What's Wrong With Social Studies of Science?

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Abstract

This paper discusses two features within influential branches of social studies of science, the adoption of the symmetry principle (first presented by Bloor 1976) and the existence of the experimenter's regress (as put forth mainly by Collins & Pinch 1994). Both are based on the following line of reasoning: in a scientific controversy no-one can decide who is right and who is wrong by referring to rational arguments and factual evidence, because if someone could, there would be no controversy in the first place. It is argued that this description of scientific controversies does not demonstrate the absense of rationality, it simply presupposes it. There may well be rational arguments at work in a scientific controversy, but if a sociologist of science does not look for them, they will not be found. The advocates of the symmetry principle and the experimenter's regress have not presented any arguments in support of their assumption that rational arguments and evidence play no role in the settlement of scientific controversies. So far, then, there is no reason to adopt the symmetry principle in social studies of science, and there is no reason to believe in the experimenter's regress.

Introduction

The title of this paper is a question: What's wrong with social studies of science? A short answer would be: nothing! Social studies of science are of course a legitimate field of inquiry, and it would be difficult indeed to claim that the field is all "wrong." Nonetheless, I will argue that some things *are* wrong within social studies of science, as pursued by some influential researchers. In particular, I will argue that at least one of the most generally endorsed tenets of social studies—the principle of symmetry—is untenable. I will also argue that this principle has had a negative influence on sociologists' discussions of the purported "experimenter's regress."

The Four Tenets of SSK

The Edinburgh School within the sociology of science has been very influential. It originated around David Bloor and colleagues at the University of Edinburgh and later gave rise to bifurcations like the Bath School (directed by Harry Collins) and Actor Network Theory (Bruno Latour *inter alia*). A seminal text is *Knowledge and Social Imagery* (Bloor 1976). Here Bloor suggests that the sociology of scientific knowledge (SSK)—or the *strong programme*, as he called it—should adhere to four tenets (quotes are from Bloor 1976):

1. *Causality*. The sociology of science would be "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. *Impartiality*. The sociology would be "impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation."
3. *Symmetry*. The sociology would be "symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs."
4. *Reflexivity*. "In principle its patterns of explanation would have to be applicable to sociology itself."

These principles have been heavily referred to by sociologists and critics alike for the last decades. Not all principles are controversial but the third, symmetry, certainly is. When philosopher Philip Kitcher (1998) listed and criticized "dogmas" of science studies, the symmetry principle was one of them. Here I will concentrate on that principle.

Canadian philosopher James R. Brown argues that the problem with the symmetry principle, as it has been invoked, is not so much its explanatory symmetry with respect to true and false beliefs as its symmetry with respect to rational and irrational beliefs.[1] For example, it would not be blatantly wrong to explain Ptolemy's (false but arguably rational) belief that the earth is at the center of the universe with approximately the same tools as we would use to explain today's (true and rational) belief that the earth is *not* at the center of the universe (Brown 2001, p. 129). But it would be strange, says Brown, to explain rational and irrational beliefs in the same way, for example the belief that my friend has the flu and the belief that my friend once was abducted by aliens.

Brown is right when he draws attention to the distinction between rational and irrational beliefs. In many cases, however, his basic point can be made simpler, by saying that it is strange not to take natural facts (evidence) into account when they can influence beliefs. I shall try to explain this as clearly as possible.

A historian usually does not view history as a march towards a predetermined goal (unless he is a marxist or hegelian, or is put under political pressure). While it is possible to trace evolutionary lines over long periods of time—e.g. the evolution of democracy in the Western world—historians try to explain them with a multitude of tools, drawing attention to various social, economic, religious, cultural, climatological, technical, personal factors, and more. Rarely do historians say: There is nothing to explain, because history has simply unfolded the way it was bound to unfold.

In contrast, science *has* a predetermined goal, namely the unveiling of true knowledge about the natural world. Historians and sociologists of science therefore should *not* act like "ordinary" historians and sociologists in assuming that anything could have happened in science. Anything clearly could *not* happen in the scientists' experiments. This is so because nature is an important part of the action, and nature does not behave arbitrarily but obeys certain laws, for example Newtonian mechanics for macro-systems with non-relativistic velocities.

