Science and Sociology of Science: Beyond War and Peace

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March 15, 1999
slightly revised November 4, 1999
missing reference added June 16, 2000

To appear in *The One Culture: A Conversation about Science*,
edited by Jay Labinger and Harry Collins
(University of Chicago Press, 2001)

1 Introduction: Neither War nor Peace

When we were asked to contribute to this volume, our immediate reaction was: We did not want a "science war" in the first place, so why should we now want peace? We don't want a truce either. These are simply not the right categories for an intellectual discussion. In "peace talks", one can and must negotiate: I'll give you this and you give me that. But truth cannot be negotiated in this way. In fact, adopting this "diplomatic" terminology would amount to conceding too much to the relativist philosophy — for which intellectual discussion is nothing more than a power struggle involving a mixture of persuasion, coercion and negotiation — that we want to criticize. On the other hand, we do believe in exchanges of ideas: these serve to clarify areas of agreement and disagreement, to test theses by subjecting them to objections, and more generally to promote a collective search for the truth. And that is why we are here.

For us it all started a few years ago: we were both puzzled and irritated by the prevailing philosophy in certain intellectual circles — encompassing large parts of the humanities, anthropology and sociology of science — whereby all facts are "socially constructed", scientific theories are mere "myths" or "narrations", scientific debates are resolved by "rhetoric" and "enlisting allies", and truth is simply intersubjective agreement. If all this seems an overstatement, consider the following assertions:

> [T]he validity of theoretical propositions in the sciences is in no way affected by factual evidence.[1]

> The natural world has a small or non-existent role in the construction of scientific knowledge.[2]

> Since the settlement of a controversy is the *cause* of Nature's representation, not the consequence, we can never use the outcome — Nature — to explain how and why a controversy has been settled.[3]

> For the relativist [such as ourselves] there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally

[1] Gergen (1988, p. 37).

[2] Collins (1981, p. 3). Two qualifications need to be made: First, this statement is offered as part of Collins' introduction to a set of studies (edited by him) employing the relativist approach, and constitutes his summary of that approach; he does not *explicitly* endorse this view, though an endorsement seems implied by the context. Second, while Collins appears to intend this assertion as an empirical claim about the history of science, it is possible that he intends it neither as an empirical claim nor as a normative principle of epistemology, but rather as a methodological injunction to sociologists of science: namely, to act *as if* "the natural world ha[d] a small or non-existent role in the construction of scientific knowledge", or in other words to *ignore* ("bracket") whatever role the natural world may in fact play in the construction of scientific knowledge. We shall argue in Section 3.2 below that this approach is seriously deficient *as methodology* for sociologists of science.

[3] Latour (1987, pp. 99, 258). See Sokal and Bricmont (1998, chap. 4) for a detailed discussion.

> accepted as such.[4]

> Science legitimates itself by linking its discoveries with power, a connection which *determines* (not merely influences) what counts as reliable knowledge ...[5]

Out of this irritation came Sokal's *Social Text* parody article and our book.[6] Rather than repeat here all the arguments contained in our book and in other publications[7], we shall try to summarize our main objections to the trends in Science Studies (or Sociology of Scientific Knowledge) loosely inspired by the "Strong Programme". These objections are epistemological and methodological; therefore, we will not discuss here the details of "case studies". While we do not deny that interesting work may have been done in those studies — particularly when the authors violated their own declared methodological precepts[8] — this fact does not answer our objections aimed at the principles of the Strong Programme.

Over the last three years, we have participated in numerous debates with sociologists, anthropologists, psychologists, psychoanalysts and philosophers. Although the reactions were extremely diverse, we have repeatedly met people who think that assertions of fact about the natural world can be true "in our culture" and yet be false in some other culture.[9] We have met people who systematically confuse facts and values, truths and beliefs, the world and our knowledge of it. Moreover, when challenged, they will consistently deny that such distinctions make sense. Some will claim that witches are as real as atoms, or pretend to have no idea whether the Earth is flat, blood circulates or the Crusades really took place. Note that these people are otherwise reasonable researchers or university professors. All this indicates the existence of a radically relativist academic Zeitgeist, which is weird.[10] To be sure, these are oral statements made in seminars or private discussion, and oral statements usually tend to be more radical than written ones. But the published written assertions quoted in the preceding paragraph are already quite weird.[11]

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[4] Barnes and Bloor (1981, p. 27), clarification added by us.

[5] Aronowitz (1988, p. 204), emphasis in the original.

[6] Sokal (1996), Sokal and Bricmont (1998). Though deliberate satire may seem to be a warlike tactic, we should stress that the main reason for using this tool was to force a honest debate which seemed blocked.

[7] See Sokal (1998) and Bricmont (1999). For related arguments, see Nagel (1997), Haack (1998) and Kitcher (1998).

[8] See Gingras (1995) for a critical analysis of several recent tendencies in Science Studies.

[9] For an example involving the origins of Native American populations, see Sokal and Bricmont (1998, Epilogue) and Boghossian (1996).

[10] We emphasize that we have no idea how widespread these extreme positions are. But their mere existence is weird enough.

[11] For extremely weird written statements, see also the discussion by Latour of the causes of the death of the pharaoh Ramses II (Latour, 1998); and for a critique, see Sokal and Bricmont (1998, note 123).

