Science Peace: Round 3

Jean Bricmont
Institut de Physique Théorique
Université de Louvain
2, chemin du Cyclotron
B-1348 Louvain-la-Neuve, BELGIUM
Internet: `BRICMONT@FYMA.UCL.AC.BE`
Telephone: 32-10-473277
Fax: 32-10-472414

Alan Sokal
Department of Physics
New York University
4 Washington Place
New York, NY 10003 USA
Internet: `SOKAL@NYU.EDU`
Telephone: 1-212-998-7729
Fax: 1-212-995-4016

November 6, 1999
revised footnote 9 on January 2, 2000

Before addressing the substantive criticisms directed at our essay, it is necessary to set straight a few commentators' misreadings of our arguments.[1]

According to Steven Shapin, "Bricmont and Sokal attribute the cultural credibility of flat-earth or witch-craft beliefs to 'the existence of a radically relativist academic Zeitgeist'." But we did no such thing. Rather, we noted "the existence of a radically relativist academic Zeitgeist" in which some "otherwise reasonable researchers or university professors ... will claim that witches are as real as atoms" — their obvious intent being to cast doubt on the existence of atoms, not to assert a sincere belief in the existence of witches — "or *pretend to have no idea* whether the Earth is flat, blood circulates or the Crusades really took place" [emphasis added]. We were thus discussing extreme *relativism or skepticism* in academia; we made no reference whatsoever to extreme *credulity* in the general non-academic culture, much less did we claim that the latter is a causal consequence of the former.

Jane Gregory says that in our brief discussion of public-policy issues involving science, "Bricmont and Sokal suggest that we need to know how the world really works *in order to determine* whom we should trust" [emphasis added]. But she gets our point exactly backwards. In setting public policy with regard to (for example) BSE, nuclear power or global warming, it is desirable to have an as-accurate-as-possible understanding of the underlying natural phenomena (i.e. how the world really works).[2] But because politicians and citizens frequently have neither the time nor the expertise to evaluate the scientific evidence themselves, they are obliged to do something *second-best*: decide which experts to trust. We have no magic recipe for how best to do this, but we did suggest a few guidelines, based on epistemological and sociological considerations.

Now to the substantive issues, the foremost of which is methodological relativism.[3] Harry Collins begins by setting forth admirably clear definitions, in terms nearly identical to our own, of "ontological relativism" and "epistemological relativism" — any combination of which he calls "philosophical relativism" — and "methodological relativism". In particular, he defines methodological relativism as the injunction that "the sociologist or historian should act as though the beliefs about reality of any competing groups being investigated are not caused by reality itself."[4] But then he

[1] We prefer to pass over without comment the pejorative epithets directed at us: "noisiest guests at the party" (Pinch), "sand-lot philosophy" (Mermin).

[2] It should, we hope, go without saying that even a complete understanding of the scientific facts would not, by itself, suffice to determine *what to do*: policy decisions will inevitably involve political, economic and ethical considerations as well as scientific ones. (In Bayesian terms, it is necessary to specify the utility function and not merely the posterior probability distribution.)

[3] Let us mention in passing our irritation at Trevor Pinch's condescending comment that "methodological relativism [is] a term which *these days* Jean Bricmont and Alan Sokal *have learnt to use*" [emphasis added]. For what it's worth, we've been using this term ever since our earliest essays on the subject — see e.g. Sokal (1998, written in early 1997), Bricmont (1997), and Sokal and Bricmont (1998, first written in French in June 1997) — and we challenge Pinch to find any publication of ours in which we've used any *other* term for this concept.

