would require that it respond to many of the now-familiar difficulties that have been rehearsed so often in the interim. However, for our purposes the most important point is the *use* Winch made of this argument. His contemporaries took the main force of the private language to be the negative consequences for traditional approaches to epistemology such as Cartesian dualism, scepticism and phenomenalism. Winch, on the other hand, used it to argue for a new

conception of epistemology, as the result of following through the implications of his Wittgensteinian grammatical analysis. That epistemology could be positively applied to questions about the nature of society, questions that had previously been regarded as empirical questions for the sociologist:

... the central problem of sociology, that of giving an account of the nature of social phenomena in general, itself belongs to philosophy. In fact, not to put too fine a point on it, this part of sociology is really misbegotten epistemology. I say 'misbegotten' because its problems have been largely misconstrued, and therefore mishandled, as a species of scientific problem. (Winch 1958/1991, 43)

To sum up: Winch puts forward a quite general philosophical argument that neither formal logic nor empirical hypotheses are appropriate methods for the study of society. Instead, one must aim at an interpretive investigation of that society's ideas and forms of life, a philosophical investigation that will make clear the kind of work that is appropriate within the social sciences. At first, Winch's argument is extremely general, and primarily concerns what he calls "the notion of human society." But later on, he sets out specific consequences for different modes of social life within a society. Not only is there an internal – logical – relation between the notion of human society and the forms of human life, but social relations between people are also internal, for the structure of our language and our relations with others are two sides of the same coin. "If social relations between men exist only in and through their ideas, then, since the relations between ideas are internal relations, social relations must be a species of internal relation too." (Winch 1958, 123)

As a result, aspects of social life far from what is usually considered the domain of philosophy, such as the significance of a pointed glance or gesture in a conversation or the giving of historical explanations (Winch 1958, 129-30, 133) have to be understood by interpreting the logic of what actors do and say. Thus it turns out that not only central questions about the nature of social phenomena, but also the detailed understanding of particular aspects of our lives, cannot be approached by the methods of natural science, but only by those particularistic, interpretive methods recommended by Winch's epistemology of forms of life.

Winch argues that Wittgenstein's treatment of rule-following shows that we must start in social science with "forms of life," the social practices of human groups. This turn to forms of life, understood as culture-specific practices, is one way of supplanting the central role occupied by representation in traditional philosophy of science – "knowledge that" – with skills or abilities – "know how." But a great deal turns on just how one conceives of this embedding of knowledge in social practice, in "forms of life." Bloor, and others working in the sociology of scientific knowledge, hold that practical context, "language games" and "forms of life," play a central role in Wittgenstein's later philosophy because they function as a background against which determinate meanings are possible. In turn, this is supposed to make possible a *scientific* and *naturalistic* understanding of the relationship between science, practice and culture.

4. BLOOR'S WITTGENSTEIN: "EPISTEMOLOGY IS MISBEGOTTEN SOCIOLOGY"

In a paper on the relationship between logic and sociology, Jeff Coulter has described Winch as "perhaps the most important figure in the history of the (renewed?) relationship between Logic and Sociology," because "Winch sought to 'dissolve' sociology into conceptual/grammatical analysis of a Wittgensteinian kind" (Coulter 1991, 32-3). In the years immediately following the publication of *The Idea of a Social Science* (1958) and "Understanding a Primitive Society" (1964), the vast majority of professional sociologists in Britain and America considered Winch's views unacceptable. One need look no further than the threat those views posed to the project of an autonomous sociology and the rejection of the possibility of systematic sociological laws.

On the other hand, Winch's respect for the particularity of other forms of life, and the need to understand them from within, was enormously attractive to those who wished to approach scientific cultures along comparable lines, by combining Kuhn's notion of a paradigm with Winch's account of understanding another culture. The crucial move here was to conceive of the culture of a particular group of scientists – one of the senses of Kuhn's famously slippery term, "paradigm," – along lines suggested, if not required, by Winch's discussion of forms of life. Bloor simply takes it for granted that the term "form of life" refers to specific cultural or social groups, social entities comparable to the "primitive societies" discussed by Winch, or Kuhn's scientific research cultures. This way of understanding science as a culture involved a conception of culture and knowledge very different from the thin and formal accounts of the nature of science and knowledge prevalent in the philosophy of science at the time. In this way David Bloor found a way of reading Wittgenstein and Winch as providing the point of departure for a reinvigorated sociology of scientific knowledge. Bloor's guiding insight in this was that Winch's positive conception of sociology as epistemology, as a study of the network of relations between actors and the world, could be prized loose from Winch's claim that "sociology is really misbegotten epistemology" (Winch 1958, 43). In effect, the distinctive feature of this post-Winchian sociology of science is that it sought to reverse the direction of Winch's program, and dissolve Winch's epistemology into sociology.