If one enquires about the justifications for these surprising views, one is invariably led to the "usual suspects": the writings of Kuhn, Feyerabend and Rorty; the underdetermination of theories by data; the theory-ladenness of observation; some writings of (the later) Wittgenstein; the "Strong Programme" in the sociology of science. Of course, the latter authors do not usually make the most radical claims that we have heard. Rather, what typically happens is that they make ambiguous or confused statements that are then interpreted by others in a radically relativist fashion. Therefore, our goal in this article will be to disentangle various confusions caused by fashionable ideas in the contemporary philosophy of science. Roughly speaking, our main thesis is that those ideas contain a kernel of truth that can be understood properly when those ideas are carefully formulated; but then they give no support to radical relativism.

However, before getting to work, we want to avoid possible misunderstandings and to emphasize some points which, we hope, are noncontroversial:

1) Science is a human endeavor, and like any other human endeavor it merits being subjected to rigorous social analysis. Which research problems count as important; how research funds are distributed; who gets prestige and power; what role scientific expertise plays in public-policy debates; in what form scientific knowledge becomes embodied in technology, and for whose benefit — all these issues are strongly affected by political, economic and to some extent ideological considerations, as well as by the internal logic of scientific inquiry. They are thus fruitful subjects for empirical study by historians, sociologists, political scientists and economists.

2) At a more subtle level, even the content of scientific debate — what types of theories can be conceived and entertained, what criteria are to be used for deciding between competing theories — is constrained in part by the prevailing attitudes of mind, which in turn arise in part from deep-seated historical factors. It is the task of historians and sociologists of science to sort out, in each specific instance, the roles played by "external" and "internal" factors in determining the course of scientific development. Not surprisingly, scientists tend to stress the "internal" factors while sociologists tend to stress the "external", if only because each group tends to have a poor grasp on the other group's concepts. But these problems are perfectly amenable to rational debate.

3) There is nothing wrong with research informed by a political commitment, as long as that commitment does not blind the researcher to inconvenient facts. Thus, there is a long and honorable tradition of socio-political critique of science[12], including antiracist critiques of anthropological pseudo-science and eugenics and feminist critiques of psychology and parts of medicine and biology. These critiques typically follow a standard pattern: First one shows, using conventional scientific arguments, why the research in question is flawed *according to the ordinary canons of good science*; then, *and only then*, one attempts to explain how the researchers' social prej-

[12]We limit ourselves here to critiques challenging the substantive content of scientific theories or methodology. Other important types of critiques challenge the uses to which scientific knowledge is put (e.g. in technology) or the social structure of the scientific community.

udices (which may well have been unconscious) led them to violate these canons. Of course, each such critique has to stand or fall on its own merits; having good political intentions doesn't guarantee that one's analysis will constitute good science, good sociology or good history. But this general two-step approach is, we think, sound; and empirical studies of this kind, if conducted with due intellectual rigor, could shed useful light on the social conditions under which good science (defined normatively as the search for truths or at least approximate truths about the world) is fostered or hindered.[13]

Now, we don't want to claim that these three points exhaust the field of fruitful inquiry for historians and sociologists of science, but they certainly do lay out a big and important area. In any case, our criticisms of SSK are aimed only at radical epistemological and methodological claims that go far beyond the above-mentioned truisms.

This paper is organized as follows: First of all, we have to clear the epistemological ground (Section 2). Next, we formulate our objections to the Strong Programme (Section 3). And finally, we shall indicate what we think are some possible areas of collaboration between scientists and sociologists of science (Section 4).

2 Some Brief Epistemological Remarks

2.1 Radical skepticism, underdetermination and all that

Before discussing some serious issues in the philosophy of science, we need to clear out of the way some old red herrings. The first point that should be non-controversial is that solipsism (the idea that there is nothing in the world except my sensations) and radical skepticism (that no reliable knowledge of the world can ever be obtained) cannot be refuted. It is doubtful whether anyone really believes those doctrines — at least when crossing a city street — but their irrefutability is nevertheless an important philosophical observation. Since the arguments are standard and go back at least to Hume, we need not repeat them here. Unfortunately, many of the arguments adduced in favor of relativist ideas are, in reality, banal reformulations of radical skepticism but applied in unjustifiably selective ways.[14]

In the same way that nearly everyone in his or her everyday life disregards solipsism and radical skepticism and spontaneously adopts a "realist" or "objectivist" attitude toward the external world, scientists spontaneously do likewise in their professional work. Indeed, scientists rarely use the word "realist", because it is taken for granted:

[13] Of course, we don't mean to imply that the *only* (or even principal) purpose of the history of science is to help working scientists. History of science obviously has intrinsic value as a contribution to the history of human society and human thought. But it seems to us that history of science, when done well, can *also* help working scientists.

[14] Another favorite tactic employed by relativists is to conflate facts and our knowledge of them, not by giving any argument, but simply by using intentionally ambiguous terminology. See Sokal and Bricmont (1998, chap. 4) for examples in the works of Kuhn, Barnes–Bloor, Latour and Fourez.

of course they want to discover (some aspects of) how the world really is! And, of course, they adhere to the so-called "correspondence theory of truth"[15] (again, a word that is barely used): if someone says that it is true that a given disease is caused by a given virus, she means that, in actual fact, the disease is caused by the virus. Philosophers often regard such views as naive, but we would like to show that they are actually quite defensible, with, however, some important qualifications.

