

# Science Peace: Replies

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There seem to be two main themes in the contributions of social scientists to this volume, and we would like to briefly discuss both. First of all, it is claimed that people on the “science side of the science war” (needless to say, we reject such labels, but that is how we are viewed anyway) miss the point in their criticism of SSK: instead of advocating philosophical relativism, their practitioners rely only on methodological relativism.<sup>1</sup> The second point is that it is a mistake for scientists to think that SSK people are anti-science or irrationalists, and here the comments follow two different lines: either it is emphasized that there are far deeper and more dangerous forms of irrationalism in our society, or it is argued that a better (sociological) understanding of how science works will lead the public to a more positive view towards science.<sup>2</sup> Let us discuss these points one by one.

Social scientists who think that we misunderstand them on the issue of relativism actually misunderstand what we say (irrespective of what other “scientific science warriors” may have said). We take pains (both in our contribution to this volume and in our book) to distinguish philosophical relativism from methodological relativism, and we realize that supporters of the strong programme often mean to defend only the latter.<sup>3</sup> But our criticism proceeds in two steps: First, we observe that it makes no sense for scientists (whether natural or social) to hold philosophical views such as radical skepticism or radical relativism. But then, we argue that methodological relativism is also untenable *unless* one adheres to some kind of radical skepticism.<sup>4</sup>

Since this latter point seems to be remarkably difficult to get across, let us argue it again using an example proposed by Harry Collins in the present volume. Collins illustrates methodological relativism by explaining how a sociologist reacts to the fact that “south of a certain line in a certain country it is generally believed that a certain kind of religious ceremony can turn wine into blood, whereas north of that line it is generally believed that no such thing occurs”. He emphasizes that the sociologist, in his explanation, should *not* take into account the matter of whether the wine really did turn into blood.

But why is that true? We know that this attitude is common among sociologists, but we nevertheless fail to see any valid reason for holding it. Suppose that wine really did turn into blood (of course, we know that it does not, but let us just imagine that it does, for the sake of the discussion). And suppose further that people south of the line had discovered solid empirical evidence in support of this (true) claim. In that

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<sup>1</sup>See, for example, the contributions to this volume of Harry Collins, Peter Dear, Trevor Pinch and Steven Shapin.

<sup>2</sup>See, for example, the contributions of Jay Labinger, Trevor Pinch, and Jane Gregory/Steve Miller.

<sup>3</sup>We also point out, although it is a somewhat secondary issue, that many statements in favor of methodological relativism are so poorly stated (e.g. Latour’s Third Rule of Method or the initial formulations of the strong programme) that they appear to support philosophical relativism (or radical skepticism) and not merely methodological relativism.

<sup>4</sup>This point is also well explained in Steve Weinberg’s contribution to this volume.

case, people south of the line would simply be rational in holding their belief.<sup>5</sup> Of course, the sociologist would still have to explain how they discovered the process for transforming wine into blood, and evidently a lot of social factors are at work there. But the explanation would be very different from what it should be if one assumes (as we do) that wine does not in fact turn into blood.<sup>6,7</sup>

Sociologists frequently admit that they don't have the background to evaluate whether the claims made by scientists (particularly concerning contemporary research) are rationally justified or not, but then assert that they are not obliged to make any such evaluation: they are concerned with social phenomena, not with physical or biological ones, and so are perfectly justified in ignoring this latter aspect. That would perhaps be fine if their aims were more modest than those of the strong programme: if, for example, they claimed only to recount *some* of the factors affecting the acceptance of scientific beliefs, without purporting to judge their relative importance. But in that case they ought not claim to give a causal account of the acceptance of scientific beliefs, when important parts of the cause — usually the dominant parts, in our view — are excluded *a priori* from consideration.<sup>8</sup>

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<sup>5</sup>More precisely, they *could* be rational in holding this belief. It is also possible, of course, that they hold this belief primarily or entirely for reasons of dogma or tradition. Their actual motives for holding the belief need to be investigated empirically.

<sup>6</sup>What creates some confusion in this example is that the people north of the line may reject the transubstantiation dogma for reasons that are no more rational than those of the people south of the line who believe in it: they may simply have another religion. In that case, of course, a symmetrical explanation might be justified. (But that too is open to discussion: after all, even among religious beliefs, some are more rational than others. Thus, the people north of the line may reject the transubstantiation dogma simply because they, like Collins and us, are rational people and there is no evidence that wine can turn into blood.)

<sup>7</sup>A more sophisticated version of methodological relativism is set forth by Peter Dear in this volume, who begins by observing that “people do not believe propositions to be true *because* those propositions are in fact true” (emphasis his). Of course, people do not believe statements *solely* because they are true; rather, they believe them (at least in some cases) because of *evidence* that they are true (or at least approximately true). But — and this is the important point glossed over by Dear — the existence of such evidence is often causally linked to the fact that the proposition *is* (approximately) true. Thus, our belief that the Earth is (approximately) round is due in part to the fact that it *is* (approximately) round: for if it were (for example) flat or tetrahedral, current observation techniques would allow us to know that.

Dear also observes, correctly, that “whatever the evidence and arguments might be, they would count as at least part of the explanation for my belief, regardless of their plausibility to other people.” Nevertheless, our evaluation of the plausibility of the arguments *is* relevant to understanding the causal processes leading to the belief: this is apparent from the example of the transubstantiation dogma, as well as from the simpler example (“it is raining today”) offered in our own article.