Broadly speaking, Bloor's reading of Wittgenstein starts from the programmatic conviction that the ahistorical, a priori reasoning about science favoured by the "philosophical tradition" had to be replaced by hard empirical research: a genuinely scientific and naturalistic study of science. Close attention to scientific culture and its characteristic practices led him to see that epistemology could not be done by simply reflecting on the necessary and sufficient conditions for the nature of such concepts as rationality, experimental success, or knowledge. Instead, one had to empirically investigate scientific forms of life and look at the

role of social relations in scientific practice. This, then, was to be SSK's conception of "epistemology naturalized": epistemology sociologized, the philosophy of knowledge to be replaced by the sociology of knowledge.

Bloor replaced Winch's sharp distinction between social and natural science, and the contrast between internal relations in the realm of social relations and external relations connecting scientific laws and observation, with a single holistic web, or network, of concepts, concepts connected by the rules that make up forms of life. But where Winch had seen social relations as expressions of ideas, he turned this idea around, and saw ideas as expressions of social relations. Bloor's transformation of Winch's conception of science and society is nicely summarized in Collins' *Changing Order*, a book that explicitly draws on Bloor's reading of Winch. Collins sums up the attractions of the "network model" of concepts by quoting the following words from Winch:

A man's social relations with his fellows are permeated with his ideas about reality. Indeed, 'permeated" is hardly a strong enough word: social relations are expressions of ideas about reality (Winch, 1958, p. 23).

and adds

We must add that the converse is equally true, that ideas are expressions of social relations" (Collins 1985/1992, p. 132).

But what is it for ideas to be expressions of social relations? Here one is faced with a series of vertiginously difficult questions, questions that are for the most part swept under the rug with the presumption that the regress of justification hits bedrock once we arrive at interests and forms of life.

Much as Winch's assumption that the natural sciences uniformly employ positivistic methods makes it easy for him to draw a sharp distinction between them and the hermeneutic social sciences, Bloor's assumption that the "philosophical tradition" is aprioristic makes it easy for him to draw a sharp distinction between that tradition and the new methods of SSK. Following Winch, Bloor conceives of epistemology as the "queen of the sciences," the discipline that characterizes the nature of knowledge and thus lays down the law to the other disciplines. Bloor and Winch agree that knowledge has an essence; their disagreement concerns whether philosophy or sociology is best placed to adjudicate its nature. Thus "Wittgenstein and Mannheim on the sociology of mathematics," Bloor's first published statement of the Strong Programme, an important point of departure for much subsequent work in SSK, ends with the following footnote:

Whereas Winch thinks that much sociology is misbegotten philosophy, the argument of this paper has been that much philosophy is misbegotten sociology. There is an irony about Winch's position which seems to have passed unnoticed. He believes that a proper philosophical understanding will illuminate our understanding of society. The example of philosophical clarity that he appeals to, which is Wittgenstein's analysis of rule following, in fact illustrates the opposite. It shows that a proper grasp of social and institutional processes is necessary for philosophical clarity. Rather than philosophy illuminating the social

sciences Winch unwittingly shows that the social sciences are required to illuminate philosophical problems. (Bloor 1973, 191 n 46)

The principal contention of the paper is that Wittgenstein solves Mannheim's problem in *Ideology and Utopia* (1936), namely his inability to give a sociological analysis of logic and mathematics. (For illuminating and, in some ways, parallel discussions of Bloor's use of Mannheim, see Pels 1996 and Kaiser 1998.) Bloor argues that Wittgenstein made possible a reply to a view he calls "Realism," a conception of mathematics on which the mathematician discovers truths about a pre-existing realm that is quite independent of our mathematical activity (1973, 42.) To think of mathematics in this way, Bloor contends, is to conceive of it as a structured and bounded realm, apart from yet somehow connected to the world we live in. It leads to an epistemology that corresponds to that ontology: mathematical knowledge consists in first gaining access to that domain and then moving within it.