The main objections to the scientists' spontaneous attitude consist in various theses showing that theories are underdetermined by data.[16] In its most common formulation, the underdetermination thesis says that, for any finite (or even infinite) set of data, there are infinitely many mutually incompatible theories that are "compatible" with those data. This thesis, if not properly understood[17], can easily lead to radical conclusions. The scientist who believes that a disease is caused by a virus presumably does so on the basis of some "evidence" or some "data". Saying that a disease is caused by a virus presumably counts as a "theory" (e.g., it involves, implicitly, many counterfactual statements). But, if one is able to convince the scientist that there are infinitely many distinct theories that are compatible with those "data", he or she may well wonder on what basis one can rationally choose between those theories.

In order to clarify the situation, it is important to understand how the underdetermination thesis is established; then, its meaning and its limitations become much clearer. Here are some examples of how underdetermination works; one may claim that:

– The past did not exist: the universe was created five minutes ago along with all the documents and all our memories referring to the past in their present state. Alternatively, it could have been created 100 or 1000 years ago.

– The stars do not exist: instead, there are spots on a distant sky that emit exactly the same signals as those we receive.

– All criminals ever put in jail were innocent. For each alleged criminal, explain away all testimony by a deliberate desire to harm the accused; declare that all evidence was fabricated by the police and that all confessions were obtained by force.[18]

Of course, all these "theses" may have to be elaborated, but the basic idea is clear: given any set of facts, just make up a story, no matter how *ad hoc*, to "account" for the facts without running into contradictions.[19]

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[15]We would not even call it a "theory"; rather, we consider it a *precondition for the intelligibility* of assertions about the world.

[16]Often called the Duhem–Quine thesis. In what follows, we will refer to Quine's version (Quine, 1980), which is much more radical than Duhem's.

[17]Particularly concerning the meaning of the word "compatible". See also Laudan (1990b) for a more detailed discussion.

[18]Of course, this latter situation, unlike the previous two, *does* occur frequently enough. But its occurrence or not depends on the particular case, while the undeterdetermination thesis is a *general* principle meant to apply to *all* cases.

[19]In the famous paper in which Quine sets forth the modern version of the underdetermination

It is important to realize that this is all there is to the general (Quinean) underdetermination thesis. Moreover, this thesis, although it played an important role in the refutation of the most extreme versions of logical positivism, is not very different from the observation that radical skepticism or even solipsism cannot be refuted: all our knowledge about the world is based on some sort of inference from the observed to the unobserved, and no such inference can be justified by deductive logic alone. However, it is clear that, in practice, nobody ever takes seriously such "theories" as those mentioned above, any more than they take seriously solipsism or radical skepticism. Let us call these "crazy theories"[20] (of course, it is not easy to say exactly what it means for a theory to be non-crazy). Note that these theories require no work: they can be formulated entirely *a priori*. On the other hand, the difficult problem, given some set of data, is to find even one non-crazy theory that accounts for them. Consider, for example, a police enquiry about some crime: it is easy enough to invent a story that "accounts for the facts" in an *ad hoc* fashion (sometimes lawyers do just that); what is hard is to discover who really committed the crime and to obtain evidence demonstrating that beyond a reasonable doubt. Reflecting on this elementary example clarifies the meaning of the underdetermination thesis. Despite the existence of innumerable "crazy theories" concerning any given crime, it sometimes happens in practice that there is a unique theory (i.e. a unique story about who committed the crime and how) that is *plausible* and compatible with the known facts; in that case, one will say that the criminal has been discovered (with a high degree of confidence, albeit not with certainty). It may also happen that no plausible theory is found, or that we are unable to decide which one among several suspects is really guilty: in these cases, the underdetermination is real.

2.2 Redefinitions of truth

When facing the problems caused by underdetermination, one may be tempted by a radical turn: What about abandoning the notion of "truth" as "correspondence with reality", and seeking instead an alternative notion of truth? There are at least two currently fashionable proposals of this kind: one is to define truth through utility or convenience, the other is to define it through intersubjective agreement. The philosopher Richard Rorty offers examples of both:

> What people like Kuhn, Derrida and I believe is that it is pointless to ask whether there really are mountains or whether it is merely convenient for us to talk about mountains.[21]

thesis, he even allows himself to change the meanings of words and the rules of logic, in order to show that any statement can be held true, "come what may" (Quine, 1980).

[20] Or, as the physicist David Mermin calls them, "Duhem–Quine monstrosities" (Mermin, 1998).

[21] Rorty (1998, p. 72). See also the critiques by Nagel (1997, pp. 28–30) and Albert (1998); and see Haack (1997) for an entertaining contrast between the two radically different "pragmatist" philosophies of C.S. Peirce and of Rorty.

> Philosophers on my side of the argument answer that objectivity is not a matter of corresponding to objects but a matter of getting together with other subjects — that there is nothing to objectivity except intersubjectivity.[22]

Similar views are expressed by some of the founders of the Strong Programme in the sociology of science:

> The relativist, like everyone else, is under the necessity to sort out beliefs, accepting some and rejecting others. He will naturally have preferences and these will typically coincide with those of others in his locality. The words 'true' and 'false' provide the idiom in which those evaluations are expressed, and the words 'rational' and 'irrational' will have a similar function.[23]

The best way to see that these redefinitions do not work is to apply them to simple concrete examples. For instance, it would certainly be useful to make people believe that if they drive drunk they will go to hell or die from cancer, but that would not make those statements true (at least on an intuitive understanding of the word "true"). Similarly, once upon a time, people agreed that the Earth was flat (or that blood was static, etc.), and we now know that they were wrong. So intersubjective agreement does not coincide with truth (again, understood intuitively).