[4] This definition is plagued, however, by one key ambiguity: Is the investigator being told to act

asserts, misleadingly, that our own "central argument [is] that methodological and philosophical relativism cannot be disentangled." Quite the contrary: we *disentangle* (i.e. distinguish) the two doctrines exactly as Collins does. Our central thesis is, rather, that if the sociologist's aim is to give a causal account of some individual's or group's beliefs — as, for instance, the Strong Programme aspires to do — then methodological relativism cannot be *justified* unless one also adopts philosophical relativism or radical skepticism. Since we have presented the arguments for this thesis in great detail in our essay and again in our commentary on other contributors' essays, we need not repeat the reasoning here. But we do need to set straight Collins' misinterpretation of one of our arguments against methodological relativism, namely the *reductio ad absurdum* concerning the inverse-square law.[5] Collins portrays our thought-experiment as an exercise in counterfactual reasoning: "arguments based on how things would have turned out if we could have altered some aspect of the past".[6] But we did *not* propose to alter any aspect of the past; rather, we proposed an experiment that could in principle (albeit probably not in practice) be carried out *today or at some time in the future*. The question is not what *would have* happened in the seventeenth century if no information about planetary movements *had been* available; it is whether one could conceivably explain what *did* happen (as well as what *did not* happen) without making reference to the information about planetary movements that *was* available in the seventeenth century.[7,8]

---

as though the beliefs are not caused, *even in part*, by reality itself? Or merely to act as though the beliefs are not caused *solely* by reality itself? The distinction between the two doctrines is crucial, for as we have argued in our essay, the first doctrine is untenable (unless one adopts philosophical relativism or radical skepticism) while the second is eminently sensible. Collins' footnote 1 suggests that he may intend the first interpretation: "I set out this view [methodological relativism] most clearly in a paper published in 1981, in which I said: '. . . the tenet of symmetry implies that we must treat the natural world as though it in no way constrains what is believed to be.' " However, even this latter formulation is ambiguous, for it depends on how one interprets the verb "constrains".

[5] Let us note, in passing, that we gave *three* versions of the argument against methodological relativism — "it is raining today", Newtonian mechanics, and the *reductio ad absurdum* concerning the inverse-square law — and Collins ignored entirely the first two.

[6] We do not wish to enter here into the delicate debate concerning the extent to which counterfactual reasoning is legitimate in historical inquiry. But we note with amusement that Peter Dear advocates precisely the type of counterfactual history that Collins rejects.

[7] Of course, it can be argued that any historical "explanation" necessarily relies (implicitly or explicitly) on some causal theory and that any causal theory necessarily entails numerous counterfactual assertions. To the extent that these theses are valid, our thought-experiment *would* implicitly involve counterfactual reasoning; but then Collins' objection to such reasoning would be without force.

[8] Let us note in passing that, *pace* Collins, we do *not* assert that belief in astrology can be explained in purely sociological terms, nor that it could be explained "without reference to the movement of the planets". We say only that "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" [emphasis added]. Obviously any valid explanation of belief in astrology would have to make reference to the

Nor did we "pick an easy case", as Collins asserts, by selecting an example in which the underlying scientific controversy was settled long ago; the reasoning would be identical were one to select a currently raging controversy such as global warming. The question is: Could one conceivably explain scientists' beliefs about the Earth's climate without making *any* reference to the currently available *evidence* concerning the Earth's climate?[9]

David Mermin also misunderstands us on this issue: he asserts that we "often ...

---

movement of the planets, if only because those movements play a central role in astrological beliefs; all we assert is that the explanation would make no reference to the alleged *causal processes* by which the planets influence human life (according to astrological doctrine), for the reason that those causal processes are, in our best rational judgment, nonexistent. Note also our observation 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."

[9]Collins is perhaps confusing this issue with a *different* one in which the distinction between long-settled science and unsettled controversies *is* relevant, namely: how the sociologist or historian of science should act when studying controversies on which he/she does not have the scientific competence to make an independent assessment of whether the experimental/observational data do in fact warrant the conclusions the scientific community drew from them. (This situation is particularly likely to arise when the sociologist is studying contemporary science, since in this case there is no other scientific community besides the one under study that could be relied on to provide such an independent assessment. By contrast, for studies of the distant past, one can take advantage of what subsequent scientists learned.) In such a situation, the sociologist will be understandably reluctant to say that "the scientific community under study came to conclusion X because X is the way the world really is" — *even if it is in fact the case* that X is the way the world is and that is the reason the scientists came to believe it — because the sociologist has no independent *grounds to believe* that X is the way the world really is other than the fact that the scientific community under study came to believe it. Of course, the sensible conclusion to draw from this *cul de sac*, it seems to us, is that sociologists of scientific knowledge should abstain from studying scientific controversies on which they (together with their scientist collaborators, if any) lack the competence to make an independent assessment of the scientific evidence, if there is no other (for example, historically later) scientific community on which they could justifiably rely for such an independent assessment.

