On the Sociology of Scientific Knowledge and its Philosophical Agenda

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For a philosopher seriously interested in the history and historiography of science, perhaps the most interesting—if also the most troubling—theoretical development of the last twenty years has been the sociology of scientific knowledge (SSK) articulated at Edinburgh by Barry Barnes and David Bloor and at Bath by H. M. Collins.[1] On the one hand, this program has inspired innovative work in the history of science that has applied methods of social history to key events in the evolution of modern science so as to yield extraordinarily rich delineations of the wider social, cultural, and political context of these events.[2] On the other hand, however, SSK, both theoretical and applied, has been framed by an explicitly philosophical agenda—an agenda that aims to reject the traditional philosophical ideal of universal standards of rationality, objectivity, and truth (which ideal has, of course, been traditionally taken to be paradigmatically exemplified in modern science itself) in favor of a relativistic conception of scientific rationality, objectivity, and truth that grounds these concepts, in the end, in local and particular social and cultural cir-

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[1]B. Barnes, *Scientific Knowledge and Sociological Theory* (London: Routledge, 1974), *T. S. Kuhn and Social Science* (London: Macmillan, 1982); D. Bloor, *Knowledge and Social Imagery* (London: Routledge, 1976; second edition, Chicago: University of Chicago Press, 1991), *Wittgenstein: A Social Theory of Knowledge* (New York: Columbia University Press, 1983); B. Barnes and D. Bloor, 'Relativism, Rationalism and the Sociology of Knowledge', in M. Hollis and S. Lukes, eds, *Rationality and Relativism* (Oxford: Blackwell, 1982); H. M. Collins, *Changing Order* (London: Sage, 1985; second edition, Chicago: University of Chicago Press, 1992).

[2]Paradigmatic examples of this kind of social history are S. Shapin and S. Schaffer, *Leviathan and the Air-Pump* (Princeton: Princeton University Press, 1985), and S. Shapin, *A Social History of Truth* (Chicago: University of Chicago Press, 1994). Shapin provides a valuable survey (as of 1982) of historical work in this tradition in 'History of Science and its Sociological Reconstructions', *History of Science* **20** (1982), 157–211.

cumstances. According to what I will call the philosophical agenda of SSK, that is, all there ultimately is to the notions of rationality, objectivity, and truth are local socio-cultural norms conventionally adopted and enforced by particular socio-cultural groups.

Barnes puts the idea this way:

> Science is not a set of universal standards, sustaining true descriptions and valid inferences in different specific cultural contexts; authority and control in science do not operate simply to guarantee an unimpeded interaction between 'reason' and experience. Scientific standards themselves are part of a specific form of culture; authority and control are essential to maintain a sense of the reasonableness of that specific form. Thus... science should be amenable to sociological study in fundamentally the same way as any other form of knowledge or culture.[3]

In a well-known article explicitly devoted to these philosophical questions, Barnes and Bloor explain that

> [the relativist] accepts that none of the justifications of his preferences can be formulated in absolute or context-independent terms. In the last analysis, he acknowledges that his justifications will stop at some principle or alleged matter of fact that has only local credibility... For the relativist there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such. Because he thinks that there are no context-free or super-cultural norms of rationality he does not see rationally and irrationally held beliefs as making up two distinct and qualitatively different classes of things.[4]

Collins, for his part, dubs his point of view 'the Empirical Programme of Relativism' and devotes the initial chapter of his major work to considering themes from 'philosophical skepticism' which point in an epistemologically relativistic direction.[5] And it is not too much to say, I believe, that this relativistic and anti-traditional philosophical agenda is responsible for a large part of the intellectual excitement that has surrounded SSK in both its theoretical and applied versions.

Moreover, this philosophical agenda of SSK, in both its theoretical and 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 and practices 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 inves-

[3]*T. S. Kuhn*, *op. cit.*, note 1, p. 10.
[4]'Relativism', *op. cit.*, note 1, pp. 27–28.
[5]*Changing Order*, *op. cit.*, note 1, Chapter 1.

tigation 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. This idea is of course most explicit in the work of Bloor, who devotes an entire book to just such a sociological (and relativistic) reading of Wittgenstein. And Bloor's conclusion is nothing less than that the sociology of knowledge should be the *replacement* for traditional philosophy.[6] For Bloor, the traditional problems of philosophy—which concern, in particular, 'the nature of rationality, objectivity, logical necessity and truth'[7]—can after all be solved, and solved by empirical sociological means.[8] That traditional philosophers will find this ambition especially troubling goes without saying.

This paper is an exploration of the philosophical agenda of SSK. I will argue that there are deep and significant tensions between the idea that SSK is an empirical scientific discipline, on the one hand, and its claim to solve the traditional problems of philosophy (and, indeed, to replace the philosophical tradition), on the other. I will approach these tensions by exploring the ways in which the aims, methods, and arguments of SSK contrast with both those of the philosophical tradition and those of Wittgenstein himself. My own aim, however, is not to indulge in disciplinary boundary drawing. I wish rather to extend an invitation to philosophically minded practitioners of SSK to engage more fully and systematically with the philosophical context and tradition that frames their philosophical agenda. For I believe, more specifically, that SSK has acted precipitously in actively seeking to embrace and to advocate one particular side in a traditional philosophical debate. Although there are indeed important reasons why disputes over philosophical relativism have arisen in precisely this intellectual context, I would urge philosophically minded historians of science to resist the temptation to become partisans in these disputes and instead to treat both philosophy and science in an historically and contextually sensitive manner. We should seek, in particular, to transcend the starkly asymmetrical treatment of science and philosophy originating with Thomas Kuhn's ground breaking work on scientific revolutions.[9] This work, as is well known, uses sensitive attention to the history of science to argue for a new philosophical picture of the nature of science. At the same time, however, the history of philosophy receives no sensitive attention at all, but is rather portrayed stereotypically as a battle between a new, progressive view and 'the' tradition—which latter

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[6]Invoking Wittgenstein's own characterization of his work as 'one of the heirs to the subject which used to be called "philosophy"', Bloor continues (*Wittgenstein*, *op. cit.*, note 1, p. 183): '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.'

[7]*Knowledge* (second edition), *op. cit.*, note 1, p. 83.

[8]Collins, too, indicates that his program is to solve the epistemological problems traditionally raised by philosophy: *Changing Order*, footnote 16 to Chapter 1.

[9]T. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

is treated as an undifferentiated and usually entirely unspecified monolith. Of course it is perfectly normal for new intellectual movements to treat the history of philosophy in this way. My point here is simply that it is now time for philosophically minded social historians of science to move beyond this stage.[10]

1. SSK and Traditional Philosophy

SSK is intended to be a naturalistic, empirical scientific enterprise. Indeed, it is by emphasizing precisely this that we rebut the common idea that SSK is intended as some kind of rejection or 'denigration' of science:

> [T]he sociology of knowledge is emphatically not, as its critics often mistakenly believe, itself a denigration of science; on the contrary, it is in many ways modelled upon scientific investigation, and any claims to credibility it comes to have must be closely related to those of science itself. The sociology of knowledge is a matter-of-fact, empirical field of study which happens to include, among its subject-matter, the knowledge and culture of science.[11]

Accordingly, the naturalistic and purely descriptive aims of SSK are explicitly contrasted with the normative and prescriptive—'moralizing'—aims of traditional philosophy.[12] And, in this sense, SSK eschews all temptation to evaluate historically given scientific practices from the point of view of a 'universal reason' and confines itself rather to describing these practices as they actually exist:

> Sociology is a subject with a naturalistic, rather than a prescriptive or normative orientation; it simply tries to understand the convictions and the concepts of different cultures as empirical phenomena. External evaluation of the convictions and concepts is irrelevant to this naturalistic concern; all that matters is why they were actually sustained.[13]

We thereby explain the widespread distaste for the relativistic conclusions of SSK within the academic community:

> A plausible hypothesis is that relativism is disliked because so many academics see it as a damper on their moralizing. A dualist idiom, with its demarcations, contrasts, rankings and evaluations is easily adapted to the tasks of political propaganda or self-congratulatory polemic. This is the enterprise that relativists threaten, not science…

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[10]Historians of science in this tradition can perhaps take encouragement from the circumstance that historians of philosophy are themselves paying increasing attention to social and other contextual considerations. In addition to the works cited in note 69 below (which pay particular attention to the concomitant history of science), I would here cite K. Kohnke, *Entstehung und Aufstieg des Neukantianismus* (Frankfurt: Suhrkamp, 1986); (partially) translated by R. Hollingdale as *The Rise of Neo-Kantianism* (Cambridge: Cambridge University Press, 1991), and F. Beiser, *The Fate of Reason: German Philosophy from Kant to Fichte* (Cambridge, MA: Harvard University Press, 1967).

[11]Barnes, *T. S. Kuhn*, p. xi.

[12]See Barnes, *op. cit*., pp. 58–63.

[13]*Op. cit*., p. 5.

> If relativism has any appeal at all, it will be to those who wish to engage in that eccentric activity called 'disinterested research'.[14]

Paradoxically, then, it is the sober and empirically based relativism of SSK rather than the traditional (philosophical) 'Cult of Rationalism' that best represents the traditional values of disinterested and empirical scientific research.

Moreover, the practitioners of SSK are unanimous that sociological explanations of 'knowledge' and 'truth' do not employ these terms as they are used within traditional philosophy. Whereas in traditional philosophy knowledge is understood to be justified true belief, so that, in particular, we have a sharp distinction between what is taken to be and what actually is knowledge, for SSK 'knowledge' refers rather to 'any collectively accepted system of beliefs'[15] or to 'whatever people take to be knowledge… those beliefs which people confidently hold and live by'.[16] Similarly, whereas nothing is more basic to traditional philosophy than the 'distinction between what is "true" and what is merely taken to be so', in SSK this distinction is definitively abandoned: truth is simply identified with 'the body of locally credible knowledge'.[17] These terminological divergences clearly reflect the fundamental divergence in aims noted above. The traditional notions of 'knowledge' and 'truth' are normative rather than descriptive: 'truth' means what ought to be believed, 'knowledge' means what is *rationally* or *justifiably* accepted. In rejecting the normative aims of traditional philosophy in favor of an explicitly non-evaluative and purely empirical point of view, SSK must also eschew the use of all traditional normative notions. Rather than articulating the structure of what *ought* to be believed, SSK simply describes and explains what is *in fact* believed.