Of course, we are using here an intuitive notion of truth, and a critic might demand a more "rigorous" definition. But the problem is that all definitions tend to be circular or else to rely on fundamental undefined terms that one either grasps intuitively or does not grasp at all. And truth falls naturally in the latter category.[24]

Since these redefinitions of "truth" are so patently absurd, why are they proposed so often[25] and why are they so popular? Presumably, the answer has to do with the fact that, radical skepticism being irrefutable, one can always doubt any particular truth without running into logical contradiction. But these redefinitions do not even solve the problem of radical skepticism. Take, for instance, utility: saying that something is useful (for some specified goal) is already an objective statement (it has to be *really* useful for the declared goal) that relies implicitly on the correspondence notion of truth. The same remark is even more obvious for intersubjective agreement: to say that (other) people think so and so is an objective statement describing part of the (social) world "as it is".

Of course, positive arguments are sometimes given to support redefinitions of truth, as for instance the following somewhat subtle sophism:

> ... the only criterion we have for applying the word "true" is justification and justification is always relative to an audience. So it is also relative to that

[22] Rorty (1998, pp. 71–72).

[23] Barnes and Bloor (1981, p. 27). See Sokal and Bricmont (1998, chap. 4) for a critique.

[24] After all, people who ask what "truth" means are not really in the same position as those who wonder what an octopus is or who Xenophon was.

[25] For a discussion of similar proposals, see Bertrand Russell's critique of the pragmatism of William James and John Dewey (Russell, 1961, chaps. 24 and 25, in particular p. 779).

> audience's lights — the purpose that such an audience wants served and the situation in which it finds itself.[26]

The beginning of the first sentence is correct, but it does not imply that truth is identical to justification. (One may well be rationally justified in believing something that turns out, on closer examination, to be false.[27]) Moreover, what does it mean to say that justification is always relative to the purpose that an audience wants served? This introduces a subtle confusion between knowledge and values, by implicitly assuming that all knowledge depends on some "purpose", i.e. some non-cognitive goal. But what if the "audience" wants to find out how (some part of) the world really is? Rorty might reply that this goal is unattainable, as the following statement suggests: "A goal is something you can know you are getting closer to, or farther away from. But there is no way to know our distance from the truth, not even whether we are closer to it than our ancestors were."[28] But is this really so? Some of our ancestors thought that the Earth was flat. Don't we know better? Aren't we closer to the truth, in that respect at least?

The view proposed here is so implausible that one is forced to resort to some "charitable" interpretation. Perhaps Rorty means by "truth" something like the fundamental physical laws governing the entire universe, or an "absolute" truth discovered by pure thought (as in classical metaphysics); and it does makes sense to be skeptical about our ability to discover truths of those kinds. But if this is what Rorty means, then he should say so explicitly, rather than making statements that allegedly apply to all possible knowledge. Or, alternatively, perhaps Rorty simply wants to reiterate the banal observation that all statements of fact (even about the flatness of the Earth) can be challenged by a consistent radical skeptic. But that is not a particularly new insight.

2.3 Then, what should one do?

Given the problems raised by underdetermination, and since redefining truth leads us from bad to worse, what should one do? There is no abstract and general answer to this question. We are, in some sense, "screened" from reality (we have no immediate access to it, radical skepticism cannot be refuted, etc.). There are no absolutely secure foundations on which to base our knowledge. Nevertheless, we all assume implicitly that we can obtain some reasonably reliable knowledge of reality, at least in everyday life. Let us try to go farther, putting to work all the resources of our fallible and finite minds: observations, experiments, reasoning. And then let us see how far we can go.

[26]Rorty (1998, p. 4).

[27]For example, Hume (1988 [1748], section X) gives the example of a person in India who, quite rationally, refused to believe that water can become solid during winter (water solidifies very abruptly around the freezing point, so if one lives in a warm climate, it is indeed hard to believe that water can freeze). It shows that rational inferences from the available evidence do not necessarily lead to true conclusions.

[28]Rorty (1998, pp. 3–4).

In fact, the most surprising thing, shown by the development of modern science, is how far we seem to be able to go.

A friend of ours once said: "I am a naive realist. But I admit that knowledge is difficult." This is the root of the problem. Knowing how things really are is the goal of science; this goal is difficult to reach, but not impossible (at least for some parts of reality and to some degrees of approximation). If we change the goal — if, for example, we seek instead a consensus — then of course things become much easier; but as Bertrand Russell observed in a similar context, this has all the advantages of theft over honest toil.

It is important to remember that scientific knowledge needs no "justification" from the outside. The justification for the objective validity of scientific theories (in the sense of being at least approximate truths about the world) lies in specific theoretical and empirical arguments. Of course, philosophers, historians or sociologists may be impressed by the successes of the natural sciences (as the logical positivists were) and seek to understand how science works. But there are two frequent mistakes to avoid: One is to think that, because some particular account fails (say, the logical-positivist one or the Popperian one), then some alternative account (e.g. the socio-historical one) *must work*. But that is an obvious fallacy; perhaps *no* existing account works.[29] The second, and more fundamental, mistake is to think that our inability to account in general terms for the success of science somehow makes scientific knowledge less reliable or less objective. That confuses accounting and justifying. After all, Einstein and Darwin gave arguments for their theories, and those arguments were far from being all erroneous. Therefore, even if Carnap's and Popper's epistemologies were entirely misguided, that would not begin to cast doubt on relativity theory or evolution.