(To forestall any possible misunderstandings, let us stress that we are *not* telling sociologists that they must refrain from studying contemporary scientific controversies. We are only saying that *if* they aspire to study the substantive content of scientific controversies (and not merely the social structures of the scientific community), and *if* they want these studies to be logically sound, *then* they cannot avoid making an independent assessment of whether the experimental/observational data do in fact warrant the conclusions that various scientists drew from them; moreover, the sociological conclusions of their study will be only as reliable as this independent assessment of the scientific evidence. For it is not enough to study the alliances or power relationships between scientists, or their deployment of rhetoric, important though these aspects may be: what appears to a sociologist as a pure power game may in fact be motivated by perfectly rational considerations which, however, can be understood as such only through a detailed understanding of the scientific theories and experiments. Consequently, sociologists or historians who aspire to analyze the substantive content of scientific controversies need to possess — or acquire — the scientific competence to make an independent assessment of the evidence, or else work together with scientist collaborators on whom they can rely for such an assessment. Of course, many historians of science, and some sociologists of science, *do* possess this competence in the scientific subfields they study; and many others are capable of acquiring enough scientific competence to collaborate fruitfully with scientists in interdisciplinary teams. We thank Harry Collins and Jay Labinger for fruitful misunderstandings that spurred us to write this clarification.)

explain the emergence of truth by reference to its truthfulness". But that mischaracterizes our reasoning, which is to make the elementary observations that (a) in at least some cases, people believe statements in part because of *evidence* that those statements are at least approximately true; and (b) the existence of such evidence is often causally linked to the fact that the statement *is* at least approximately true. Moreover, in many cases, *after* one has obtained strong evidence for the (approximate) truth of the underlying proposition, one is also enabled to understand, at least in part, the causal processes at work in (b).[10]

Collins summarizes our view, albeit a bit crudely, by saying that beliefs[11] can be divided roughly into three classes: "Type 1, where the current consensus is overwhelmingly dominated by the natural world ...; Type 2, where the outcome is overwhelmingly dominated by the social world ...; and Type 3, perhaps live controversies, where there is a mixture of both causes."[12] And he correctly states our view that "even in the case of Type 3 science, methodological relativism is untenable [unless one also adopts philosophical relativism or radical skepticism] because if you know there is some contribution from the natural world you must include it". But then he purports to refute our view by *reductio ad absurdum*, as follows:

> Thus, in Type 3 cases there are two types of cause. Therefore in Type 3 passages of science, explanatory papers must invoke both kinds of cause if they are sincere and free from error. This means that papers in Type 3 areas of science, written by scientists and published in a scientific journal, that did not mention the social factors that contributed to the scientists' beliefs would be flawed.

But this manifestly confuses papers whose goal is to present arguments for or against the underlying scientific propositions, with papers whose goal is to explain scientists' beliefs. Most papers published in scientific journals are of the former type, and it would be silly for them to drag in social factors that can have no causal effect on the scientific phenomena under discussion.[13]

---

[10] Thus, our belief that the Earth is approximately spherical is due in part to the fact that it *is* approximately spherical: for if it were (for example) flat or tetrahedral, present-day observation techniques would allow us to know that.

[11] We use the neutral word "belief" instead of the word "science" employed by Collins, because some of the beliefs in question (e.g. astrology) cannot, in our view, be properly characterized as being "science" (to put it mildly). Let us hasten to add that we do *not* assert the existence of a sharp demarcation between "science" and "non-science", much less one based on rigid criteria such as those proposed by Popper or the logical positivists; rather, we make a case-by-case evaluation of the extent to which the beliefs in question are based on a rational evaluation of empirical evidence, and we note that astrology is radically different in this respect from the mainstream sciences.

[12] Collins' summary of our view is too crude because (a) he draws too sharp a distinction between Types 1 and 3, (b) he oversimplifies the complex *interaction* between natural and social factors in Type 3, which cannot be reduced to a mere "mixture of causes", and (c) in Types 2 and 3 it is necessary to include also psychological and biological factors.