In this way we straightforwardly obtain the celebrated 'symmetry', 'impartiality', or 'equivalence' postulate:

> Our equivalence postulate is that all beliefs are on a par with one another with respect to the causes of their credibility. It is not that all beliefs are equally true or equally false, but that regardless of truth and falsity the fact of their credibility is to be seen as equally problematic. The position 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 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.... [The practitioners of SSK] simply investigate the contingent determinants of belief and reasoning without regard to whether the beliefs are true or the inferences rational. They exhibit the same degree and kind of curiosity in both cases.[18]

As we have seen, SSK is an empirical and purely descriptive discipline, which, for this very reason, must leave the traditional normative or evaluative uses of 'knowledge', 'truth', and 'rationality' wholly out of account. The aim is to explain,

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[14]Barnes and Bloor, 'Relativism', p. 47, footnote 44.
[15]*Op. cit.*, p. 22, note 5.
[16]Bloor, *Knowledge* (second edition), p. 5.
[17]See especially Shapin, *Social History*, *op. cit.*, note 2, pp. 3–8.
[18]Barnes and Bloor, 'Relativism', p. 23.

not why beliefs are rationally or *correctly* accepted, but simply why beliefs are *in fact* accepted. In thus explaining why beliefs are in fact accepted—how local consensus is in fact achieved—we clearly have no option but, in all cases, regardless of the normative status of the beliefs in question, to appeal to purely naturalistic and entirely empirical contingent causal factors. And, since what is in fact believed—what is in fact locally credible—obviously varies from context to context and from group to group, a naturalistic and empirically based relativism is the inevitable result.

At this point, however, one might very well wonder why SSK represents itself as in conflict or competition with traditional philosophy. Why do we not simply acknowledge the fundamental divergence in aims and methods and leave it at that? Why, in particular, should the enterprise of empirically and naturalistically describing how beliefs become locally credible as a matter of fact compete or stand in conflict with the enterprise of articulating the non-empirical and prescriptive structure in virtue of which beliefs ought to be accepted as a matter of norm? The answer, of course, is that defenders of SSK represent themselves as explicitly rejecting the aims and methods of traditional philosophy—not simply as leaving them out of account. They feel compelled, that is, explicitly to *deny* the philosophical theses underlying the traditional normative enterprise: for example, 'science is *not* a set of universal standards, sustaining true descriptions and valid inferences in different specific cultural contexts'; 'there is *no* sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such'; 'there are *no* context-free or super-cultural norms of rationality'; and so on. Moreover, it is precisely by insisting on such negative philosophical conclusions that defenders of SSK adopt an explicitly philosophical agenda which itself goes beyond the bounds of purely descriptive empirical research. Does the practicing empirical ethnologist really need to be concerned whether there are—or are not—'super-cultural norms of rationality' in the sense envisioned in traditional philosophy? Is the assertion that there are no—absolutely no—such norms really supposed to be an empirical generalization supported solely by currently available empirical evidence?

Lying behind this rejection of traditional philosophical discussions of science is the conviction that such accounts straightforwardly compete with the sociological explanations offered by SSK. If there *were* 'super-cultural' norms of rational argument and evidence, so the argument goes, then scientific theories would be determined one way or another by reality, experience, and reason. There would be no room left, as it were, for sociological, historically contingent explanations of the content of scientific knowledge. Steven Shapin puts the idea this way:

> If scientific representations were simply determined by the nature of reality, then no sociological accounts of the production and evaluation of scientific knowledge could be offered… It would be pointless to argue against the kind of naive realism and positivism which has few, if any, philosophic proponents at present. The underdeterm-

ination of scientific accounts of reality and the 'theory-laden' nature of fact-statements are both quite widely accepted. Nevertheless, the way forward from these basic sensibilities to a full-blown sociology of scientific knowledge is by no means generally recognized. Even so, this is the best way to proceed: the sociology of knowledge is built upon an appreciation of the contingent circumstances affecting the production and evaluation of scientific accounts.[19]

It is striking, then, that Shapin's survey of empirical work in the application of SSK to the history of science begins with explicitly philosophical considerations—just as it is similarly striking, as noted above, that Collins begins his major work with a chapter on 'philosophical skepticism'. In both cases it is thought that we need such skeptical considerations precisely to undercut the competing explanations of scientific knowledge developed by traditional philosophical 'realism' and 'rationalism'. We need, in Bloor's words, 'to stop the intrusion of a non-naturalistic notion of reason into the causal story'.[20]

But this line of thought rests on a misunderstanding. All that is necessary to stop such an 'intrusion' of reason is mere abstinence from normative or prescriptive considerations. We can simply describe the wealth of beliefs, arguments, deliberations, and negotiations that are actually at work in scientific practice, as Bloor says above, 'without regard to whether the beliefs are true or the inferences rational'. In this way, 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. And, in such a descriptive, purely naturalistic enterprise, there is precisely enough room for sociological explanations of why certain scientific beliefs are in fact accepted as the empirical material permits. Whether or not philosophers succeed in fashioning a normative or prescriptive lens through which to view these very same beliefs, arguments, deliberations, and so on, is entirely irrelevant to the prospects for empirical sociology. In this sense, there is simply no possibility of conflict or competition between 'non-naturalistic', philosophical investigations of reason, on the one hand, and descriptive, empirical sociology of scientific knowledge, on the other.[21]

2. The Intellectual Context of Philosophical Relativism

We can deepen our appreciation of this last point, and, at the same time, begin to understand why, nonetheless, there is indeed an intimate relation between recent

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[19]Shapin, 'Sociological Reconstructions', *op. cit.*, note 2, p. 159.

[20]*Knowledge* (second edition), p. 177.

[21]Consider, for example, the *philosophical* thesis of the underdetermination of theory by evidence, nowadays known as the Duhem-Quine thesis—discussed by Shapin in 'Sociological Reconstructions', footnote 9, and often figuring centrally in discussions of SSK. This thesis challenges the view that scientific theory is *rationally* or *justifiably* determined by evidence by invoking the mere *logical* possibility of modifying background or auxiliary hypotheses in the case of a supposed negative crucial experiment. The unavoidable historical contingencies governing how scientists *in fact* move from evidence to theory in actual scientific practice are entirely independent of whether or not this philosophical thesis is correct.

interest in the sociology of scientific knowledge and philosophical relativism, by briefly sketching a chapter in the history of scientific philosophy. This chapter begins with the philosophy of Newtonian science developed in the late eighteenth century by Immanuel Kant—who, we should add, was the first philosopher to make 'scientific objectivity' a fundamental philosophical theme. At the same time, as we shall see, Kant was also the first important thinker of the modern period to insist on a sharp separation between philosophy and empirical natural science.

Kant undertook the task of explaining how the best scientific knowledge of his day—the mathematical physics of Newton's *Principia*—is possible. And, on this basis, he hoped thereby to reformulate the very meaning and function of philosophy or 'metaphysics' as well. He hoped to answer the question 'How is metaphysics as a science possible?' by answering the questions 'How is pure mathematics possible?' and 'How is pure natural science possible?'[22] In this connection, Kant interprets the Newtonian achievement as indicating that the fundamental concepts of space, time, and motion, along with those of material substance, force, and interaction, have no application whatever to 'things-in-themselves' existing wholly independently of the human mind. On the contrary, they are rather expressions of forms, categories, and constructions of our own, on the basis of which alone nature—as the object of truly objective and determinate scientific knowledge—is first possible. Rather than describing an ultimate 'noumenal' reality of 'things-in-themselves', metaphysics, in Kant's hands, has now the task of articulating the conditions of the possibility of experience, that is, the possibility of the truly objective and determinate experience of nature exemplified, paradigmatically, in Newtonian natural science.

The crucial question, however, is what do 'objectivity', 'knowledge', and 'truth' now mean here? For Kant, these concepts emphatically do not refer to a relation of 'mirroring' or correspondence between the human mind on one side and a mind-independent reality on the other. Kant's insistence that we have no knowledge whatsoever of 'things-in-themselves' is explicitly intended to block such a 'realist' interpretation once and for all. For Kant, 'objectivity' is now interpreted rather as necessary intersubjective validity: as necessary validity for all times and for all human knowers. The point is that pure mathematical knowledge is taken, above all, to be exemplary of such necessary intersubjective validity, and what the Newtonian achievements has shown, for Kant, is how such mathematical intersubjective val-

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[22]This strategy is most explicit in the *Prolegomena to Any Future Metaphysics* (1783). The metaphysical tradition Kant is attempting to transform is that of Leibniz and Christian Wolff, wherein natural science is assigned the role of describing spatio-temporal 'phenomena' while metaphysics describes the underlying 'noumenal' reality of ultimate simple substances or monads. For discussion see my *Kant and the Exact Sciences* (Cambridge, MA: Harvard University Press, 1992), and also G. Buchdahl, *Metaphysics and the Philosophy of Science* (Oxford: Blackwell, 1969).

idity can be determinately injected or transplanted, as it were, into our experience of sensibly given nature. In this way, empirical natural science becomes capable of the same kind of intersubjective validity as is pure mathematics, and, for the first time, it thereby becomes truly objective knowledge. And it is in this way, too, that natural scientific knowledge—the kind of knowledge represented by modern empirical natural science—becomes paradigmatic of the successful and objective use of 'universal' human reason.

Now none of this is intended, by Kant, as a causal explanation of how natural science actually evolves and develops as an historically given empirical phenomenon. On the contrary, Kant explicitly distinguishes between the 'transcendental' task of philosophy—the enterprise of articulating the necessary conditions of truly objective scientific knowledge—and the 'empirical' task of the various scientific disciplines. Philosophy, unlike the sciences themselves, is not concerned with empirical objects and events occurring in space and time; it is not concerned with what Kant calls 'phenomena' or 'appearances'. The task of philosophy is rather to articulate the necessary spatio-temporal background structure or framework within which alone phenomena—as objects of empirical knowledge—are first possible. In particular, the 'transcendental', philosophical enterprise has the task of explaining the possibility (as opposed to the *actuality*) of scientific knowledge by depicting the necessary framework of fundamentally mathematical presuppositions within which all future empirical science must necessarily proceed. For all future scientific knowledge, for Kant, must emerge as an extension and further development of the Newtonian paradigm into more and more regions of empirically given nature. This does not mean, however, that we *know* such an extension will in fact occur. Rather, the idea of such continual progress is what Kant calls a *regulative ideal.* It is something that we can neither achieve in fact (in real time, as it were) nor confidently expect in the future. We can only hold it up before us as a goal or norm towards which we strive to make our actual scientific knowledge converge. And it is in this way that the Kantian conception of scientific objectivity is genuinely normative or prescriptive, without any hint at all of confusing the normative with the descriptive.

Nevertheless, despite the purely normative character of this regulative ideal, the actual further development of science created overwhelming problems for Kant's conception of scientific objectivity. For, as we have seen, the necessary framework for all further scientific development, on Kant's view, is modelled on the spatio-temporal structure of Newtonian mechanics. Its mathematical expression, accordingly, is through the laws of Euclidean geometry, on the one hand, and Newton's three laws of motion, on the other. Yet the great revolutions in late nineteenth and early twentieth century mathematics and mathematical physics have shown that precisely these laws must be given up if we are to progress beyond Newton. In particular, the spatio-temporal framework of Einsteinian relativity theory employs both a non-Euclidean geometry and a fundamentally non-Newtonian conception of

how space and time are essentially interrelated through the concept of motion: what we now call relativistic space–time has a fundamentally different underlying mathematical structure from that of Newtonian space–time.[23] And this means that we now know that Kant's conception of scientific objectivity is impossible in principle, even conceived purely as a regulative ideal.