Bloor's reply to Realism turns to Wittgenstein's discussion of what is involved in continuing a simple number sequence, such as 2, 4, 6, 8 ... The Realist takes it for granted that "the correct continuation of the sequence, the true embodiment of the rule and its intended mode of application, exists already" (Bloor 1973, 181.) But the Realist conception of rule-following "fails to provide answers to the problems that it was designed to solve. These problems are: How can we make the *same* steps again and again; what makes 'the same' the same; what guarantees the identical character of the steps at the different stages of the rule's application?" (Bloor 1973, 181.) Indeed, how could such an archetype guide someone who was trying to follow it?

For how does the human actor, following the supposed archetype, know that it really is the correct embodiment of the rule he wants? To know that the archetype is correct requires exactly the knowledge that was considered problematic in the first place, *viz.* knowledge of how the rule goes. It emerges that this argument is quite general. It works for any archetype, this-worldly or other-worldly. The trouble with Realism does not lie in the puzzling nature of its ontology but in the circular character of its epistemology. It presupposes precisely what it sets out to explain. (Bloor 1973, 182)

However, this train of argument isn't a knock-down argument against Realism. For it depends on the Realist's agreeing that the actor must select, or identify, the archetype that is to be followed. Once this is granted, the way is open for the Wittgensteinian to reply that the act of selection, or identification, calls for the very skills that the archetype was supposed to explain. On Bloor's naturalistic, causal, framework, there appears to be no alternative. But if one accepts a teleological perspective, one on which either actors have a natural tendency toward the truth, or on which the archetypes actively impose themselves on us, there is no longer any need to invoke the actor's abilities to know what is the same, and what is different. Bloor holds that the teleological approach cannot be refuted, but can be made far less plausible by articulating the "consistent sociological account towards mathematics" (Bloor 1973, 189) he finds in Wittgenstein. On

this approach, the meaning of a formula, or rule, is simply a matter of how it is applied, or used, and this, in turn, is "the culmination of a process of socialization" (Bloor 1973, 184). This is how the question concerning the correct application of a rule in a particular instance is to be answered. Moving on to the more general question, as to what makes several applications of a rule consistent with each other, Bloor's answer begins with the same point. People have been trained to behave in certain ways, and so when faced with new circumstances, are likely to respond in a similar way. While there is no external standard of correctness, particular practices can be criticized by appeal to other practices. The appeal to what is natural presupposes a certain background of physical and psychological facts, but these are compatible with considerable variation on the cultural level, and so rule-following is institutional rather than instinctual (Bloor 1973, 186.) In what sense, then, are we compelled by the laws of logic?

Logic compels by the sanctions of our fellow men:

Nevertheless the laws of inference can be said to compel us; in the same sense, that is to say, as other laws in human society. (Wittgenstein 1956 I 116)

Wittgenstein does not deny that logic compels. What he offers is an explanation of the content of the compulsion. ... The importance of the institution explains why we learn to count as we do. ... It is *we* who are inexorable. ... Perhaps the most significant conclusion is that mathematics can now be seen as invention rather than discovery (Bloor 1973, 187-8).

As a result, "calculation and inference are amenable to the same processes of investigation, and are illuminated by the same theories, as any other body of norms. ... The great insight of [Wittgenstein's] *Remarks* is that it treats the grip that logic has upon us as a fact to be explained rather than a truth to be justified" (Bloor 1973, 189-190).

What Bloor misses here, in his determination to find a non-normative and purely causal bottom level of explanation in Wittgenstein's writings on rule-following, is that for Wittgenstein, following a rule does have a normative significance. Wittgenstein's point of departure is that standards for assessment of correctness, of correct and incorrect usage, are part and parcel of our grasp of the rules of logic, or of the language we speak.