Physicist Steven Weinberg has described this difference succinctly:

> "[I]t is true that natural selection was working during the time of Lamarck, and the atom did exist in the days of Mach, and fast electrons behaved according to the laws of relativity even before Einstein. Present scientific knowledge has the potentiality of being relevant in the history of science in a way that present moral and political judgments may not be relevant in political or social history" (Weinberg 2001, p. 120).

Weinberg gives the following example: J. J. Thomson, a physicist known for his discovery of the electron, measured the ratio of the electron's mass to charge. He found a range of values. He favoured the values at the high end of the range. Why did he do that? Maybe Thomson knew that they had been produced in the most carefully performed measurements. Or maybe his first values were at the high end of the range, and he wanted to stick to these values in order to demonstrate that he had been right at the beginning. Which one of these hypotheses is correct cannot be settled by a careful study of all preserved historical records. But the question *can* be settled by the fact that today's actual value of the ratio of the electron's mass to charge is at the low end of Thomson's range of values. This strongly

[1] Larry Laudan, too, emphasizes the need to distinguish between symmetry related to truth/falsity and symmetry related to rationality/irrationality (Laudan 1996, pp. 192ff).

favours the second hypothesis: Thomson wanted to stick to his first values (Weinberg 2001, p. 121).

Thomson's rationale for favouring the higher values may not be the most important question in the history of science, but I believe this example shows clerarly that the sociologists' refusal to take evidence into account is nothing but throwing away potentially useful information. I would like to take this opportunity to give another example in the same vein.

Astronomy is the scientific study of cosmos. It is based on observations and rational arguments; or so the astronomers claim. Astrology, though sharing a distant origin with astronomy, is not considered scientific. Generally astrologers do not even claim to work rationally and scientifically. But since astronomers do, it seems appropriate to try to explain astronomers' beliefs at least partly by referring to rationality and evidence. Please note that the incorporation of factual evidence in an explanation of astro*nomical* beliefs does not mean that factual evidence should be completely absent in an explanation of astro*logical* beliefs. There is compelling evidence, for example, that planetary motions are governed by laws. These laws are part of astronomical *and* astrological beliefs, and so could enter explanations of both. But a difference between astronomy and astrology is that the latter is much less backed by evidence than the former. For example, the central astrological claim that planetary motions and/or positions direct (or reflect) human lives is totally unsubstantiated. Hence this astrological belief cannot be explained by reference to evidence, because there is no evidence.[2] Many astronomical beliefs, on the other hand, are supported by evidence. This evidence arguably should be taken into account in explanations of why the astronomical beliefs are held.

Philosophically, there is more to this story than simply the rejection of evidence as contributing factors of explanations. It seems to me that the only way of justifying such a waste of potentially useful information is to claim that our knowledge is not growing and that science is not making any progress. Although this notion has been put forth by some philosophers of science, it is so manifestly wrong that it hardly needs a rejoinder. Progress is evident in all fields of science. Fields that are not characterized by growth of knowledge are soon abandoned. A quote from the British philosopher of science Ian Hacking is appropriate:

> "Perhaps there are fools who think that the discovery of isotopes is no growth in real knowledge. (...) [T]hey are likely idle and have never read the texts or engaged in the experimental results of such growth. We should not argue with such ignoramuses. When they have learned how to use isotopes or simply read the texts, they will find out that knowledge does grow" (Hacking 1983, p. 120).[3]

The Experimenter's Regress

Harry Collins has authored several papers on the experiments and discussions among physicists about detection of gravitational radiation.[4] There is no doubt that Collins is very knowledgable in the field. His descriptions of the experiments have met with satisfaction from physicists. Not all of his conclusions, however, have been accepted.