How, then, did scientifically minded philosophers respond to this new situation? It is interesting to note that those philosophers who were most concerned to argue that Kant's particular conception of scientific objectivity is now untenable in the light of Einstein's work did not reject the Kantian project of articulating the a priori or 'transcendental' presuppositions of scientific objectivity *tout court*. On the contrary, these philosophers—who soon became known as the logical positivists or logical empiricists—continued to hold that scientific objectivity is only possible in virtue of an *a priori* mathematical framework that must first be injected or transplanted into sensible nature before any properly empirical science of nature is then possible. Kant's mistake, however, was in thinking that he could specify the precise content of such a mathematical framework entirely independent of, and antecedent to, the actual future development of science—that he could specify the particular structure of what we now call Newtonian space–time as eternally valid once and for all. Relativity theory, in particular, has decisively refuted this idea; for we now use, as we have seen, an entirely different mathematical structure as our underlying framework for mathematical physics. Nevertheless, this new framework, in the new context of Einsteinian relativisitic physics, has the same status, *relative to the new physical situation*, that the old framework had in the context of Newtonian physics. In both cases the underlying spatio-temporal structure represents a form or construction of our own that we must first inject into nature before any properly empirical study of nature is then possible. Yet we now see that such an underlying spatio-temporal mathematical framework is not, as Kant thought, the expression of eternal and universally valid laws of thought. Following the deep mathematical and philosophical study of non-Euclidean geometries undertaken by Henri Poincare, we now see that such *a priori* constructions of our own are better described as *free conventions*.[24]

The logical positivists then busied themselves in attempting to generalize and apply these new ideas so as to develop a new philosophical conception of scientific objectivity. Their first efforts involved a sharp distinction between the pure mathematics and logic implicated in the articulation of any mathematical framework

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[23]For an intellectual history of these developments see R. Torretti, *Relativity and Geometry* (Oxford: Pergamon, 1983).

[24]On these developments see my 'Geometry, Convention, and the Relativized A Priori', in W. Salmon and G. Wolters, eds, *Logic, Language, and the Structure of Scientific Theories* (Pittsburgh: University of Pittsburgh Press, 1994). For a general history of scientific philosophy from Kant through the logical positivists see J. A. Coffa, *The Semantic Tradition from Kant to Carnap* (Cambridge: Cambridge University Press, 1991). For a history of geometry and its philosophy through Poincare's 'conventionalism' see R. Torretti, *Philosophy of Geometry from Riemann to Poincare* (Dordrecht: Reidel, 1978).

for physics whatsoever and the particular applied mathematical representations of nature employed by the various particular such frameworks. Following the first published work by Ludwig Wittgenstein (who, at the time, was closely associated with the logical positivists), his *Tractatus Logico-Philosophicus* (1921), the positivists argued that pure mathematics and logic (which, in accordance with the work of Gottlob Frege, and with Bertrand Russell's and Alfred North Whitehead's *Principia Mathematica*, were thought to be fundamentally identical) in fact possess the necessary and absolutely universal validity Kant had attributed to all scientific knowledge. For pure mathematics and logic express the absolutely necessary structure of any system of linguistic representation whatsoever. Yet the further development of mathematical logic during the 1920s suggested that this idea, too, must be called into question. For it now appeared that a variety of different logico-mathematical calculi, some essentially diverging from the 'classical' mathematical logic of *Principia Mathematica*, were equally possible and legitimate. In particular, no overarching notion of universal logical 'correctness' appeared capable of ruling out the new 'intuitionistic' mathematical logic developed by L. E. J. Brouwer and Arend Heyting on the basis of a rejection of the 'classical' logical law of excluded middle. In this respect, the situation in mathematical logic appeared precisely analogous to that in geometry: in neither case did the idea of a single, uniquely 'correct' system appear to make sense.[25]

So, in 1934, Rudolf Carnap, the leading representative of logical positivism, bit the bullet and took the decisive step of asserting that there is *no* over-arching notion of universal validity or 'correctness' independent of the particular, and diverse, rules of the particular, equally possible and legitimate, formally specifiable calculi. For this reason, the very notions of 'rationality', 'objectivity', and 'correctness' must be *relativized* to the choice of one or another formal language or 'linguistic framework'. The particular logical rules of a given linguistic framework define what counts as 'correct' and therefore rational within that given framework, and it follows that no over-arching notion of rationality or 'correctness' can govern the choice between different such frameworks. This latter choice, as Poincare had first emphasized in the particular case of Euclidean and non-Euclidean geometries, can only be a free stipulation or convention governed by pragmatic—*as opposed to rational*—criteria. The result is Carnap's celebrated 'Principle of Tolerance':

> In logic there is no morality. Anyone may construct his logic—i.e., his form of language—as he wishes. But, if he wishes to discuss [matters] with us, he must clearly specify how he wishes to construct it and give syntactical determinations instead of philosophical considerations.[26]

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[25] All of these developments (including, in particular, the relationship between Wittgenstein and the logical positivists) are discussed in detail in Coffa, *op. cit.*, note 24.

[26] R. Carnap, *Logische Syntax der Sprache* (Wien: Springer, 1934), §17; translated by A. Smeaton as *The Logical Syntax of Language* (London: Kegan Paul, 1937).

In Carnap's later work the same idea is formulated as a distinction between *internal* and *external* questions. Internal questions arise within the context of a particular, already chosen and agreed upon linguistic framework. As such, these questions are rationally and objectively answerable *relative to* the given logical rules of the framework in question—rules which alone can give content to the notions of 'rationality', 'validity', and 'truth' in the first place. External questions, by contrast, concern the choice between different such linguistic frameworks. These questions cannot be rationally or objectively answerable, because the over-arching rules that could define and characterize these notions are, in this case, necessarily missing. External questions are therefore entirely a matter of conventional choice based, in the end, on purely pragmatic criteria of suitability or adaptedness for one or another given purpose.[27]

At this point, one might very well experience a strong sense of *déja vu*. For this Carnapian distinction between internal and external questions is closely analogous to the central Kuhnian distinction between normal and revolutionary science. Thus, the 'puzzle-solving' activities of normal science proceed against the background of a generally accepted and agreed upon paradigm that defines, relatively unproblematically, what could count as either a 'correct' or an 'incorrect' solution. In revolutionary situations, by contrast, the very background framework which alone can define such 'correctness' is itself at issue. And this is why revolutionary science, for Kuhn, poses a particularly acute challenge to what he takes to be the traditional philosophical conception of the rationality of science. For it is precisely in this case—where we are faced with the choice between competing scientific paradigms—that over-arching standards of rationality and validity are entirely missing. Hence, the traditional ideal of scientific rationality must here evidently give way to non-rational factors in explaining the emerging new consensus. It is doubly ironic, then, that this Kuhnian collapse of the traditional notion of universal standards of rationality was already clearly prefigured in the development of the philosophical tradition itself, as this tradition evolved from the 'transcendental' universalism of Kant to the explicitly relativized notion of rationality characteristic of Rudolf Carnap's work.[28]

And it is at this point, too, that we can discern the true relationship between the explicitly philosophical agenda of SSK, on the one hand, and the philosophical tradition it is intended to replace, on the other. It is not that the philosophical tradition sets up a competing model for causally explaining the actual historical

[27]See R. Carnap, 'Empiricism, Semantics, and Ontology', *Revue Internationale de Philosophie* **4** (1950), 20–40; reprinted in his *Meaning and Necessity* (second edition; Chicago: University of Chicago Press, 1956).

[28]*The Structure of Scientific Revolutions* was first published in the *International Encyclopedia of Unified Science* edited by Carnap and Charles Morris. Carnap wrote to Kuhn in his editorial capacity and comments enthusiastically about Kuhn's work in two letters of 12 April 1960 and 28 April 1962. (These two letters are reproduced, and discussed, in G. Reisch, 'Did Kuhn Kill Logical Empiricism?', *Philosophy of Science* **58** (1991), 264–277.) Kuhn, towards the end of his career, regretted the fact that he had interpreted Carnap's letters as expressions of 'mere politeness' and acknowledged the point that his philosophical conception is akin to 'Kant's a priori when the latter is taken in [a] relativized sense'— see T. Kuhn, 'Afterwords', in P. Horwich, ed., *World Changes: Thomas Kuhn and the Nature of Science* (Cambridge, MA: MIT Press, 1993), pp. 313, 331.

evolution of science. On the contrary, the philosophical tradition is concerned throughout with purely normative *ideals* of scientific rationality that are intended to capture the underlying structure of what is taken to be exemplary in contemporary science, so as to hold this up as a model for objectivity and rationality as such. Yet, as we have seen, as contemporary science develops and changes, philosophers have been forced to change fundamentally the resulting ideals of scientific rationality as well. And it is in *this* way that the philosophical tradition is in fact essentially intertwined with the actual historical evolution of science. Finally, as we have also seen, this same process of mutual scientific and philosophical evolution has, in the end, resulted in a thoroughly relativized and conventionalistic conception of scientific rationality—a conception that in turn prepares the way for the philosophical agenda of SSK. All we need do is turn our attention—as Carnap himself does not—to the social factors actually responsible for the consensual acceptance of the conventions underlying one or another Carnapian linguistic framework or Kuhnian paradigm.[29] Carnap plus Kuhn equals the philosophical agenda of SSK.

3. SSK and Wittgenstein

We already know, however, that the practitioners of SSK do not as a matter of fact appeal to Carnap in articulating and defending their philosophical agenda. Their main philosophical hero is rather Ludwig Wittgenstein, and, in particular, the Wittgenstein of the so-called later philosophy—represented, above all, by the *Philosophical Investigations* (on which Wittgenstein worked intensively throughout the 1930s and 40s and which was finally published posthumously in 1953). And this appeal to the later philosophy of Wittgenstein is, at least at first sight, perfectly intelligible, since this philosophy undoubtedly takes as its basis and starting point the fundamentally social and practical character of all human thinking and knowing. In sharp contrast to both his own earlier philosophy as expressed in the *Tractatus* and Carnap's *Logical Syntax of Language*—both of which discern the philosophically interesting 'essence' of language in its purely formal, purely syntactic properties—Wittgenstein here introduces the concept of 'language-*game*' precisely 'to emphasize that the *speaking* of language is part of an activity, or of a form of life' (PI, §23).[30] Language as such, and therefore all human thinking and knowing, only

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[29] A version of this understanding of the sociological relevance of Kuhn's work is completely explicit in Barnes, *T. S. Kuhn*. Carnap himself was by no means unsympathetic to investigation of such social factors. On the contrary, in unpublished notes in connection with his correspondence with Kuhn (note 28 above) Carnap compares the choice of scientific framework to a Darwinian process of selection 'on the basis of preference in the community of scientists, whereby all kinds of sociological, cultural, etc., factors are involved'. (This note is reproduced in J. Earman, 'Carnap, Kuhn, and the Philosophy of Scientific Methodology', in Horwich, ed., *op. cit.*, note 28.)