"How am I able to obey a rule?" – if this is not a question about causes, then it is about the justification for my following the rule in the way that I do. (Wittgenstein 1953, §217)

If we are to give a causal explanation of what we do when we follow rules, the best we can do is talk about how we learned them, and the conditions under which people conform to them. We can talk about the role of training, and the role of institutions in making sure that certain regularities are maintained. On the other hand, we can not only talk about how one *will* act, but about justification, about what one *ought* to do. In that case, what is at issue is "what actions accord with the rule, are obliged or permitted by it, rather than what my grasp of it actually makes me do" (Brandom 1994, 15). This contrast is a central theme in

the rule-following passages in both the *Philosophical Investigations* and the *Remarks on the Foundations of Mathematics*: "Many of his most characteristic lines of thought are explorations of the inaptness of thinking of the normative 'force,' which determines how it would be appropriate to act, on the model of a special kind of causal 'force'" (Brandom 1994, 14). Wittgenstein's principal point here, about the connection between the existence of a community's common responses and the existence of practices is that proprieties of practice *presuppose* agreement in judgements, actions, and behaviour, but are not *reducible* to them.

But we do not need to undermine a non-naturalistic conception of reason in order to give a causal story, and for this reason the Winch-and-Wittgenstein-inspired scepticism with which Bloor begins is unnecessary. According to Bloor, we need to "stop the intrusion of a non-naturalistic notion of reason into the causal story" (1991, 177). This is a widespread conviction within SSK, as Roth (1996, 1998) and Friedman (1998) have observed. Shapin (1982), a frequently cited review of the early SSK-influenced literature in the history and sociology of science, begins with a philosophical response to the threat SSK supposedly faced from philosophical 'realism' and 'rationalism.' Likewise, Collins begins *Changing Order* (1985/1992) with a chapter on Wittgenstein-inspired scepticism and how forms of life provide a sociological solution to epistemological and linguistic scepticism. But we do not need a philosophical argument in order to refrain from normative considerations. We can, in the words of Nancy Reagan, "just say no." There is no need to refute philosophical 'realism' or 'rationalism' before one can legitimately go to work describing what goes on in scientific practice or looking at scientific knowledge from a sociological perspective. Similarly, "we can seek to explain why scientific beliefs are in fact accepted without considering whether they are, at the same time, rationally or justifiably accepted" (Friedman 1998, 245).

5. SSK'S WITTGENSTEIN: A MISBEGOTTEN EPISTEMOLOGY AND SOCIOLOGY

If we accept the "compatabilist" resolution of the supposed dispute between philosophy and sociology offered at the end of the previous section, then the philosopher who wants to ask normative and justificatory questions about science, and the sociologist of scientific knowledge who wants to ask descriptive and explanatory questions about science need not disagree. In that case, we may be left wondering what the dispute was about. Why did Bloor and his colleagues think it essential to produce a philosophical refutation of philosophical approaches to the study of science? And why did so many philosophers respond in kind? Why has this dispute been so heated and so prolonged, and why have so few participants even considered the program of pluralist tolerance and peaceful coexistence Friedman proposes? A large part of the answer must be that each

side has seen the other as not just mistaken but as an illegitimate claimant to its rightful inheritance, and so threatening to supplant it. Peaceful coexistence has proven impossible because each side can see no way of peacefully sharing the intellectual terrain.

To see how charged the dispute has been from the very beginning, we need go no further than Winch and Bloor's pointed use of "misbegotten", a term that has both a descriptive and a normative significance, to describe the other's discipline. The literal meaning of "misbegotten" is "unlawfully begotten; illegitimate; a bastard"; unsurprisingly, the *Shorter Oxford English Dictionary* also states that it can be used figuratively and as a "term of opprobrium". Philosophers of knowledge have traditionally held that the sociology of knowledge is illegitimate, if not impossible, because knowledge is the concern of epistemology; sociologists of knowledge have replied that epistemology is illegitimate, if not impossible, because knowledge is the concern of sociology. In order to mount a frontal attack on philosophy, sociologists have felt the need for philosophical argument, though they have usually vacillated on the question of whether they were putting an end to epistemology ("epistemology is misbegotten sociology") or setting it straight ("epistemology is really social epistemology"). But to think that we must refute or transform the "philosophical tradition" in order to go ahead is to begin not with practice, but by tilting at windmills. SSK would do better to give up its dependence on a fictional alter ego and the misbegotten arguments that went along with it, and concentrate on doing what it does best: studying what scientists do.

NOTES

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