<sup>8</sup>In passing, let us observe that imposing strict *a priori* rules on the kind of information allowed to be used in scientific research is radically alien to the methods of the natural sciences. We cannot imagine a chemist or a biologist saying: “Look, no matter what, I am never going to use information coming from physics.” He may think that this kind of information is unlikely to be relevant to his work or is going to be too difficult to understand, but these are practical considerations, not matters of principle. Truth is hard enough to find without encumbering oneself with arbitrary methodological limitations.

Jay Labinger addresses this issue explicitly:

One frequently voiced objection is that these studies are at best incomplete: by focusing on the social to the exclusion of the natural world they produce severely distorted pictures . . . [But] one typical strategy of scientific experimentation is isolation of variables: determining the effect of changing one while holding others constant.

So far so good, but then Labinger goes on to assert that

The sociologist’s practice of “bracketing out” the “correct” results in carrying out his case study can arguably be viewed as a close analog of that practice.

Absolutely wrong! The sociologist’s practice corresponds to *ignoring* some relevant variables (i.e. the strength of the empirical evidence), not to holding them constant. One can’t hold constant a variable that one refuses to measure.<sup>9</sup>

Of course, our objections to the strong programme’s methodological relativism can be circumvented if one asserts that no belief is *ever* objectively more rational than another (and here we mean *really* rational, not merely “rational relative to some socially determined rules”).<sup>10</sup> But this then brings us back to radical philosophical skepticism or relativism. That is why we say that methodological relativism is untenable *unless* one adheres to philosophical relativism.

When all is said and done, if the strong programme had been called the weak programme — emphasizing the limitations intrinsic to the sociological perspective — we would probably never have had any quarrel with it.

Turning to the issue of whether SSK is “anti-science” or hurts the public image of science, we’ll just make some brief remarks. First of all, we do not judge ideas according to their real or alleged “consequences”, nor according to the real or alleged motivations of their advocates or detractors, nor according to the real or alleged moral worth of the social groups in which they are popular or unpopular. Our objection to the strong programme is that we think it is philosophically and methodologically misguided, not that it is harmful to the public image of science.<sup>11</sup>

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<sup>9</sup>The sociologist’s practice is analogous to investigating the effect of religious beliefs on the incidence of lung cancer, without bothering to ask whether the subjects are smokers or non-smokers.

<sup>10</sup>More precisely, we are referring here to epistemic rationality (what one should do if one wants to find out what is true about the world). This is to be distinguished from practical rationality (what one should do in order to achieve some specified practical goal).

<sup>11</sup>In reading the social scientists’ contributions to this volume, we were struck by the recurrence of emotive words: “threat” and “hostility” (Labinger), “undermining” (Pinch), “anti-scientific” (Shapin). Suffice it to say that we have never attacked the alleged motivations of practitioners of SSK, nor have we used overly vague accusation of being “anti-scientific”. Rather, we have made precise philosophical and methodological criticisms.

Let us also mention, in passing, that we fail to see what the list of “contentious and provocative metascientific claims” cleverly concocted by Steven Shapin proves. Either it shows that one can misrepresent people’s views by quoting an isolated sentence out of context, or that scientists (even famous ones) sometimes express their views in a rather confused way — things we never doubted. But

Of course, the reader might wonder why two obscure physicists ever got to worry about the views held by a fraction of the people working in a subdiscipline of the social sciences called SSK. As we explained in the introduction to our essay, we gradually came into contact with the relativist zeitgeist that is widespread in certain academic circles, where it is taken for granted that science is merely one “narration” among many others, with no claim to objective validity; indeed, this idea is considered so well established that it need not even be argued anymore.<sup>12</sup> And when we asked for the basis of this remarkable discovery, we were led to what we called the “usual suspects”: Kuhn, Feyerabend, Rorty and the strong programme (among many other sources, of course). So we went back and looked at the (alleged) sources. What we found was a *mélange*: some statements are clearly false, others are ambiguous, still others are reasonable but rather banal; and finally, there is a lot of concrete empirical work, on which we make no judgment.

Practitioners of SSK frequently emphasize that they really love science, and simply want to give the public a better understanding of how science really works. We applaud that goal, but argue that SSK’s sociological reductionism offers a grossly distorted view of the scientific enterprise. Of course, SSK cannot be held responsible for the irrationality prevailing in the general society, which extends from religious fundamentalism to New Age<sup>13</sup> and is obviously far worse than anything we might complain about in SSK. Moreover, SSK’s influence probably does not extend much outside academic circles (though it does have some influence on elementary and secondary education). Nevertheless, an intellectual movement that claims to give a causal explanation of the content of scientific theories by treating them, for methodological purposes, just as if they were a religion or a myth cannot be expected to be of much help in fighting the irrationalism prevalent in our societies (to put it mildly).

Our reaction to SSK might be criticized by attributing to us a slightly elitist and old-fashioned view, namely that the universities should be strongholds of scientific rationalism in the middle of the “ocean of insanity upon which the little barque of human reason insecurely floats”, as Bertrand Russell once said in another context.<sup>14</sup> But if that is the case, so be it.

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we do not think that we misrepresent the views of a large group of practitioners of SSK when we say that they defend methodological relativism (which is what we criticize them for), and we don’t see why confused statements made by physicists or biologists do anything to justify confused statements made by sociologists.

<sup>12</sup>Of course, it is possible that the people in question are not *really* radical relativists, but simply express themselves unclearly. If so, their ambiguities are quite serious.

<sup>13</sup>According to recent polls, 47% of Americans believe in the literal validity of the creation account of Genesis, 36% in telepathy, 25% in astrology, 11% in channeling, and 7% in the healing power of pyramids. See Sokal and Bricmont (1998, Appendix C, note 17) for details and original sources.

<sup>14</sup>Russell, 1949.

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