[13] On the other hand, scientific journals do occasionally publish papers of the latter type (e.g. some

Collins offers a second purported *reductio ad absurdum* of our view: he notes that the inverse-square law would not likely become accepted

> unless humans had brains of a certain size and unless civilization had not been destroyed by meteor impact prior to the law's discovery. Yet the study of the origins of our belief in the inverse-square law does not have to involve either human anatomy or terrestrial catastrophes.

But Collins forgets that our thought-experiment asked one to explain why seventeenth-century English physicists came to believe that the gravitational force decays with the inverse square of the distance, *rather than the inverse cube*. The non-destruction of the human race and a certain minimal size of the human brain are necessary preconditions for humans' belief in *either* the inverse-square *or* the inverse-cube law; they have no *differential* explanatory value, and thus need not be included in the requested explanation.[14]

Finally, Collins misstates Hume's advice on miracles as "do not believe anything 'miraculous' unless you see it with your own eyes". The correct formulation of Hume's (and our) view is rather: do not believe anything "miraculous" unless you see it with your own eyes *or* the proponents offer you reasons to believe the "miraculous" claim that are more compelling than the alternative explanations of fraud or self-deception.[15] Thus, as Mermin notes in his essay in this volume, "I have never been to China. I can nevertheless assure people that such a place exists, because the conspiracy necessary to fool me into believing it, were it false, is too implausible to contemplate." Similarly, though neither of us has personally witnessed the experimental measurement of the magnetic moment of the electron — or, for that matter, personally carried out the order-α^3 theoretical calculation — we are convinced by the truly "miraculous" agreement between theory and experiment[16]

$$\begin{array}{rl} \text{Theory:} & 1.001\,159\,652\,201 \pm 0.000\,000\,000\,030 \\ \text{Experiment:} & 1.001\,159\,652\,188 \pm 0.000\,000\,000\,004 \end{array}$$

that quantum electrodynamics must be saying *something* at least approximately true about the world[17]; our judgment that this explanation is more likely than fraud

historical review articles); and such papers *should* bring in social factors to the extent that they are causally relevant.

[14]The structure of the human brain *would* be relevant if it were being argued that humans have an *innate* predisposition (e.g. one sculpted by natural selection) to believe in the inverse-square law rather than the inverse-cube. This is, of course, unlikely in the case at hand, but it could well be relevant to some other scientific theories, e.g. to humans' predisposition to believe in Euclidean rather than Lobachevskian geometry.

[15]Of course, one should also be duly skeptical of "miracles" observed with one's own eyes, and take account of the possibility of faulty perception, misinterpretation, etc.

[16]See Kinoshita (1995) for the theory, and Van Dyck *et al.* (1987) for the experiment. Crane (1968) provides a non-technical introduction to this problem.

[17]In a sense of the phrase "approximately true" that does *not* require the approximately true theory to have the correct *fundamental* ontology.

or self-deception results from a web of reasons that are both scientific (the experiments we *have* witnessed, the calculations we *have* performed) and sociological (our understanding of the scientific community's epistemology, methodology and social structure).[18]

Peter Saulson is disappointed that our essay concentrated (as we made clear at the beginning that it would) on SSK's declared methodology rather than on its concrete case studies.[19] All we can say is, *à chacun son goût*: both are legitimate objects of analysis.[20] In particular, since the "new" sociology of science distinguished itself from its Mertonian predecessors precisely by trumpeting its own radical methodology (embodied notably in the symmetry principle), it is of especial interest to analyze critically that proclaimed methodology.

Saulson does make one valid criticism: he notes that our analysis of the strong programme's methodological precepts was incomplete because we omitted to address Bloor's statement in Principle 1 that "Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief." This question was addressed in our book, where we observed that "the trouble is that he [Bloor] fails to make explicit *in what way* natural causes will be allowed to enter into the explanation of belief, or what precisely will be left of the symmetry principle if natural causes are taken seriously."[21] It is ironic to note that Bloor's position on the causal role of the natural world has been criticized, in terms far harsher than our own, by Bruno Latour, who sees it as mere lip-service aimed at fending off the accusation of idealism: "[A]re these objects allowed to *make a difference* in our thinking about

[18] Of course, to spell out in detail the reasoning underlying this judgment would be rather lengthy. See e.g. Haack (1998, Chapters 5 and 6) for a brief sketch of some of the factors that would enter.