Moreover, the underdetermination thesis, far from undermining scientific objectivity, actually makes the success of science all the more remarkable. Indeed, what is difficult is not to find a story that "fits the data", but to find even one *non-crazy* such story. How does one know that it is non-crazy? A combination of factors: its predictive power, its explanatory value, its breadth and simplicity, etc. Nothing in the underdetermination thesis tells us how to find inequivalent theories with some or all of these properties. In fact, there are vast domains in physics, chemistry and biology where there is a unique non-crazy theory that accounts for the known facts and where many alternative theories have been tried and failed because their predictions contradicted experiments. In those domains, one can reasonably think that our present-day theories are at least approximately true.[30]

[29] See McGinn (1993, chap. 7) for the interesting suggestion that understanding our own knowledge-producing mechanisms simply lies outside the bound of what is biologically feasible for our limited minds.

[30] With certain caveats to be made about the status of unobservable entities introduced in physics and the related debates between realists and instrumentalists. See Bricmont (1999) for a more detailed discussion. Note, however, that relativists sometimes tend to fall back on instrumentalist positions when challenged, but there is a profound difference between the two attitudes. Instrumen-

Now that we have sketched our attitude on epistemological issues, let us turn to the consequences for contemporary sociological studies of science.

3 Against Relativism

There exist several contemporary variants of relativism. Quite often, people deny being relativist because they are relativist in some other sense than the one under consideration. So it is necessary to go over several possible meanings of the word. We shall consider here only two meanings: cognitive (or epistemic) relativism, and methodological relativism. Our main thesis is that cognitive relativism is a position that no scientist (in either the natural or the social sciences) should wish to embrace, and that methodological relativism makes sense only if one adheres to cognitive relativism.

3.1 Cognitive relativism

Roughly speaking, we will use the term "relativism" to refer to any philosophy that claims that the truth or falsity of a statement is relative to an individual or to a social group.[31]

The first thing to notice about cognitive relativism is that this doctrine follows naturally if we accept a radical redefinition of truth: clearly, if truth reduces to utility or to intersubjective agreement, then the "truth" of a proposition will depend on the individual or the social group in question. On the other hand, if we adopt the customary ("correspondence") notion of truth, then cognitive relativism is patently false: since a proposition is true to the extent that it reflects (some aspects of) the way the world is, its truth or falsity depends on the way the world is and not on the beliefs or other characteristics of any individual or group.

Since we have already discussed redefinitions of truth, there is not much to add, except that it makes no sense for ordinary scientists — whether they study nature or society — to adopt, even implicitly, a cognitive relativist attitude. For cognitive relativism amounts to abandoning the goal of objective knowledge pursued by science. However, it seems that some historians and sociologists want to have it both ways: adopt a relativist attitude with respect to the natural sciences, and an objec-

talists may want to claim either that we have no way of knowing whether "unobservable" theoretical entities really exist, or that their meaning is defined solely through measurable quantities; but this does not imply that they regard such entities as "subjective" in the sense that their meaning would be significantly influenced by extra-scientific factors (such as the personality of the individual scientist or the social characteristics of the group to which she belongs). Indeed, instrumentalists may regard our scientific theories as, quite simply, the most satisfactory way that the human mind, with its inherent biological limitations, is capable of understanding the world.

[31] We will consider only relativism about statements of fact (i.e. about what exists or is claimed to exist), and leave aside relativism about ethical or aesthetic judgments.

tivist (even naive realist) attitude with respect to the social sciences.[32] But that is inconsistent; after all, research in history, and in particular in the history of science, employs methods that are not radically different from those used in the natural sciences: studying documents, drawing the most rational inferences, making inductions based on the available data, and so forth. If arguments of this type in physics or biology did not allow us to arrive at reasonably reliable conclusions, what reason would there be to trust them in history or sociology? Why speak in a realist mode about historical categories, such as Kuhnian paradigms, if it is an illusion to speak in a realist mode about scientific concepts (which are in fact much more precisely defined) such as electrons or DNA?

3.2 Methodological relativism

Methodological relativism is not in itself a philosophical position; it is rather, as the name indicates, a set of methodological principles. This relativism is associated to developments in the history and sociology of science that began during the 1970s under the banner of the so-called "Strong Programme" and which have had an enormous impact in the field of sociology of scientific knowledge (SSK) as well as outside that field (in cultural studies, anthropology, etc.).[33] The Strong Programme proposes to give a causal account of the acceptance of scientific ideas, while remaining "impartial" (or "symmetrical") as to whether they are true or false, rational or irrational. Here is how David Bloor lays out the principles for the new sociology of knowledge:

> 1. It would be causal, that is, concerned with the conditions which bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief.
>
> 2. It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation.
>
> 3. It would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs.
>
> 4. It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. (Bloor 1991, p. 7).

How is one to understand the symmetry and the impartiality theses? In order to see the difficulty, let us first consider perception in everyday life (we'll turn to scientific theories in a moment). Suppose that several of us are standing outdoors in the rain, and someone says: "It is raining today." That statement expresses a belief;

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[32]See Sokal and Bricmont (1998, chap. 4) for relevant quotes from Kuhn, Feyerabend, Barnes–Bloor and Fourez, along with a more detailed critique.