One of Collins's conclusions about the search for gravity waves, presented in various papers and books (e.g. Collins 1985), sometimes in collaboration with Trevor Pinch (Collins & Pinch 1994, pp. 91-107), is that there is something murky about calibration. A good well-calibrated experimental apparatus (measurement device) is one that gives correct re-

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[2] Possibly, the belief could be explained by *lack* of evidence, but that's another story.

[3] Hacking attributes this attitude to Lakatos, though I suspect it is shared by Hacking himself.

[4] I will use the terms "gravitational radiation" and "gravity waves" interchangeably.

sults. Correct results, on the other hand, are those that are given by a good well-calibrated apparatus. There seems to be a circle here, called the *experimenter's regress*. According to Collins, the regress is broken by negotiation within the scientific community.

On the face of it, this state of affairs may not be a widespread problem, since examples where measurement devices are calibrated without the presence of the experimenter's regress are easy to find. Let's say we would like to calibrate a stick used for length measurements. It can be calibrated by comparison with another stick of known length. That stick, in turn, is calibrated in the same way. In the end, this calibration regress is broken by a comparison with a stick the length of which is *defined*. In a similar manner, devices for measuring mass (weight) are ultimately calibrated by comparison with prototypes defined to have a specified mass. The same goes for measurements of time. Length, mass and time are easy examples, but I see no principal reason why a similar argument could not be made for more complex quantities, like energy.[5] Calibration of energy-recording apparatus is, incidentally, important in Collins's discussions of the purported experimenter's regress in the search for gravity waves.

So there *is* a regress in experimental situations, but it is obviously not the same as the regress claimed by Collins to be present. The true regress is trivial and is broken by appeal to definitions. In contrast, Collins's regress is broken mainly by appeal to authority and is therefore much more controversial (to those who believe that the role of personal authority should be downplayed in science).

Collins would not deny the existence of the trivial regress. Quite the opposite, he would probably say that most regresses are broken in that way.[6] But he would insist that this was not the case with gravity waves since at the time of controversy it was not known whether gravity waves existed or not. The correct behaviour of the apparatuses could therefore not be predicted. Had this been known, there would not have been a controversy in the first place.

In other words, this is what Collins says: There is a controversy among scientists. If there is a controversy, you cannot decide who is right and who is wrong. And if you cannot decide this, you cannot tell whether an apparatus is functioning properly or not; you cannot calibrate it. That is the origin of the experimenter's regress.

In the case of gravitational radiation, Joseph Weber, professor of physics at the University of Maryland, was pursuing a programme of research in the 1970's based on his conviction that he had detected such cosmic gravitational radiation. Several other groups of researchers within the same field were sceptical. According to Collins, it was the intervention of a very influential physicist, Richard Garwin, that brought the controversy to an end. Through his personal authority and rhetorical skill he convinced a majority of physicists that Weber was wrong (Collins & Pinch 1994, pp. 104ff).

This description of the course of events has been challenged—successfully, in my opinion—by the physicist and philosopher of science Allan Franklin (1998). In essence, Franklin maintains that the critics' results were not only more numerous but had been carefully cross-checked in a way that Weber's had not. Moreover, the critics had investigated whether Weber's choice of a special computational algorithm could explain the other groups' failure to replicate his results. The critics used Weber's preferred procedure but still found no effect. They calibrated their own experimental apparatuses by inserting acoustic energy of known energy, finding that the signal could be detected. There were other arguments fa-

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[5] As it happens, energy (E) can be derived from the quantities of mass (M), length (L) and time (T): $E = ML^2T^{-2}$.

[6] "In most science the circle is broken because the appropriate range of outcomes is known at the outset. This provides a universally agreed criterion of experimental quality" (Collins & Pinch 1994, p. 98).

vouring the sceptical stance as well. All in all, Weber's critics had better arguments than Weber, arguments connected to factual evidence. They were publicly debated in print and in conferences and so were available for a perceptive sociologist like Collins had he looked for them.

Collins and the "Methodological Imperative"

In an article named "What is TRASP?: The Radical Programme as a Methodological Imperative", Collins discusses his methodology (Collins 1981). He states that Bloor's tenets of impartiality and symmetry constitutes the "Radical Programme" in the sociology of knowledge.