[19] Collins also finds our choice disappointing, especially inasmuch as we acknowledged that "interesting work may have been done" in some of the concrete studies carried out by SSK practitioners. And Collins goes on to compare us to people who would claim that "though quantum theory has come up with useful results, the underlying indeterminacy is impossible to accept and ... quantum theorists should go back to doing sensible classical physics". But this comparison is ludicrous. First of all, when we said that "interesting work may have been done" in some of the empirical studies associated with the strong programme, we never meant to concede (and certainly do not believe) that those studies enjoy the same level of empirical support as does quantum mechanics. (Did we perhaps overlook some recent successes in SSK comparable to the 11-decimal-place agreement between theory and experiment achieved by quantum electrodynamics?) Secondly, for the reasons explained in our essay, nothing in those empirical studies justifies methodological relativism (much less philosophical relativism, as Collins used to think: how could any empirical study justify a philosophy that basically says that empirical studies never allow us to discover objective truths?). Finally — though this is a much subtler point, which we cannot address in detail here — even in the case of quantum mechanics, it is far from obvious that its empirical successes warrant the philosophical claims usually associated with the Copenhagen interpretation.

[20] Perhaps the difference in taste arises from the fact that we are theoretical physicists, while Saulson is an experimentalist. See Gingras (1995) for a critical analysis of several recent tendencies in SSK, focussing on concrete case studies; and see also some of the essays in Koertge (1998).

[21] Sokal and Bricmont (1998, footnote 115, italics in the original). See Laudan (1981) and Slezak (1994) for more detailed analyses of Bloor's ambiguities.

them? The answer given by David [Bloor] and repeated over and over again by all the descendants of this tradition — even empirically minded ones such as Shapin, Schaffer and Collins — is a resounding 'no'."[22] Finally, although Bloor is vague about the role that the natural world should play in sociologists' explanations of scientists' beliefs, other SSK practitioners (such as Harry Collins) are explicit in their rejection of such a role, as discussed above.

Concerning redefinitions of truth in terms of intersubjective agreement, we argued in our essay that

> 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 (understood intuitively).

Mermin criticizes this argument by saying that

> The fact that "once upon a time, people agreed that the Earth was flat and we now know that they were wrong" does not establish that there is more to objectivity than intersubjectivity. All it shows is that in this instance the inference of the objective from the intersubjective was inadequately based and badly drawn.

But Mermin's formulation implicitly concedes the point that we were trying to make: for if it is possible to make an inadequately based inference from A to B, then clearly B cannot be *identical* to A.[23]

Mermin says that we "exaggerate" when we attribute to Barnes, Bloor and Henry (BBH) "a rather tolerant (or even favorable) attitude" towards astrology, but he gives no evidence to support this charge.[24] He further protests that his Humean

[22] Latour (1999, p. 117, italics in the original). Let us stress that we do not agree with most of Latour's criticisms of the Edinburgh school.

Let us also note in passing the double standard upheld by Peter Dear, who defends Latour's criticisms on the grounds that "such observations take us inside Science Studies, rather than conforming to caricatures of Science Studies drawn by unengaged critics" — and this even though our own "caricature" of Bloor's views is rather more charitable than Latour's "inside" critique.

[23] Of course, it could turn out that B is *logically equivalent* to A, by virtue of a deductive argument of which the people in question are unaware. But that possibility is not relevant to the case at hand, in which A (intersubjective agreement that the Earth is flat) is true while B (flatness of the Earth) is false.

Note also that Mermin's use of the word "objective" confuses two distinct questions: whether the belief in question is (objectively) *true*, and whether it is (objectively) *rationally justified* with respect to the available evidence. We contend that *neither* truth nor rational justification is equivalent to intersubjective agreement, but different arguments are needed to establish the two claims. (In particular, the flat-Earth example does *not* suffice to prove that rational justification is different from intersubjective agreement, since the belief in a flat Earth may well have been rationally justified in all those places and times where it was widely accepted.)