[33]See Laudan (1981, 1990a), Slezak (1994a, 1994b) and Kitcher (1998) for related criticisms of the Strong Programme. Note particularly Kitcher's criticism of the "Four Dogmas of Science Studies" (1998, pp. 38–45), which is quite similar to our own critique.

how are we to explain this belief "causally"? Well, no one today knows the complete details of the causal mechanisms, but it seems obvious that part of the explanation involves the fact that it really is raining today. If someone said that it is raining when it is not, one might think that he is joking or that he is mentally disturbed; but the explanations would be very asymmetrical, depending on whether it is raining or not.[34]

Faced with this problem, supporters of the Strong Programme could admit what we say for ordinary knowledge, but maintain that it does not apply to scientific knowledge: in the latter, reality would play little or no role in constraining our beliefs.[35] However, this claim looks particularly implausible, since scientific activity — far more so than everyday life — is set up (through experiments, etc.) precisely so as to make Nature itself constrain our beliefs about it as strongly as possible.

Let's consider, once again, a concrete example: Why did the European scientific community become convinced of the truth of Newtonian mechanics sometime between 1700 and 1750? Undoubtedly a variety of historical, sociological, ideological and political factors must play a part in this explanation — one must explain, for example, why Newtonian mechanics was accepted quickly in England but more slowly in France[36] — but certainly *some* part of the explanation (and a rather important part at that) must be that the planets and comets really do move (to a very high degree of approximation, though not exactly) as predicted by Newtonian mechanics.[37]

At the risk of beating a dead horse, let us rephrase our critique of the Strong Programme's sociological reductionism as a *reductio ad absurdum*. Consider the following thought-experiment: Suppose that a Laplacian demon were to give us all conceivable information about seventeenth-century England that could in any way be called sociological or psychological: all the conflicts between members of the Royal Society, all the data about economic production and class relations, etc. Let's even include documents that have been destroyed and private conversations that were never recorded.

[34]See Gross and Levitt (1994, pp. 57–58) for a similar discussion. Of course, even ordinary perception is "social" in some sense. For example, in order to see clearly, some people need eyeglasses that are socially produced. More fundamentally, the meaning of the words through which one expresses one's perceptions is to some extent influenced by the environment in which they are used. Sometimes relativists insist that all they claim is that science is "social" is some equally weak sense; but that seems to us like a considerable watering-down of the "symmetry" thesis. Indeed, when one studies perception scientifically, there is no "symmetry", in any meaningful sense, between hallucination and correct perception. And the difference between the two is related to how the world really is, so that the latter is partly causally responsible for correct perceptions.

[35]See note 2 above for an explicit assertion of this thesis by Harry Collins.

[36]See, for example, Brunet (1931) and Dobbs and Jacob (1995).

[37]Or more precisely: There is a vast body of extremely convincing astronomical evidence in support of the belief that the planets and comets do move (to a very high degree of approximation, though not exactly) as predicted by Newtonian mechanics; and *if* this belief is correct, then it is the fact of this motion (and not merely our belief in it) that forms part of the explanation of why the eighteenth-century European scientific community came to believe in the truth of Newtonian mechanics.

Add to this a gigantic super-fast computer that can process all this information as much as desired. But do not include any astronomical data (such as Kepler's observations). Now, try to "predict" from those data that scientists will accept a theory in which the gravitational force decays with the inverse square of the distance, rather than the inverse cube. How could one do it? What kind of reasoning could one use? It seems obvious to us that this result simply cannot be "extracted" from the given data.[38]

Now suppose, by contrast, that one wants to give a causal account of belief in astrology. In this case it is at least conceivable that one could obtain a purely sociological or psychological account of the incidence of such beliefs, without ever invoking the good evidence supporting those beliefs — simply because there is no such evidence.[39] This comparison between Newtonian mechanics and astrology shows clearly a necessary and crucial asymmetry in the explanatory scheme: in the one case, evidence must enter into any satisfactory explanation, in the other case not. Note, of course, that if you happen to believe (wrongly) that astrology *is* well supported by evidence, then this factor *should* presumably enter into what you regard as a satisfactory causal account of belief in astrology.

In summary, it seems clear that an adequate causal explanation of how scientific theories come to be accepted would have to combine "natural" and "social" factors, just as for ordinary perception. Of course, explaning scientific knowledge is much more complicated than explaining perception, which is complicated enough.

Earlier in this essay, we made an analogy between scientific investigations and police enquiries. Continuing this analogy, one could say that ontological relativism amounts to saying that there is no objective fact of the matter about whether a particular suspect is innocent or guilty, while epistemological relativism is the assertion that no method of enquiry can be said to be objectively better than another (e.g., carefully analyzing fingerprints versus planting evidence). Methodological relativism, on the other hand, amounts to trying to understand how the police, judge and jury become convinced of X's guilt without ever taking into account the fact that, in some cases at least, there might be good evidence for X's guilt.

Let us consider in this light an assertion of Collins and Pinch about Einstein's theory of relativity:

> Relativity ... is a truth which came into being as a result of decisions about how we should live our scientific lives, and how we should licence our scientific observations; it was a truth brought about by agreement to agree about new things. It was not a truth forced on us by the inexorable logic of a set of crucial

[38] Of course, one can argue that the rise of science is linked to the rise of the bourgeoisie (although the causal link between the two, if any, is unclear); one might even argue that a "mechanical world-view" is associated with the bourgeois ethos. But that kind of argument will not extend to detailed empirical statements like the inverse-square law.