[30] I shall cite Wittgenstein's works as follows: (T) *Tractatus Logico-Philosophicus*, trans. C. K. Ogden (London: Routledge, 1922); (BB) *The Blue and Brown Books* (Oxford: Blackwell, 1958); (PI) *Philosophical Investigations*, ed. G. E. M. Anscombe (Oxford: Blackwell, 1953); (RFM) *Remarks on the Foundations of Mathematics*, ed. G. H. von Wright, R. Rhees, and G. E. M. Anscombe (Oxford: Blackwell, 1956); (OC) *On Certainty*, ed. G. E. M. Anscombe and G. H. von Wright (Oxford: Blackwell, 1969).

makes sense in the context of essentially social and practical 'streams of life'. Indeed, Kuhn's decisive emphasis on the importance of scientific communities is itself explicitly modelled on these later Wittgensteinian ideas.[31]

On second sight, however, there are also very significant philosophical disparities and divergences between Wittgenstein and the philosophical agenda of SSK. In the first place, as we have seen, SSK is itself intended to be an empirical scientific discipline. It is intended to describe and explain the naturally occurring phenomenon of human scientific knowledge in the same way, and by the same methods, that science describes and explains any other natural phenomenon. But Wittgenstein himself is adamant, both early and late, that what he calls philosophy is entirely distinct from natural science. This theme begins in the *Tractatus*:

> Philosophy is not one of the natural sciences.
> (The word 'philosophy' must mean something that stands above or below the natural sciences, not beside them.) (T, 4.111)

The theme is continued, even more insistently, in the 'Blue Book' (unpublished notes dictated in 1933–34 which are considered to initiate the period of the *Philosophical Investigations*):

> Our craving for generality has another main source: our preoccupation with the method of science. I mean the method of reducing the explanation of natural phenomena to the smallest possible number of primitive natural laws; and, in mathematics, of unifying the treatment of different topics by using a generalization. Philosophers constantly see the method of science before their eyes, and are irresistibly tempted to ask and answer questions in the way science does. This tendency is the real source of metaphysics, and leads the philosopher into complete darkness. I want to say here that it can never be our job to reduce anything to anything, or to explain anything. Philosophy really *is* 'purely descriptive'. (BB, p. 18)

And, in the *Philosophical Investigations* itself, when looking back critically at the earlier project of the *Tractatus*, the very same point stands out perfectly explicitly:

> It was true that our considerations could not be scientific ones… And we may not set up any kind of theory. There must not be anything hypothetical in our considerations. We must do away with all *explanation*, and description alone must take its place. And this description receives its light, i.e., its purpose, from the philosophical problems. These are certainly not empirical problems. They are solved, rather, through an insight into the working of our language—and in such a way, in fact, that this is recognized: *contrary* to an urge to misunderstand it. The problems are solved, not by bringing to bear new experience, but by arranging what has long been known. Philosophy is the battle against the bewitchment of our understanding by means of our language. (PI, §109)

Wittgenstein's interest in carefully and attentively describing the workings of vari-

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[31]See Kuhn, *Structure, op. cit.*, note 9, pp. 44–46. Kuhn's assimilation of Wittgenstein was probably mediated by his association with the philosopher Stanley Cavell, who is cited in the Preface.

ous particular language-games is in no way preparatory for the kind of general socio-cultural 'systematic theory of language-games' envisioned by Bloor.[32]

By the same token, Wittgenstein shows no interest in articulating generalizations about human thought and language—either such straightforward empirical generalizations as might be found in actual ethnological practice or in the kind of bold and exciting philosophical generalities about human knowledge considered earlier ('there are *no* context-free or super-cultural norms of rationality', and so on). On the contrary, as stressed above, philosophy stays always on the level of particulars so as solely to reveal perspicuously what we all already know:

> Philosophy merely presents everything, and neither explains nor infers anything.— Since everything lies open, there is also nothing to explain. For what might be hidden does not interest us.
> One could also call 'philosophy' what is possible *before* all new discoveries and inventions.
>
> The work of philosophy is an assembling of reminders for a definite purpose.
>
> If one wanted to set up *theses* in philosophy there could never be any discussion about them, because everyone would agree with them. (PI, §§126–128)

Thus, whatever may be the 'definite purpose' of Wittgenstein's 'arranging' (§109), 'assembling' (§127), and 'ordering' (§92) of various particular 'language-games' (§23) so as to produce a 'surveyable presentation' of 'the use of our words' (§122), this purpose is certainly not to marshal evidential support, by instances, as it were, for any kind of generalized picture of language—whether empirical or philosophical.

In the second place, the particular language-games that Wittgenstein actually considers are, as often as not, *imaginary* uses of language rather than real ones. From the famous example of the primitive builders that opens the *Investigations* (§2), to the 'pupil' who 'understands' simple arithmetical examples differently than we do (§§144 ff.), or the strange 'tribe' or 'people [*Volk*]' whose forms of life are essentially different from our own (e.g., §§200, 206–207), Wittgenstein is constantly presenting us with entirely imaginary examples which are 'invented' (§122) as '*objects of comparison* that are supposed to cast light on the relations of our language through similarities and dissimilarities' (§130). And it is for precisely this reason that, although Wittgenstein's purely descriptive enterprise is certainly intended to point to the very general facts of human (and non-human) nature in which our language-games are necessarily embedded, this enterprise is neither natural science nor natural history:

> If the formation of concepts can be explained on the basis of facts of nature, should we not be interested, instead of in grammar, in that in nature which lies at its basis?— We are certainly interested also in the correspondence of concepts with very general facts of nature. (Those that mostly do not strike us because of their generality.) But our interest does not extend back to the possible causes of the formation of concepts.

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[32]See *Wittgenstein*, Chapter 7.

> We are not engaged in natural science, and not even in natural history—since we can also surely provide fictitious natural history for our purpose. (PI, II, xii)

The importance and prominence of entirely imaginary language-games in Wittgenstein's work very clearly brings out its fundamentally non-empirical character.

A third, closely related point is that 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. He shows very little interest, that is, in either the kind of socio-cognitive changes studied in the history of science or in comparative cultural ethnology. Wittgenstein is interested, to be sure, in alternatives to 'our' ordinary practices; and he is interested, in particular, in showing that there is therefore no absolute necessity somehow inherent in these practices.[33] Yet the deviation from 'our' ordinary practices manifest in such Wittgensteinian alternatives—which are typically, as emphasized above, purely imaginary alternatives—is much more radical than in the alternative socio-cultural practices studied in the history of science and ethnology. An imaginary 'tribe' of people who consistently 'misunderstand' the most elementary possible arithmetical rules, for example, is clearly quite another kettle of fish from the alternative socio-cognitive communities encountered in actual history and ethnology. Indeed, it is typical of Wittgenstein's alternative 'tribes', unlike the alien communities we study in actual history and ethnology, that, try as we might, we literally *cannot*, in the end, understand them.[34]

Finally, there is no trace of socio-cultural relativism in Wittgenstein. Although the 'we' that constitutes Wittgenstein's central object of concern is normally left entirely unspecified, it is also quite frequently used as synonymous with 'humanity' or the totality of 'human beings'.[35] The contingent 'agreement in form of life' (§241) lying at the basis of human thought and knowledge appears to have more to do with 'the common human way of acting' (§206) than with the socio-cultural conventions of one or another particular socio-cultural group.[36] And it appears to be for precisely this reason, in fact, that Wittgenstein focusses on purely imaginary alternatives to 'our' customary forms of life—alternatives which are so extreme that 'we' human beings cannot, in the end, understand them. Accordingly, when Wittgenstein speaks of 'language-games' and 'forms of life' in the plural, he is typically calling attention to the manifold uses of language and linguistic activities found within 'our' single cultural-linguistic community. Indeed, Wittgenstein is doing precisely this in the very passage where he first introduces these expressions:

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[33]See the continuation of PI, II, xii: 'If someone believes that certain concepts are absolutely the correct ones, that whoever might have different ones would simply not comprehend what we do, then let him imagine certain very general natural facts to be different from what we are used to, and the formation of concepts different from the usual ones will become understandable to him.'

[34]See, e.g., PI, §207, and II, xi, p. 223. This point is emphasized in an insightful discussion by Barry Stroud, 'Wittgenstein on Logical Necessity', reprinted in G. Pitcher, ed., *Wittgenstein* (New York: Doubleday, 1966).

[35]See, e.g., PI, §§204, 206, 241, 242.

[36]Compare OC, §156: 'In order to make a mistake, a human being must already judge in conformity with humanity.'

> But how many kinds of sentences are there? Perhaps assertion, question, and command?—There are *innumerable* such kinds: innumerable different kinds of uses of everything that we call 'signs', 'words', 'sentences'. And this multiplicity is nothing fixed, given once and for all; rather, new types of language—new language-games, as we might say—come into being and others become obsolete and are forgotten. (The changes in mathematics can give us an *approximate picture* of this.)
>
> The word 'language-*game*' is here intended to emphasize that the speaking of language is part of an activity, or of a form of life.
>
> Review the multiplicity of language-games in these examples and others:
>
> Giving orders and acting in accordance with them—
>
> Describing an object by its appearance, or on the basis of measurements—
>
> Constructing an object in accordance with a description (a drawing)—
>
> Reporting an event—
>
> Speculating about the event—
>
> Setting up and testing an hypothesis—
>
> Presenting the results of an experiment by means of tables and diagrams—
>
> Inventing a story; and reading it—
>
> Play-acting—
>
> Singing rounds—
>
> Guessing riddles—
>
> Making a joke; telling it—
>
> Solving an applied calculation—
>
> Translating from one language into another—
>
> Requesting, thanking, cursing, greeting, praying—
>
> —It is interesting to compare the multiplicity of linguistic tools and their uses, the multiplicity of kinds of words and sentences, with what logicians have had to say about the construction of language. (Including the author of the *Tractatus Logico-Philosophicus*.) (PI, §23)

Here Wittgenstein is in no way considering conflicting or competing socio-cognitive communities as appear in the history of science or alternative socio-cultural communities as studied in comparative ethnology. Wittgensteinian 'forms of life' are, in this respect, something entirely distinct from the alternative 'forms of life' considered in SSK.