[24] Indeed, Mermin himself cites, in his review of the BBH book (Mermin 1998a, pp. 621–622), the relevant passage:

> Astrology ... and homeopathy ... remain firmly saddled with the label of pseudo-

argument for the implausibility of Gauquelin's alleged data was not intended "to refute an argument for astrology, but [only] to reply to Bloor's remark that their [BBH's] point was well illustrated by my own unwillingness to take off a year or two attempting to duplicate the astonishing data." We accept Mermin's statement of his intentions, but his reasoning nevertheless shows that astrology is so unlikely to be true that (a) it's not worth believing in astrology unless its advocates come up with vastly more convincing empirical evidence, and (b) it's not worth taking off a year or two to study the evidence unless astrology's advocates come up with vastly more convincing empirical evidence. Perhaps Mermin only intended to assert (b), but the same "Humean" reasoning leads to both (a) and (b).

An even starker example of a sociologist's favorable attitude toward a pseudo-science — in this case homeopathy — is provided by Jane Gregory's characterization of Benveniste's claims about the memory of water as "inconvenient truths". In order to illustrate why we (and most scientists) take a vastly more skeptical attitude toward homeopathy and astrology than do some sociologists, we would like to discuss critically some of Gregory's assertions.

Gregory's central claim is that "In science, replications, peer-review and publication in *Nature* are usually good enough: the end-product is usually well on its way to becoming what Bricmont and Sokal might call 'reality' or 'truth'." To begin with, this grossly misunderstands what we mean by "truth": as we explained at length in our essay, "truth" signifies for us "correspondence with reality"; it thus makes no sense to say that an assertion *becomes* true through replication, peer-review and publication. But more importantly, while "replications, peer-review and publication in *Nature*" can constitute *evidence* (sometimes strong evidence) for the truth of a scientific claim, they are by no means conclusive, nor are they the sole criteria for judging a claim. And when the claim concerns the alleged "memory of water", even a few replications, peer-reviewed and duly published in *Nature*, do not suffice to overcome our rational skepticism. Why not? The crux of the matter is not, as Gregory seems to think, that Benveniste's claim is an "inconvenient truth" that threatens to "mak[e] redundant the pharmaceutical industry"; it is rather that everything we know about physics and chemistry renders this claim so improbable that an overwhelming quantity and

---

> sciences in spite of recent work which seems to some to call for a reassessment (Gauquelin, 1984; Benveniste, 1988).
>
> Michel Gauquelin's statistical evidence in support of astrology would perhaps be a serious embarrassment to scientists if they were not so good at ignoring it. But one day it could conceivably come to be accommodated as a triumph of the scientific method. Gauquelin's work seems to imply the existence of forces and interactions unrecognized by current scientific theory and yet it is based on methodological principles and empirical evidence which have so far stood up to sceptical challenge. (Barnes, Bloor and Henry 1996, p. 141)

Though this passage does not indicate unequivocal support for astrology, it does demonstrate a tolerant (and even cautiously favorable) attitude towards astrology, as well as a failure to comprehend the vast gulf between the established natural sciences and astrology as regards both methodology and degree of empirical confirmation. See also note 28 below.

quality of evidence would be needed in order to make it believable. This attitude — namely, "the more implausible the claim, the stronger the evidence needed to justify it" — is of course common sense, but it is not for that reason any less valid.[25] Indeed, all human beings necessarily proceed in this way: while one or two witnesses might suffice to convince Gregory that Baroness Thatcher dined last night with General Pinochet, even a hundred witnesses claiming to have seen Thatcher dine in a flying saucer would be taken with a grain of salt. Likewise — though it would be too long to explain the reasoning in detail here — the idea that substances may have therapeutic effects (other than the placebo effect) even when they are "diluted" so much that not a single molecule of the original substance remains in the final product, *does* run counter to all modern physics and chemistry (based, as they are, on the atomic theory of matter).