[39] Of course, one may have a separate worry: Does anyone at present have a well-tested sociological or psychological theory that yields a causal and explanatory account of *any* system of beliefs, even superstitious ones?

experiments. (Collins and Pinch, 1993, p. 54)

Wouldn't it sound odd to say that "it is true that X is guilty" but that this truth "came into being as a result of decisions about how we should licence our police investigations; it was a truth brought about by agreement to agree about new things"? The whole thing is plagued with ambiguities: Does one mean to say that X is guilty or not? Is this merely a confusing way of stating the banal observation that our *belief* in X's guilt arose from a social process?[40]

When all is said and done, methodological relativism makes no sense unless one adheres to the idea that the natural sciences form some kind of ideology or religion, while our knowledge of the social world is truly scientific and explains (or will someday explain) why natural scientists believe what they do. But then, we have a direct competition: Which theories are more scientific, i.e. are better supported by evidence, make more accurate predictions, etc.? Those of physics and chemistry and biology, or those of sociology (including the sociology of religion and of fashion)? The answer seems clear enough.[41,42] This unpleasant situation (for sociologists of science) sometimes leads them to employ arguments supporting cognitive relativism, which have the "merit" (from their point of view) of stopping the "direct competition": if no theory is objectively better than another, then physics is not more scientific than sociology. But, as we explained previously, cognitive relativism is not a view that any scientist — natural or social — should want to hold.[43]

[40]Let us note in passing that the last sentence of the quote is correct: the notion of "crucial experiment", which is used by some philosophers of science, grossly oversimplifies the complex web of interlocking evidence that gives support to well-confirmed scientific theories. The physicist David Mermin, in his excellent critique of the account of relativity given by Collins and Pinch, correctly concedes that scientists' oversimplified histories, as presented in textbooks, sometimes do make this error (Mermin, 1996a, 1996b, 1996c, 1997). On the other hand, experiments and observations, *taken collectively*, are indeed crucial since there is no other way to obtain reliable knowledge of the external world.

[41]To avoid misunderstandings, let us emphasize that this does not mean that natural scientists are more clever than sociologists or historians, but simply that they deal with easier problems.

[42]In a similar vein, the chapter of Barnes, Bloor and Henry (1996) on "proof and self-evidence" is eerily fascinating. The authors try to refute the claim that some beliefs, like $2 + 2 = 4$ or the *modus ponens*, are so obvious that they need not be explained sociologically. But their arguments show, at most, that those beliefs are not as evident as they may seem (e.g., because the nature of arithmetic statements is open to divergent interpretations in the philosophy of mathematics, or because the *modus ponens* applies only to ideally precise propositions and not to those containing ill-defined words like "heap"). But that answer misses the obvious point that all human beings — be they physicists or sociologists or plumbers — have, in practice, no sensible alternative but to use arithmetic and logic. And to seek a sociological explanation for such basic notions surely puts the cart before the horse. Do Barnes *et al.* really think that their sociological theories are more reliable than $2 + 2 = 4$ and *modus ponens*? See Nagel (1997) for an elaboration of these arguments, and Mermin (1998) for another critique.

[43]For charity, we have here left aside Bloor's fourth principle ("reflexivity"). Indeed, it seems to us that if sociologists start trying to explain why they hold their own beliefs without taking into account the evidence that those beliefs are somehow better or more objective than those of their

It is interesting to compare SSK and postmodernism, which are often confused by their opponents. Postmodernists tend to reject objectivity even as a goal: everything becomes dependent on one's subjective viewpoint, and moral or aesthetic values displace cognitive ones. Quite the opposite is true for supporters of the Strong Programme, who often appear extraordinarily scientistic: for example, Bloor often emphasizes that his view is materialistic, naturalistic and scientific. But the methodological relativism contained in the "symmetry" and "impartiality" theses — unless it is interpreted in so watered-down a fashion as to become virtually empty — undermines the rationalist aspect of the sociological enterprise, so that practitioners of SSK eventually have to fall back on radical-skeptical arguments about objectivity that often lead them to make common cause with the postmodernists.

4 Conclusion: Real Issues

We do not want to give the impression that there are no interesting questions to be dealt with by sociologists of science. On the contrary, there are lots of them. But we contend that the philosophical confusions currently fashionable in SSK circles hinder rather than foster the possibility of seriously studying them.

The kind of questions we have in mind revolve primarily around the problem of expertise. We are constantly subjected to reports of "expert" opinion, on all possible topics. But should one believe them? Should one believe that tobacco is bad for one's health? That olive oil is good (after all)? That nuclear power plants are safe? That the austerity measures of the IMF are good for the economy? That newspaper reports are accurate? That publications on the "memory of water"[44] concern some real physical effect?

When confronted with experts, any individual or small group of individuals is in a difficult situation. There is no way to find the time and the means to check directly even a small fraction of the experts' assertions. And yet, in many practical situations we have to decide whether or not to trust their claims. How should we proceed? That is a truly interesting and difficult question. But epistemic and methodological relativism do not help here. We want to find out who is right and who is wrong, and that depends ultimately on how the world really is. Nor is the question particularly new: for example, Hume addressed it already, and gave some guidelines for solving it, in his discussion of whether one should believe in miracles.[45] The argument is

critics, then we simply move from error to absurdity. Note that, by contrast, Collins (1992, p. 188) argues that "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." That move allows him to escape from self-refutation, but why should anyone accept his rule? See Friedman (1998) for a more detailed discussion.

[44] An effect allegedly found by French scientist Jacques Benveniste that, if true, would give theoretical support to homeopathy (Davenas *et al.* 1988). See Maddox *et al.* (1988) for a critical analysis and, for a more detailed discussion, see Broch (1992).