I do not mean to suggest, however, that practitioners and defenders of SSK are simply ignorant of these fundamental divergences between their agenda, on the one hand, and Wittgenstein's actual philosophical conception, on the other. On the contrary, Bloor himself forthrightly calls attention to the sharp contrast between Wittgenstein's explicitly anti-scientific and anti-theoretical characterization of his enterprise and Bloor's own frankly empirical and theoretical ambitions at the very beginning of his discussion. Yet, in his eagerness to get on with the business of developing an empirically based 'systematic theory of language-games', Bloor merely brushes this divergence aside: what he wants to do is 'replace a fictitious natural history by a real natural history, and an imaginary ethnography by a real ethnography'.[37] My point, in emphasizing the significance of these divergences

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[37]*Wittgenstein*, p. 5. In Chapter 8 Bloor seeks to account for Wittgenstein's 'unfortunate' anti-scientific and anti-theoretical attitude as a product of the 'pessimistic' *Lebensphilosophie* popular in the 1920s. I here attempt to show, on the contrary, that Wittgenstein's anti-scientific and anti-theoretical

nonetheless, is not at all to dispute the possibility or importance of the kind of empirically based ethnology of science advocated and articulated in SSK. Nor does it follow, of course, if practitioners of SSK in fact diverge significantly from Wittgenstein in their philosophizing, that Wittgenstein is 'right' and they are 'wrong'. Indeed, it is not at all my purpose here to argue that practitioners of SSK are (philosophically) 'wrong'. I wish rather to argue that, if one insists on embedding the empirical enterprise of SSK within an explicitly philosophical agenda, then one might very well profit from attending more carefully and systematically to the particular philosophical conception of that historically given philosopher one takes as one's philosophical model.[38]

4. 'The Hardness of the Logical *Must*'

In attempting to understand the philosophical conception underlying Wittgenstein's later philosophy, it is very helpful to begin with his earlier conception expressed in the *Tractatus Logico-Philosophicus*. Indeed, Wittgenstein himself says as much in the Preface he prepared for the *Philosophical Investigations*: '[My new thoughts] can be seen in the proper light only in contrast with and against the background of my old way of thinking' (PI, p. x). And the *Tractatus*, as indicated above, attempts to present the philosophical foundations of the new mathematical logic recently articulated by Gottlob Frege and Bertrand Russell—which logic, according to the *Tractatus*, depicts the universal and absolutely necessary framework within which alone any linguistic representation whatsoever is possible.[39] It is in virtue of this logical framework, and this framework alone, that it is possible at all to represent the world:

> Logical propositions describe the framework of the world, or rather they present it. They do not 'treat' of anything. They presuppose that names have meaning and elementary propositions have sense; and this is their connection with the world. It is clear that it must indicate something about the world that certain connections of symbols—which essentially have a determinate character—are tautologies. Herein lies the decisive point. We said that some things are optional in the symbols we use and some things are not. In logic only the latter is expressed. This means, however, that in logic *we* do not express what we wish with the help of signs; rather, in logic the nature of the absolutely necessary sign itself gives testimony: if we know the logical syntax of any sign language whatsoever, then all the propositions of logic are already given. (T, 6.124)

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attitude towards philosophy, in particular, is an integral part of his philosophical conception that cannot be so easily brushed aside.

[38]In §5 below I attempt to illustrate how the recent theoretical debates surrounding SSK have become bogged down in tortuous metaphysics precisely by paying insufficient attention to Wittgenstein's own philosophical conception.

[39]As Peter Winch has emphasized to me, in particular, this is not to say that Wittgenstein does not have serious disagreements with Frege and Russell about the nature and form of this new logic. On the contrary, he emphatically rejects their attempts to incorporate something like what we now call set-theory into the foundations of mathematics and logic: see especially T, 6.031.

And it is in this way, too, that logic obtains a uniquely privileged status: 'Logic is not a doctrine, but a mirror-image of the world; logic is transcendental.'(6.13) Thus, Wittgenstein's conception of logic in the *Tractatus* is a clear descendant of the Kantian conception of universal reason—a reason that yields the absolutely necessary presuppositions or conditions of possibility for any thinking about reality whatsoever.

There is one important respect, however, in which the conception of the *Tractatus* dramatically diverges from Kant. Central to the *Tractatus* is a sharp distinction between what can be *said* [*gesagt*] on the one side and what cannot be said but rather can only be *shown* [*gezeigt*] on the other. And this means, in particular, that the philosophical conception of logic just reviewed cannot, strictly speaking, itself be formulated or articulated. This philosophical conception can itself only be *shown*—it is shown in and through the logical relationships among propositions:

> Propositions cannot present logical form; this mirrors itself in them.
> That which mirrors itself in language, language cannot present.
> That which expresses *itself* in language, *we* cannot express by means of language.
> Propositions *show* the logical form of reality.
> They exhibit it. (4.121)
>
> What *can* be shown *cannot* be said. (4.1212)

This is why, at the end of the *Tractatus*, Wittgenstein paradoxically characterizes his own philosophical reflections as 'non-sensical', as a 'ladder' that one must 'throw away after one has climbed it' (6.54). Unlike Kant, then, Wittgenstein can find no place at all for a 'transcendental' philosophical *theory* of universal reason. The reality, as it were, of this universal reason can only be shown—shown in the properly understood logical workings of our language.

In this respect, moreover, there is also a sharp divergence between Wittgenstein's conception of logic, in the *Tractatus*, and the related view later articulated by Carnap—which view, as Carnap himself insists, is explicitly inspired by the *Tractatus*. Both Wittgenstein and Carnap hold that logic is based solely on the purely formal or syntactic combinatorial possibilities of symbolic language—and is therefore, in particular, entirely independent of the meanings of the signs in question (compare T, 3.33). It is for this reason, for both Wittgenstein and Carnap, that logic is thereby entirely independent, in addition, of all particular empirical facts holding in the actual world. For Carnap, however, there is a theory or discipline whose subject matter is the logical syntax of language, and this theory is precisely the new discipline of *metamathematics* recently developed by David Hilbert. Here, in the *meta-language*, we can indeed explicitly describe the formal logical syntax of the *object-language*; and, on this basis, Carnap himself rejects Wittgenstein's conception of the inexpressibility of logical syntax.[40] For Wittgenstein, by contrast, there is no point of view outside of logic and language—outside, that is, of the single, absolutely universal logical language—from which one could possibly

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[40]See, e.g., Logical Syntax, *op. cit.*, note 26, §8. For discussion see my 'Carnap and Wittgenstein's *Tractatus*', in W. Tait, ed., *Early Analytic Philosophy* (La Salle, IL: Open Court, 1997).

describe or articulate what he calls logical syntax (4.12). Hilbertian meta-languages, from Wittgenstein's own point of view, are therefore entirely irrelevant.[41]

There is a close connection, then, between Carnap's rejection of Wittgenstein's conception of the inexpressibility of logical syntax, on the one hand, and Carnap's logico-linguistic pluralism, on the other. It is precisely because there are many, logically distinct formal languages that Carnap has room for the distinction between object-language and meta-language—and hence room for an explicit formal theory of logical syntax. During the late 1920s and early 1930s, the period when Wittgenstein was most closely associated with Carnap and the other logical positivists, Wittgenstein, too, came to reject his earlier commitment to the uniquely privileged status of the 'classical' mathematical logic of Frege and Russell. Indeed, both Wittgenstein and Carnap were greatly impressed by the development of 'intuitionism' by the mathematician L. E. J. Brouwer, which showed, in particular, that alternatives to the 'classical' logical framework are in fact quite possible. Moreover, during this same period Wittgenstein developed a conception of 'grammatical rules' that appeared to be quite similar in many ways to Carnap's conception of the 'logical rules' definitive of a given linguistic framework.[42] Nevertheless, Wittgenstein did not, in the end, go the route of Carnap's *Logical Syntax of Language*. In particular, he never adopted the conception of the arbitrariness of logical grammar or logical syntax expressed in the Principle of Tolerance, and he never followed Carnap in embracing the possibility of an explicit and articulated philosophical theory of language. Wittgenstein instead pursued the entirely different path that eventually issued in the *Philosophical Investigations*.

In the *Philosophical Investigations*, as we know, Wittgenstein abandons the logical absolutism of the *Tractatus* in favor of a kind of linguistic pluralism—a pluralism embracing an indefinite variety of language-games and forms of life. Yet this is not, as in Carnap, a pluralism of alternative logico-mathematical frameworks, each articulating its own particular version of logico-mathematical validity. Wittgenstein's linguistic pluralism rather involves the even more philosophically radical idea that logico-mathematical thinking as such is in no way foundational for the workings of language as a whole. In particular, one should no longer take the fact-stating, assertive or descriptive use of language—the use for which the concepts of 'true' and 'false' together with the logical principles that essentially govern these concepts encapsulate the rules of the language-game—as *the* fundamental use of language to which all other uses must somehow be reduced. On the contrary, language in the end rests simply on the indefinite multiplicity of human forms of life, and the fact-stating, assertive or descriptive use of language is merely one part of this immense multiplicity (PI, §23). Nevertheless, Wittgenstein does not in any way lose interest in the particular case of logico-mathematical thinking. On the contrary, this particular human linguistic phenomenon continues to play a

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[41]The importance of this rejection of the project of metamathematics for Wittgenstein's later philosophy as well is stressed in Paul Feyerabend, 'Wittgenstein's *Philosophical Investigations*', reprinted in G. Pitcher, ed., *op. cit.*, note 34.

[42]The overlap between Carnap's views and Wittgenstein's at this time was in fact so extensive that Wittgenstein accused Carnap of plagiarism. See Coffa, *op. cit.*, Chapters 13–14, for a discussion of this episode by a Carnapian partisan.

very central role indeed in his own philosophical reflections in the *Philosophical Investigations*—and, even more, in the unpublished notes he originally intended to integrate into the *Investigations* that are now collected together as the *Remarks on the Foundations of Mathematics*. And Wittgenstein continues to be obsessed, just as he was in the *Tractatus*, with the question of the ultimate source or ground of logico-mathematical necessity: with the question of '[t]he hardness of the logical *must*' (RFM, I, §121).

In sharp contrast to the *Tractatus*, however, it is clear that logico-mathematical necessity, in the period of the *Investigations* and the *Remarks*, now has no ultimate source or ground—no philosophical foundation or explanation of the kind the *Tractatus* had attempted to depict. The real problem, in philosophy, is now precisely to *resist* the characteristically philosophical demand for such an ultimate justification:

> The danger here, I believe, is to give a justification of our procedure, where there is no justification and we should simply say: *that's how we do it*. (RFM, II, §74)

> [I]it is a peculiar procedure: that I go through the proof and then accept its result.— I mean: that is simply how we *do* it. That is customary among us, or a fact of our natural history. (I, §63)[43]

In particular, we should reject the attempt of the *Tractatus* to trace the source of logical necessity to 'the nature of the absolutely necessary sign' which, entirely independently of what we wish to do with it, 'itself gives testimony' (T, 6.124). We should reject the whole idea of such an 'absolutely necessary sign'—which is made, as it were, of 'the purest crystal' (PI, §97) and which, 'hidden in the medium of the understanding', (PI, §102) determines in advance how it is to be understood. No sign—not even the very simplest combinatorial sign configuration used in elementary logic and arithmetic—can have this latter property. For all signs, in the end, only unfold their meanings in and through their concrete applications at the hands of actual human beings. In the end, it must after all be *we* who determine the meanings of logico-mathematical rules by the way in which we all agree in how to go on in continually *applying* these rules.[44]

Does this mean that Wittgenstein now intends to advocate a naturalistic, empirically oriented theory of logico-mathematical necessity that locates the source of this necessity in human agreement and convention—a sociological theory of logico-mathematical necessity of the kind explicitly championed by David Bloor? Does Wittgenstein, that is, intend a naturalistic and reductionist account of this phenomenon according to which all there is to logico-mathematical necessity is socio-cultural convention? Nothing, I think, could be further from the truth.[45] For, not

[43]Compare PI, §217, in the context of the celebrated 'rule-following' argument.