One may then ask which is more probable: that a peer-reviewed paper published in *Nature* is wrong, or that the whole edifice of modern physics and chemistry is badly flawed? And the sensible response is again provided by Hume's argument against belief in miracles. We know, from direct experience, that articles published in respectable scientific journals (such as *Nature*) can be wrong. But we have no evidence at all in favor of the claims made by homeopathy[26], and rather heavy evidence against it (namely, all the experimental evidence confirming modern physics and chemistry).

Of course, science is not a religion[27]: its claims, even the best-established ones, are in principle revisable, so that the "memory of water" could turn out to be a real effect after all (and due to hitherto unknown causes). But this remark applies to all pseudoscientific claims, even astrology.[28] Moreover, all the pseudosciences are sometimes supported by alleged spectacular discoveries in their favor (Benveniste, Gauquelin, etc.). But if we adopt a non-discriminatory attitude with respect to all similar claims — faith healing, New Age medicine, and the lot — how are we supposed to proceed? There is, after all, a vast number of such theories (especially if, following our non-discriminatory policy, we include the traditional beliefs of other cultures). How much time and effort should one invest checking each of those claims? The most

[25]For what it's worth, this precept can also be justified by an elementary Bayesian calculation.

[26]Not even in favor of what Jane Gregory calls "the potency of homeopathic remedies", since, to our knowledge, that alleged "potency" has never been established by means that could convince a reasonable skeptic, e.g. through double-blind experiments.

[27]We therefore cannot share Peter Saulson's avowed belief in "the Church of Science". For further discussion of the radical methodological opposition between science and religion, see Bricmont (1999).

[28]However, as Mermin observed,

> BBH's gloss on astrology — 'the existence of forces and interactions unrecognized by current scientific theory' (BBH, 141) — fails adequately to convey the truly spectacular degree to which compelling evidence in support of astrology would require a massive radical reconstruction of our current understanding of the world. (Mermin 1998b, p. 642)

A similar remark can be made for homeopathic claims (although the reconstruction might be less radical in this case).

reasonable attitude, it seems to us, is to do what most scientists do: namely, keep a skeptical eye on those "miraculous" claims, wait and see if some really convincing evidence turns up some day, and then, if it does, find out what has to be revised in the standard scientific world-view.

To say, as Collins does in the context of our discussion of Benveniste's claims, that for us "replication of observations is the key" is not quite right. In the case of highly implausible claims such as the memory of water, even a few replications are insufficient; Hume's argument still applies. Mermin said it well:

> An important motive behind rejecting such claims without any attempt at replication, unmentioned by BBH but clearly recognized by those doing the rejecting, is the gross inefficiency of investing extensive time and resources in an attempt to refute overwhelmingly improbable claims. For similar reasons, one turns down an offer, rendered on the spot, to purchase the Brooklyn Bridge for five dollars, without making a trip to the courthouse to confirm the conjectured non-existence of the claimed deed of ownership.[29,30]

Finally, though in this reply we have necessarily focussed on disagreements (and on misrepresentations of our views), we wish to conclude by noting briefly some important points of agreement with other contributors:

- We share Shapin's equal distaste for ignorance (or impermeability to empirical evidence) in biology and in history.
- We applaud Lynch's disapproval of *ad hominem* argument.
- We second Pinch's observation on the diversity of views in this debate, which are by no means determined by one's profession.
- We agree with Saulson that "Outside observers with a background in understanding social life clearly have a primary role to play in elucidating the function of science as a social system, embedded in a larger society."[31]

[29] Mermin (1998b, p. 642).

[30] We do, however, wish to apologize to Collins and Pinch for misunderstanding their assertion that "if homeopathy cannot be demonstrated experimentally, it is up to scientists, who know the risks of frontier research, to show why" (Collins and Pinch 1993, p. 144). We accept their assurances that they were not defending homeopathy or attempting to shift the burden of proof away from homeopathy's advocates.