[45] Hume (1988 [1748], section X).

well-known: If you have never seen a miracle yourself, your belief is based on believing someone who reports the occurrence of a miracle. But you know from direct experience that people sometimes deceive themselves or cheat others. So, whenever you hear the report of a miracle, it is always more rational to believe — at least in the absence of powerful countervailing evidence — that some kind of deception is taking place rather than a true miracle.[46]

To give a concrete example of the kind of reasoning that we have in mind, consider the issue of the "memory of water". One way to make a "Humean" argument about its plausibility goes as follows: Given that the result, if true, would provoke a revolution in physics and chemistry, at least some scientists around the world should have an interest (in both senses of the word) in duplicating the result. Moreover, the experiment itself does not require huge investments. However, no replication has been claimed, at least not by people totally independent of Benveniste.[47] Negative results are usually not reported, so skepticism with respect to the original experiment becomes reasonable (to say the least). Of course, we are here only suggesting the outline of an argument. A real investigation would have to find out whether the experiment really was easy to replicate and whether attempts at replication (leading to negative results) were in fact made. And that involves considerations both of physics and of sociology.

However, one of the disturbing aspects of SSK's methodological relativism is that its adherents tend to combine an exaggerated skepticism towards conventional scientific knowledge with a rather tolerant (or even favorable) attitude towards the pseudo-sciences. For example, Barnes, Bloor and Henry comment sympathetically on homeopathy and astrology[48], and go so far as to assert that the data gathered by Gauquelin in support of the astrological theory that there is a "Mars effect" affecting the destiny of sports champions "could conceivably come to be accommodated as a triumph of the scientific method".[49] As Mermin observes, following a Humean type of reasoning, the data of Gauquelin are so surprising (amounting to a "miracle") that it is reasonable to assume rather that some kind of deception (or self-deception) has occurred.[50] Another example is provided by the comment by Collins and Pinch that "if homeopathy cannot be demonstrated experimentally, it is up to scientists, who know the risks of frontier research, to show why."[51] But this amounts to shifting the burden of evidence: it is up to advocates of homeopathy to "demonstrate experimen-

[46]As Hume observed, it is even rational for an inhabitant of India to disbelieve the claim that water can become solid during winter: see note 27 above.

[47]Unlike, for example, the experiments showing the existence of superconductivity at high temperatures, which were replicated around the world within a few weeks.

[48]Barnes, Bloor and Henry (1996, chap. 6).

[49]Barnes, Bloor and Henry (1996, p. 141).

[50]Mermin (1998). See also Benski *et al.* (1996) for a critical and detailed factual examination of the "Mars effect".

[51]Collins and Pinch (1993, p. 144).

tally" that their therapy works beyond the placebo effect, not the other way around. It is unfortunately easy to find similar statements: for instance, in one of the early books on "Alternatives to Big Science", one could already read: "... future prospects for a great breakthrough in Western science and in the occult sciences of the Orient are said to be good. Astrology will again become a recognised science, once it has made use of cybernetics and statistical analysis. But such claims fall on the deaf ears of official science."[52]

These remarks are shocking but are perhaps not so surprising, since the "neutrality" of the Strong Programme leads its adherents to disregard any epistemological distinctions between science and pseudo-science.

Of course, believing experts is not the same thing as believing in miracles (or in the pseudo-sciences); and in order to formulate deeper principles of rational inference in real-life situations, many sociological considerations become relevant. First of all, it is important to have at least a rough idea of how "experts" in a given field become accredited and what methods they use within their area of "expertise": this allows one to distinguish, on epistemological grounds, between licensed medical doctors and equally highly "accredited" practitioners of aura reading. Then, roughly speaking, one should believe a (genuine) expert when there are other, equally competent experts that have both an interest and the means to contradict the expert in question and do not do so. But that involves numerous sociological questions: How free is the so-called "free market of ideas"? Do contradictory viewpoints get a fair hearing? Nothing prevents the existence of what one might call "democratic Lysenkos", namely people who, within democratic societies, get hold of some institutional position of power (a scientific journal, a research institute) and impose their favorite "line" of research there, leading to a dead end. In fact, anyone working in a university knows that there are lots of democratic Lysenkos, at least on a small enough scale. A most interesting problem for sociologists (and policy-makers) is to design institutions that minimize the likelihood that such people become too powerful.

When all is said and done, there is no need for a "science war" between scientists and sociologists. Both could perfectly well cooperate on a variety of issues. In our view, Science Studies' epistemological and methodological conceits are a diversion from the important matters that motivated Science Studies in the first place: namely, the social, economic and political roles of science and technology. To be sure, those conceits are not an accident; they have a history, which can itself be subjected to sociological study.[53] But Science Studies practitioners are not obliged to persist in a misguided epistemology; they can give it up, and go on to the serious task of studying science. Perhaps, from the perspective of a few years from now, today's so-called "Science Wars" will turn out to have marked such a turning point.

[52] Nowotny (1979, p. 15).

[53] For an interesting conjecture, see Nanda (1997, pp. 79–80). For a different (but not incompatible) conjecture, see Gross and Levitt (1994, pp. 74, 82–88, 217–233). Both these conjectures merit careful empirical investigation by intellectual historians.

Acknowledgments

We would like to thank Michel Ghins, Shelley Goldstein, Antti Kupiainen, Norm Levitt and Tim Maudlin for many interesting discussions on the issues discussed here. Of course, they are in no way responsible for what we have written.

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