[44]Compare RFM, I, §118: 'it is *we* who are inexorable in applying these laws'.

[45]Shapin and Schaffer, as the motto for Chapter IV of *Leviathan*, *op. cit*., note 2 (p. 110), cite a remark from RFM, I, §116, where Wittgenstein says: 'Yet one can nevertheless say that the laws of inference compel us; in the sense, namely, that other laws in human society do so.' The implication, it seems, is that here Wittgenstein is advocating a general theory of logico-mathematical necessity that equates it with societal laws and conventions (an implication which is fully explicit in Bloor, *Wittgenstein*, pp. 121–122). This kind of reading perfectly illustrates the dangers of overly generalizing approaches to Wittgenstein. For the remark in question is tied to a very specific example—the example of an official clerk (apparently a military clerk) who follows instructions to assign people in accordance with height to various 'divisions' (I, §17). This clerk, in the words of I, §116, 'would be punished if

only does Wittgenstein explicitly reject *all* attempts to provide theoretical accounts or explanations for the phenomena he considers—whether these explanations be philosophical or empirical—but he also insists that his investigation is a purely 'grammatical' one (PI, §90), which, just as in the *Tractatus*, is concerned precisely with the 'essence' of our language:

> [I]f we, too, are striving in our investigations to understand the essence of language—its function, its construction—it is not *this* that these questions [of the *Tractatus*] have in view. For they see in the essence, not something that already lies open and becomes *surveyable* [*ubersichtlich*] by ordering, but rather something that lies *under* the surface—something that lies within, that we see if we penetrate [*durchschauen*] the thing, and which an analysis is supposed to dig out. (PI, §92)

Since, in Wittgenstein's new conception, '*essence* is expressed in grammar' (§371) and 'grammar says what kind of object something is' (§373), we see that the chief difference, in this connection, between the new conception and the old can be expressed as follows: whereas, on the old conception, the essential—and therefore non-empirical—features of language are uncovered when we *penetrate* [*durchschauen*] to the 'crystalline purity' of logical form underlying our ordinary uses of language via analysis, these same features, on the new conception, are rather laid open to view when we *survey* [*ubersehen*] a number of rightly chosen particular examples of such ordinary uses—whether real or imaginary—to reveal their 'grammar' (compare §122). In this way: 'The *prejudice* of crystal purity can only be removed by rotating our entire examination. (One could say: the examination must be rotated, but around the fixed axis of our real need.)' (§108, and see the immediately following §109).

When we perform this 'rotation', moreover, we see that logico-mathematical thinking has by no means lost its central, and essentially non-empirical position in our thinking and acting in general. For, as a matter of fact, logico-mathematical thinking, characterized precisely by the peculiar 'hardness of the logical *must*', plays a central and very particular role—both theoretical and practical—in our actual human life:

> [The logico-mathematical *must*] is an expression of an attitude towards the technique of calculating that shows itself everywhere in our life. The emphasis of the *must* corresponds only to the inexorableness of this attitude—both towards the technique of calculating and towards innumerable similar techniques.
> The mathematical *must* is only another expression of the fact that mathematics forms concepts.

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he inferred differently'. But consider an only slightly more complicated example: a clerk who must give correct change for purchases. Such a clerk is certainly legally liable for making false change—but only if he is knowingly and intentionally cheating. In such a case the clerk is emphatically not being punished for making mistakes in arithmetic—indeed, if *this* is the problem he is *not* legally liable. Here the connection between 'laws of inference' and 'laws in human society' is already considerably more complex and indirect than in the first example.

> And concepts serve for conceptualizing. They correspond to a definite way of dealing with situations.
>
> Mathematics forms a network of norms. (RFM, V, §46)[46]

Thus, although logico-mathematical thinking simply rests, in the end, on the very general facts of human (and non-human) nature in virtue of which we all agree in thus going on, in spite of this fact—or better, precisely because of it—such thinking is in no way arbitrary or optional: 'thinking and inferring (like counting) is of course not bounded for us by an optional definition, but rather by the natural limits corresponding to the body of that which we can call the role of thinking and inferring in our life' (RFM, I, §116). As a matter of grammar, *not* a matter of experience, then: 'The propositions of logic are "laws of thought", because they express the essence of human thinking—or, more correctly: because they express, or show, the essence, the technique, of thinking. They show what thinking is, and also types of thinking.' (RFM, I, §133, and compare §131).

We have now reached the heart of the matter, I believe, for it is at precisely this point that three fundamental themes of Wittgenstein's later philosophizing become clear. First, there continue to be non-empirical, essentially normative elements in our thinking; and logic and mathematics continue to be paradigmatic of such elements. Second, such normative features of linguistic practice are the particular province of a peculiarly philosophical investigation—an investigation that is itself non-empirical or 'grammatical'.[47] And third, this peculiarly philosophical investigation nonetheless issues in no special body of doctrine or theory. On the contrary, it can only proceed by a piecemeal examination of various particular language games that 'shows' or 'expresses' the grammatical features in question. All of these themes—with the exception, of course, of the emphasis on piecemeal examination of various particular language-games—are, as we have seen, also central to the *Tractatus*. The fundamental difference, as we have also seen, is that the *Tractatus* points to a philosophical explanation of the normativity of logic in terms of the absolute necessity and 'crystalline purity' of underlying logical form, whereas the *Investigations* points rather to the ineluctable connection of such normativity to the ultimately contingent and 'mundane' facts of practical social life.

Yet it is of crucial importance in assessing the philosophical agenda of SSK, I believe, that the *Investigations* only gestures towards these 'mundane' facts of practical social life. At precisely this point, Wittgenstein himself self-consciously and deliberately steps back from embracing any kind of naturalistic theory claiming to 'explain' the normativity in question. And he will have absolutely nothing to do, in particular, with any kind of reductionist account according to which the

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[46]For the essentially non-empirical—'grammatical'—character of 'concept formation' in general and mathematics in particular see, e.g., RFM, I, §§128, 155; II, §§71–75; III, §29; V, §14–18. And compare PI, II, xi (pp. 224–226), xii. In the passages from PI, II, xi, Wittgenstein emphasizes the constitutive importance of the (by him unexplained) *fact* that there is general or complete agreement [*volle Ubereinstimmung*] in mathematics. In is in this context, especially, that one should understand the famous remarks about the relationship between 'human agreement' and truth at PI, §§240–242.

[47]The importance of Wittgenstein's non-empirical notion of 'grammar' is emphasized by Stanley Cavell, 'The Availability of Wittgenstein's Later Philosophy', in G. Pitcher, ed., *op. cit*. In particular, Cavell compares Wittgenstein's distinction between 'grammatical' and 'empirical' propositions with Kant's 'transcendental'/'empirical' distinction.

normativity of logico-mathematical thinking is *nothing but* a matter of social convention: as emphasized above, there is no trace of socio-cultural relativism in Wittgenstein's own philosophizing. In sharp contrast to all such tendencies, Wittgenstein himself is rather centrally concerned with what he terms the 'dignity' of logic and mathematics:

> What relation has [the mathematical proposition] to these empirical propositions [describing how calculations in fact turn out]? The mathematical proposition has the dignity of a rule.
> *This* much is true in the thesis that mathematics is logic: it moves within the rules of our language. And this gives it its special solidity, its separate and unassailable position. (RFM, I, §164)

Moreover, Wittgenstein explicitly considers the relationship between the 'dignity' of logico-mathematical rules and a possible ethnological description of them:

> But can one not then replace the rule by an empirical proposition that says that measuring instruments are made in such and such a way, that people make use of them in *this* way? One might give an ethnological portrayal of this human institution. Now it is obvious that this ethnological portrayal could take over the function of the rule.
> …
> Whoever knows a mathematical proposition is not yet supposed to know *anything*. I.e., the mathematical proposition is only supposed to supply the framework for a description. (RFM, V, §2)
>
> A rule *qua* rule is detached, it stands there, so to speak, tyrannically—even though what gives it importance are the facts of everyday experience.
> What I have to do is something like this: describe the office of a king—whereby I may not make the mistake of explaining the kingly dignity by the king's usefulness; and yet I may leave neither usefulness nor dignity out of account. (V, §3)

Even though the ethnological portrayal 'could take over the function of the rule', it is clear that the point of these passages is precisely to distinguish the two. An ethnological description functions *as* a rule, for example, when we correct the linguistic usage of a foreigner by the phrase, 'we don't say that', which here means 'you *shouldn't* say that'.[48] Used in this way, however, the ethnological portrayal is precisely not a naturalistic, purely empirical description of our practice; for the latter would necessarily leave out the 'dignity' of the rules governing our practice— their actual normative power.[49]

We can now appreciate the full force of Wittgenstein's strategy of attempting to describe 'the essence of language—its function, its construction' from *within* the very norms and practices he is describing. We can now see, in particular, why Wittgenstein attempts to 'show' or 'exhibit' the essential features of our sociolinguistic practice by adducing particular examples of ordinary usage rather than

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[48]Compare RFM, II, §65, for the example of a judge attempting to use a 'handbook of anthropology' as a statute book.

[49]The German word *Würde*, unlike the English 'dignity', has the connotation of 'worthiness [*würdigkeit*]'. In this sense, a rule that has *Würde* is 'worthy' of being followed.

engaging in ethnological theorizing. For the point of view of the ethnological theorist is explicitly *outside* the cultural system being considered. The ethnological theorist, unlike the members of the cultural system themselves, is in no way bound by or committed to the norms of the system in question—for the ethnologist, these norms certainly do not have 'the dignity of a rule'. Indeed, precisely in so far as the ethnologist explicitly conceives the cultural system in question as simply one possible system among many diverging alternatives, the normativity of the system, from this point of view, must inevitably be called into question: its natural and 'internal'—taken-for-granted—character is thereby necessarily dissolved. But Wittgenstein, as we have seen, does not conceive his own investigation as belonging to comparative ethnology in this way. On the contrary, Wittgenstein takes himself to be describing features of our socio-linguistic practice that lie so deep and are so pervasive, as it were, that there is, as a matter of fact, no point of view outside them—no such point of view that we human beings can, as a matter of fact, understand. Wittgenstein then attempts to depict the true character of these most fundamental features of our practice—features through which we see what thinking and inferring amount to for us, for example—from within, and thus in a way that embraces, rather than dissolves, their normativity.