[31] However, we disagree with Saulson's next paragraph, where he asserts that "many scientists are prepared to dispute" this view, without citing a single example. For what it's worth, we have repeatedly stressed our belief in the value of an intellectually rigorous sociology of science. Indeed, even the reputed "extremists" Gross and Levitt stated in no uncertain terms that

> Natural scientists ... do not feel that their particular expertise in some area of science automatically endows them with insight into the human phenomenology of scientific practice, or that familiarity with the recent results and the liveliest questions of their specialty qualifies them to pronounce on its evolution as that relates to the course of

- Last but not least, we concur with Mermin that "no reliable body of knowledge can be undermined by viewing its acquisition as a collective human activity", and with Dear that "a detailed investigation of the socio-historical conditions that brought about a belief in some scientific truth-claim does not in itself undermine that claim".

Bibliography

Barnes, Barry, David Bloor and John Henry. 1996. *Scientific Knowledge: A Sociological Analysis*. Chicago: University of Chicago Press.

Bricmont, Jean. 1997. "Science studies – what's wrong?" *Physics World* **10**(12) (December): 15–16.

Bricmont, Jean. 1999. "Science et religion: l'irréductible antagonisme". To appear in: *Où va Dieu?*, edited by Antoine Pickels and Jacques Sojcher. Revue de l'Université de Bruxelles, Éditions Complexe.

Collins, Harry and Trevor Pinch. 1993. *The Golem: What Everyone Should Know about Science*. Cambridge: Cambridge University Press.

Crane, H. R. 1968. "The g factor of the electron". *Scientific American* **218**(1) (January): 72–85.

Gingras, Yves. 1995. "Un air de radicalisme: Sur quelques tendances récentes en sociologie de la science et de la technologie". *Actes de la Recherche en Sciences Sociales* **108**: 3–17.

Gross, Paul R. and Norman Levitt. 1994 (2^{nd} ed. 1998). *Higher Superstition: The Academic Left and its Quarrels with Science*. Baltimore: Johns Hopkins University Press.

Haack, Susan. 1998. *Manifesto of a Passionate Moderate: Unfashionable Essays.* Chicago: University of Chicago Press.

Kinoshita, Toichiro. 1995. "New value of the α^3 electron anomalous magnetic moment". *Physical Review Letters* **75**: 4728–4731.

human development. Apart from the most arrogant, they concede that the psychological quirks and modes of personal interaction characteristic of working scientists are not entitled to special immunity from the scrutiny of social science. If bricklayers or insurance salesmen are to be the objects of vocational studies by academics, there is no reason why mathematicians or molecular biologists shouldn't sit still for the same treatment. (Gross and Levitt 1994, p. 42)

Koertge, Noretta, ed. 1998. *A House Built on Sand: Exposing Postmodernist Myths About Science*. New York: Oxford University Press.

Latour, Bruno. 1999. "For David Bloor... and beyond: A reply to David Bloor's 'Anti-Latour' ". *Studies in History and Philosophy of Science* **30**: 113–129.

Laudan, Larry. 1981. "The pseudo-science of science?" *Philosophy of the Social Sciences* **11**: 173–198.

Mermin, N. David. 1998a. "The science of science: A physicist reads Barnes, Bloor and Henry". *Social Studies of Science* **28**: 603–623.

Mermin, N. David. 1998b. "Abandoning preconceptions: Reply to Bloor and Barnes", *Social Studies of Science* **28**: 641–647.

Slezak, Peter. 1994. "A second look at David Bloor's *Knowledge and Social Imagery*". *Philosophy of the Social Sciences* **24**: 336–361.

Sokal, Alan. 1998. "What the *Social Text* affair does and does not prove". In: *A House Built on Sand: Exposing Postmodernist Myths About Science*, edited by Noretta Koertge, pp. 9–22. New York: Oxford University Press.

Sokal, Alan and Jean Bricmont. 1998. *Intellectual Impostures: Postmodern Philosophers' Abuse of Science*. London: Profile Books. Published in the US and Canada under the title *Fashionable Nonsense: Postmodern Intellectuals' Abuse of Science*. New York: Picador USA. [Originally published in French under the title *Impostures intellectuelles*. Paris: Odile Jacob, 1997. 2^{nd} ed. Paris: Livre de Poche, 1999.]

Van Dyck, Robert S., Jr., Paul B. Schwinberg and Hans G. Dehmelt. 1987. "New high-precision comparison of electron and positron g factors". *Physical Review Letters* **59**: 26–29.