It is this that gives Wittgenstein's own investigation its deeply philosophical quality and continues, despite all his philosophical radicalism, to bind him to the philosophical tradition. For, in considering the most fundamental features of our thinking—those that define what 'objectivity', 'rationality', and 'truth' amount to for us—the philosophical tradition has attempted to attain a reflective position wherein these notion lose their 'internal' or taken-for-granted character, but are nevertheless, from this same reflective position, shown to be ultimately grounded or justified. In this way, the philosophical tradition has attempted to take reflective responsibility, as it were, for the normativity of our most fundamental cognitive categories. Yet Wittgenstein has reached a point where the traditional philosophical enterprise of depicting such an ultimate explanation or justification appears hopeless. His project, accordingly, is to wean us once and for all from the need for such a justification while, at the same time, showing us how we can continue to take reflective responsibility for our most fundamental human practices nonetheless. We can see that the normativity of these practices rests, in the end, on nothing more nor less than certain very basic but 'mundane' facts of practical social life—yet we can do this in such a way that our commitment to, and responsibility for, the normativity of these practices is in no way endangered or diminished.[50]

It is of particular interest, then, that proponents of the philosophical agenda of SSK also emphasize the new perspective on reflective responsibility made possible by a sociological conception in which objectivity and rationality are seen to be our own constructions. Bloor, at the beginning of his book on Wittgenstein, puts the point this way:

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[50]The themes of commitment and responsibility are emphasized by Cavell, *op. cit.*, in a closely related context.

> If what [Wittgenstein] says is true, or anywhere near the truth, the great categories of objectivity and rationality can never look the same again. Think how often our polemical appeals to these two things depend on portraying them as forms of external compulsion. A social theory of knowledge changes all this. Objectivity and rationality must be things that we forge for ourselves as we construct a form of collective life… The things we had seen ourselves as answerable *to*, we are now answerable *for*. So the body of work that we are about to examine redraws the boundaries of responsibility; it is a subtle attempt to alter our cultural self-consciousness.[51]

And the often cited penultimate sentence of *Leviathan and the Air-Pump* explains that '[a]s we come to recognize the conventional and artifactual status of our forms of knowing, we put ourselves in a position to realize that it is ourselves and not reality that is responsible for what we know'.[52] But to my ear these professions of responsibility ring hollow. One precisely does not express commitment and take responsibility by adopting the purely naturalistic and deliberately non-evaluative point of view of the empirical ethnologist towards one's own intellectual standards. And still less does one do so if, in a philosophical vein, one then proceeds to make sweeping negative claims of the form 'science is *not* a set of universal standards' and 'there is *no* sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such'. For the normativity of one's own standards is now explicitly reduced, from this point of view, to the status of an otherwise arbitrary 'preference' for the practices of one's own particular social group;[53] and the actual normative force or binding power of these standards is thereby inevitably dissolved. Wittgenstein's intense philosophical struggles with the ultimately social character of all human thinking and acting—and with the question of the normativity of our standards in the light of this ultimately social character—show us, in the end, that genuine reflective responsibility cannot be so easily attained.[54]

5. Entanglements of 'Reflexivity'

I have tried to make it clear why Wittgenstein himself does *not* follow the practice of SSK in linking his philosophical exploration of the ultimately social character of all human thought to an empirical, naturalistic sociology of knowledge. An empirical, purely naturalistic treatment of our scientific and other intellectual prac-

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[51]*Wittgenstein*, p. 3.

[52]Shapin and Schaffer, *Leviathan*, p. 344.

[53]See Barnes and Bloor, 'Relativism', pp. 26–27, 29–30. The idea is perhaps most explicit on p. 27, where Barnes and Bloor consider two primitive 'tribes' with different beliefs and standards: 'Faced with a choice between the beliefs of his own tribe and those of the other, each individual would typically prefer those of his own culture. He would have available to him a number of locally acceptable standards to use in order to assess beliefs and justify his preferences. What a relativist says about himself is just what he would say about the tribesman… When confronted with an alien culture he, too, will probably prefer his own familiar and accepted beliefs and his local culture will furnish norms and standards which can be used to justify such preferences if it becomes necessary to do so.' (There follows the passage denying the existence of 'super-cultural norms of rationality' with which we began.)

[54]We are here faced, of course, with the problem of 'reflexivity', which has been much discussed in the more recent theoretical literature on SSK. In the following section I briefly consider this literature.

tices occupies a detached or external standpoint relative to these practices. In parti-cular, the norms and standards that govern these practices from the inside, as it were, now lose their normal, taken-for-granted character. Indeed, when we now ask ourselves, from the outside, why precisely these norms and standards should govern our behavior, they entirely lose, from this point of view, their customary normative force. Traditional philosophical efforts at depicting the ultimate sources or grounds of our knowledge, by contrast, have attempted to occupy an anal-ogous—but non-empirical—external standpoint from which one could underwrite our customarily taken-for-granted standards by depicting a 'transcendental' ration-ality behind our ordinary one. The unique contribution of Wittgenstein's genius was to create a new style and method of philosophizing, wherein we acknowledge both the failure of such philosophical attempts and the ultimately social character of all human thought, while continuing to maintain—and, indeed, to take reflective responsibility for—the normativity of our most fundamental norms and standards without which we could not think at all. And it is therefore absolutely essential, as I hope to have made clear above, that what Wittgenstein has created is a new style of *philosophy* held self-consciously and deliberately distinct from all empirical social science.

It is striking, then, that extended theoretical discussions revolving around pre-cisely these issues have emerged in the recent literature on sociological approaches to science inspired by SSK. What we find, in fact, is the emergence of approaches to the social study of science that have been driven by precisely the tensions between philosophy and social science we have been considering to articulate new methods explicitly intended to overcome these tensions. The discussions in question revolve around the problem of 'reflexivity'—the problem, that is, of applying skept-ical and relativistic conclusions about science in general to the particular case of the social study of scientific knowledge itself. And they have led to new attempts to interpret and apply the thought of Wittgenstein, on the one hand, and to efforts at treating social reality entirely on a par ('symmetrically') with natural reality, on the other. In both cases, perhaps not too surprisingly, the newer approaches have faced vigorous opposition from the 'classical' SSK first articulated at Edinburgh and Bath.

The most detailed discussion of the proper interpretation of Wittgenstein, in this context, occurs in a debate between Michael Lynch, who opposes the theoretical and 'analytical' sociology of knowledge developed by the Edinburgh school on behalf of the more particularistic and descriptive methods of ethnomethodology, and David Bloor.[55] Unlike Bloor, Lynch takes Wittgenstein's anti-theoretical,

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[55]For this debate see Lynch, 'Extending Wittgenstein: the Pivotal Move from Epistemology to the Sociology of Science', Bloor's reply, 'Left and Right Wittgensteinians', and Lynch's rejoinder, 'From the "Will to Theory" to the Discursive Collage', all in A. Pickering, ed., *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992). I am indebted to both Thomas Gieryn and Steven Shapin for urging me to consider this encounter.

purely descriptive ambitions very seriously indeed. And, again unlike Bloor, Lynch pays careful attention to the anti-skeptical dimension of Wittgenstein's treatment of rule following—the extent to which agreed upon intellectual rules, like those of logic and mathematics, can and must have genuine normative force from within the standpoint of the practice in which they are embedded. Accordingly, Lynch reads Wittgenstein as explicitly warning us against the kind of explanatory social science recommended by Bloor, in which we try to *reduce* intellectual norms to social conventions:

> As I understand Wittgenstein's discussion of rules, he specifically warns against such an explanation, but not because of any anticausal irrationalism. He insists that there is no better site for the explication of a rule's sense, relevance, and agreed-to use than the ensemble of expressions and techniques that make up the practice in which it is embedded. He does not offer an alternative form of causal explanation, since a practice is not a center of agency or a causal factor. Rather than trying to explain a practice in terms of underlying dispositions, abstract norms, or interests, a task for sociology would be to describe the ensemble of actions that constitute the practice. This is precisely what ethnomethodology seeks to do.[56]

For Lynch, then, intellectual practices have their own 'internal' authority and integrity, which is not to be reduced to or explained in terms of 'external' social factors such as conventions, institutions, or interests.

There is no doubt, I believe, that Lynch's Wittgenstein is both closer to the actual Wittgenstein and more sophisticated philosophically than Bloor's. Nevertheless, Lynch, too, recommends that we diverge from Wittgenstein's actual philosophical practice by 'replac[ing] a fictitious natural history by a real natural history, and an imaginary ethnography by a real ethnography'. For ethnomethodology, according to Lynch, is 'an empirical extension of Wittgenstein' in which we produce faithful empirical case studies of actual instances of scientific practice—for example, by analyzing tape-recorded or videotaped behaviors and interactions of scientists in the laboratory.[57] Yet, at the same time, Lynch takes such an ethnomethodological perspective to be incompatible with philosophical 'realism' and 'Platonism' (which, I imagine, is the same as what others call 'rationalism'), and he considers this new enterprise to be an extension and development of 'epistemology' which represents, presumably, a distinctive philosophical stance.[58] Here Lynch continues to miss the central Wittgensteinian distinction between 'empirical' and 'grammatical' investigations—which Wittgenstein himself marks, as stressed above, precisely by appealing to imaginary *rather than* to real examples. Lynch thereby misses the Wittgensteinian connection, also emphasized above, between 'grammatical' explorations of the 'essential' features of our language and the exhibition, from within our language, of the normativity of such features. Although Lynch's ethnomethodology indeed eschews sociological theorizing, it continues to

[56]Lynch, 'From the "Will to Theory"', in Pickering, ed., *op. cit*., note 55, p. 290.
[57]See Lynch, 'Extending Wittgenstein', *op. cit*., pp. 256–259.
[58]*Ibid*., pp. 247, 251, 257–259.

be an empirical, purely naturalistic enterprise. As such, it can neither display the normativity of our practices from within nor engage the philosophical tradition.[59]

An even clearer illustration of the tensions between philosophical and empirical investigation here, however, is provided by a second recent debate responding more directly to the problem of 'reflexivity'. This debate involves Bruno Latour and his associates, on the one side, and H. M. Collins and his associates, on the other.[60] Latour began the debate by complaining that the idea of a 'social construction' of scientific facts is groundlessly asymmetrical in its treatment of the facts of natural science which are the primary objects of SSK and the facts invoked by SSK itself (facts about social conventions, institutions, and interests) in order to explain the 'construction' of the former. For, if SSK is itself a scientific enterprise, then the facts to which it appeals should have precisely the same status as all other scientific facts. In this sense, we should no longer speak of an asymmetrical construction of natural reality out of social reality, but we should rather seek for an entirely symmetrical construction of both natural reality and social reality simultaneously.[61] Collins, for his part, objects that a fully symmetrical treatment of natural and social reality simply repeats the 'realist' and 'rationalist' errors of traditional philosophical and historical approaches to science, and, more generally, that it gives too much weight to the natural sciences in relation to the social sciences.[62]

How, then, does Collins deal with the problem of 'reflexivity'? How does he rebut the charge that SSK is groundlessly asymmetrical in refusing to apply the skeptical and relativist philosophy it brings to the productions of the scientists under study also to its own productions *qua* social science? Here Collins invokes the concept of 'meta-alternation', inspired by the sociologist Peter Berger's conception of the 'alternation' between cultural worlds that good sociology and anthro-

[59]In this connection, it is also worth emphasizing that Wittgenstein's own attitude towards the traditional debate between 'realism' and 'constructivism' (that is, 'idealism') is definitely *not* one of philosophical partisanship: see especially PI, §402.

[60]For this debate see Collins and S. Yearley, 'Epistemological Chicken', S. Woolgar, 'Some Remarks About Positionism', M. Callon and Latour, 'Don't Throw the Baby Out with the Bath School!', and Collins and Yearley, 'Journey into Space', all in A. Pickering, ed., *op. cit*. See also the Afterword to the second edition of Collins, *Changing Order*.

[61]This idea, as is well known, is explained in the Preface to the second edition (Princeton: Princeton University Press, 1986) of Latour and Woolgar, *Laboratory Life*, where the subtitle is changed from that of the first edition (London: Sage, 1970), *The Social Construction of Scientific Facts*, to *The Construction of Scientific Facts*.

[62]Although there is not space adequately to consider this issue here, the question of the relationship between the natural sciences and the social sciences traces back, ultimately, to the debates over the relationship between the *Naturwissenschaften* and the *Geisteswissenschaften* that arose within the late nineteenth and early twentieth-century neo-Kantian tradition. Within the tradition of Husserlian phenomenology it then emerged in an especially stark form in an encounter between Husserl and Wilhelm Dilthey in 1911. And this encounter was crucial in moving Martin Heidegger decisively to transform Husserlian phenomenology—in explicit opposition to Husserl's own scientific 'universalism'—in the direction of 'historicism' and 'relativism'. This same issue, finally, forms part of the essential backdrop to the twentieth-century divergence between 'analytic' and 'continental' philosophical traditions—and thus, by implication, to the current problematic of 'post-modernism'. For discussion see my 'Overcoming Metaphysics: Carnap and Heidegger', in R. Giere and A. Richardson, eds, *Origins of Logical Empiricism* (Minneapolis: University of Minnesota Press, 1996).

pology requires.[63] Consistency does indeed require that we apply skeptical and relativist philosophy to SSK itself, just as much as SSK applies this philosophy to the productions of natural science which are its primary object of study. Nevertheless, we cannot do both simultaneously, as it were, for, as scientists, practitioners of SSK need to take a 'natural attitude' of 'social realism' towards the explanatory factors they invoke (conventions, institutions, interests, and the like), just as much as natural scientists need to take a parallel attitude of 'natural realism' towards the explanatory factors invoked in natural science (electrons, quarks, gravitational waves, and the like). We therefore need to perform a 'meta-alternation' between the two standpoints whereby they are hermetically 'compartmentalized' from interfering with one another:

> [SSK] reexamines the nature of science while at the same time doing science. But for the two activities not to interfere with one another one needs to put them in separate compartments. This is the crucial reflexive insight.... If, then, I want to make some new scientific objects—in this case some objects belonging to the social sciences and having to do with the institutions of natural science—I will have to make absolutely certain that I ignore the social origins of these objects within my own practice as a (social) scientist and that my audience is encouraged to remain equally ignorant. Science—the study of an apparently external world—is constituted by not doing the sort of thing that the sociology of scientific knowledge does to science; the point cannot be made too strongly. Sociologists of scientific knowledge who want to find (or help construct) new objects in the world must compartmentalise; they must not apply their methods to themselves.[64]

The tension between the internal norms and standards governing the practice of science, on the one hand, and the distanced, external conclusions of skeptical and relativist philosophy, on the other, could not be more sharply exhibited.

One interpretation of Collins's 'meta-alternation' is congenial to the general point of view I have been urging here. What I have been calling the philosophical agenda of SSK is really not a part of the social scientific and historical practice of SSK. From the point of view of this practice it appears rather as a fundamentally extraneous addition which is in fact quite superfluous to the genuine empirical achievements of SSK. (Indeed, from this point of view, it now appears that the skeptical and relativistic philosophical agenda of SSK is positively harmful to its empirical practice.) But this cannot be what Collins himself intends. His view is rather that what I call the philosophical agenda of SSK is part and parcel of SSK itself. By 'reexamin[ing] the nature of science while at the same time doing science', SSK operates against the background of a presupposition in favor of skeptical and relativistic philosophy—a presupposition which is then later confirmed by the empirical success of SSK.[65] It is essential to Collins's own view, in

[63]See P. Berger, *Invitation to Sociology* (New York: Doubleday, 1963). This is the same Berger, of course, who later, together with Thomas Luckmann, produced the classic text, *The Social Construction of Reality* (London: Allen Lane, 1967).

[64]*Changing Order*, Afterword to second edition, p. 188.

[65]See *op. cit.*, p. 185: '[W]hile sociology of scientific knowledge does not *prove* relativism it does lead inexorably in that direction. This is because the more successful an analysis based on certain presuppositions the more those presuppositions look right… Just as the empirical success of descriptions of the world based on Euclidean geometry encourages us to think that parallel lines never meet, it is

other words, in harmony with the general manner of proceeding within the literature of SSK, that we do *not* make the crucial distinction between philosophical and empirical investigation on which I have been insisting. How, then, can Collins himself possibly 'compartmentalize' the two standpoints? How can he insulate the empirical scientific practice of SSK from the corrosive effects of skeptical and relativistic philosophy?

We can clarify and deepen our appreciation of this problem by glancing back briefly at the contrasting philosophical practice of David Hume, undoubtedly the greatest modern proponent of philosophical skepticism and naturalism. For Hume is careful explicitly to segregate the skeptical conclusions about human reason he reaches as a philosopher, alone in his 'chamber' in a condition of 'forelorn solitude', from the 'natural' attitude exhibited in science and everyday life when he emerges, whereupon his previous skeptical reflections appear 'cold, and strain'd, and ridiculous'. These skeptical reflections, undertaken from the philosophical standpoint, indeed provide an effective counterweight to the pretensions of philosophical rationalism. But the practicing scientist, immersed in the world and thus in the 'natural' attitude, has no real use for *either* of these philosophical convictions.[66] And an analogous situation obtains in the relativism of linguistic frameworks articulated by Rudolf Carnap. This relativism is addressed entirely to philosophers, and it is intended, in particular, to free philosophy from fruitless and interminable disputes by leading philosophers to reconceive their problems as involving purely conventional, purely pragmatic choices of one or another form of linguistic framework for the total language of science. In this way, most importantly, we see that philosophy—*as opposed to science*—is involved with no genuine theoretical questions whatsoever.[67] For both Hume and Carnap, therefore, we insulate the practice of science from the corrosive effects of skeptical and relativistic philosophy precisely by sharply segregating philosophy, as a discipline, from the scientific enterprise. Here, as we have seen, they are also entirely in agreement with Wittgenstein.

6. Conclusion

The theoretical debates in the recent literature on sociological approaches to science appear, to my mind, to have become bogged down in tortuous metaphysics. I fear that the smoothly running gears of empirical social history are in danger of

the fruitfulness of the sociological case studies that leads us to reevaluate the nature of science.' For Collins, then, philosophical relativism is as much an essential part of the empirical practice of SSK as Euclidean geometry is of classical physics. (I hate to be pedantic, but it is tiresome to see the same mistake so often repeated: 'not meeting' is the *definition* of 'parallel'—the Euclidean parallel postulate then asserts that given any line and any point not on that line in a plane, *there exists one and only one* line through this point in the plane that is parallel to the original line.)

[66] See *A Treatise of Human Nature*, Book I, Section VII. Here I am especially indebted to an unpublished paper by Graciela De Pierris, 'Causality as a Philosophical Relation in Hume', a version of which was presented at the Twenty-Third Hume Society Conference, University of Nottingham, July 15–16, 1996.

[67] See my 'Tolerance and Analyticity in Carnap's Philosophy of Mathematics', in J. Floyd and S. Shieh, eds., *Future Pasts* (Cambridge, MA: Harvard University Press, forthcoming).

being ground to a halt by too precipitous philosophical partisanship. Nevertheless, as we have seen, there are good historical reasons behind the contemporary fascination with skeptical and relativistic philosophy—reasons arising from the mutual interaction between the historical evolution of scientific philosophy and the historical evolution of the sciences themselves. So I am not suggesting that social historians of science simply give up on their fascination with philosophy.

My suggestion is rather that philosophically minded social historians of science apply their historical and contextual skills to the evolution of philosophy as well—that they treat philosophy in precisely as historically and contextually sensitive a manner as they treat the scientific disciplines. Indeed, we already have works by socially sensitive historians of science that do precisely this—at least with respect to aspects of the older philosophical tradition. One important example is just the seminal *Leviathan and the Air-Pump* itself, for one of the main innovations of this study is the way in which it traces the unexpected involvement of Hobbes's political philosophy in seventeenth-century debates over the emergence of experimental science.[68] A second example is the work of Peter Dear on the early modern period, in which philosophical themes in such thinkers as Descartes and Mersenne are subtly woven together with the parallel evolution of the new science.[69] A third example is recent work of Joan Richards on the reception of non-Euclidean geometry in Victorian England, where themes from the philosophy of geometry—culminating in the early philosophy of geometry of Bertrand Russell—are illuminatingly integrated with social and intellectual history of mathematics.[70] And, finally, although I am by no means a social historian, the present discussion of the intellectual context and development of Wittgenstein's philosophizing may perhaps also serve as an example, which could, in particular, point those centrally concerned with our contemporary relativistic philosophical predicament in a more fruitful and promising direction.

[68]Shapin and Schaffer, *op. cit*., especially Chapters III and IV.

[69]See P. Dear, *Mersenne and the Learning of the Schools* (Ithaca: Cornell University Press, 1988) and *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago: University of Chicago Press, 1995). I might also mention D. Bertoloni Meli, *Equivalence and Priority: Newton versus Leibniz* (Oxford: Clarendon, 1993), where philosophical divergences between Newton and Leibniz are interwoven with mathematical, physical, and methodological ones. Happily, historians of early modern philosophy are now paying increasing attention to such interweaving as well. See, for example, Daniel Garber, *Descartes' Metaphysical Physics* (Chicago: University of Chicago Press, 1992); Douglas Jesseph, *Berkeley's Philosophy of Mathematics* (Chicago: University of Chicago Press, 1993); Lynn Joy, *Gassendi the Atomist* (Cambridge: Cambridge University Press, 1987); Paolo Mancosu, *Philosophy of Mathematics and Mathematical Practice in the Seventeenth Century* (Oxford: Oxford University Press, 1996); Catherine Wilson, *Leibniz's Metaphysics* (Princeton: Princeton University Press, 1989) and *The Invisible World: Early Modern Philosophy and the Invention of the Microscope* (Princeton: Princeton University Press, 1995).

[70]See J. Richards, *Mathematical Visions: The Pursuit of Geometry in Victorian England* (Boston: Academic Press, 1988).