> "The tenet of symmetry tells us something about the content of our explanations. The same types of explanation will be applied to all 'qualities' of scientific endeavour. Explanations of the true will be like explanations of the false, and similarly for the rational and irrational, and the successful and unsuccessful and, we may suppose, for the apparently progressive and the degenerative. (…) [I]t follows that there are things that cannot form part of an explanation belonging to the radical programme. Knowledge cannot be explained by reference to what is true, rational, successful or progressive (hereafter 'TRASP'). If such categories were allowed into explanations then the explanation of, say true, knowledge would not be of the same type as the explanation of false knowledge" (Collins 1981, p. 217).

Collins is correct: *If* the principle of symmetry is taken as a postulate, one is not allowed to make reference to what is TRASP in an explanation of scientific beliefs. But this argument does not in itself contain any justification for the symmetry principle.

Does Collins give any reason for his embracing the Radical Programme? Yes, he asserts that the alternative is inferior. Any research strategy not committed to the impartiality and symmetry tenets is part of the "Normal Programme", according to Collins.[7] An investigator who wishes to give an explanation involving rationality must collect data about which scientists' acts are TRASP and which are not. But that is impossible, says Collins:

> "It goes almost without saying that the investigator should not make his own judgements about which of a set of competing scientists' accounts were the correct ones. To do this would be to introduce personal bias into the data collection process. Furthermore, such a judgement would rest on the implication that the investigator—not the experimenter, theorist or professional expert in the area in question—was in a position to make scientific judgements that the scientists themselves could not make" (Collins 1981, p. 220).

Though skillfully worded, this argument is insufficient. It does not *show* that scientific controversies are not resolved by way of rationality, it merely *presupposes* it. If scientists do not resolve scientific conflicts by invoking rational arguments and evidence, of course an investigator will not be able to invoke them either. If, on the other hand, rationality and evidence contribute to scientists' judgements, the investigator should try to incorporate this in his explanation of the conflict resolution. Certainly, it may be difficult for an investigating sociologist to comprehend the exact nature of the rational arguments involved, but it ought not be impossible. In his TRASP paper (Collins 1981), Collins's only example of a scientific controversy where the correct position was not available at the time of controversy (and the investigator therefore should not make any judgements) is the gravity wave episode discussed above. As already mentioned, many arguments based on rationality and factual evidence *were* available during that controversy.

As should be evident, Collins's argument for stopping TRASP factors from entering sociological explanations of scientific beliefs (and hence his argument for using the sym-

[7] One could question the appropriateness of this definition, making Normal Programmes out of every strategy that is not explicitly Radical, but let us not pursue the matter here.

metry principle) is identical to his argument for the emergence of the experimenter's regress. That is why it is appropriate to treat them in conjunction. In both cases, the crucial point is that since there is a controversy, no-one (and particularly not a sociologist) can tell who is right and who is wrong.

Collins's methodological imperative urges the sociologist to offer explanations of scientific endeavours in which any references to what is TRASP have been bracketed out. As far as I can see, this must mean that (1) in reality, and contrary to scientists' beliefs, TRASP factors play no role in science, or that (2) TRASP factors do play a role in science, but they are so insignificant that it makes no big difference to leave them out in sociological explanations, or that (3) TRASP factors play a significant role in science. If (1) is true, then science does not make any progress. This notion is absurd and is hardly worth discussing. If (3) is true, there is no point in pursuing the Radical Programme. This leaves (2). Could (2) be true? Maybe, but I don't think so and I have seen no arguments supporting it.

Conclusion

In summary, the principle of symmetry forces the sociologist to leave out factors of possible major influence. I believe that the advocates of this principle repeatedly have shown that *if* you apply it you will end up with explanations devoid of rationality and factual evidence. But that is trivial and supplies no reason for accepting the symmetry as a postulate in the first place. No-one has, as far as I know, offered good reasons for accepting the symmetry principle. Its proponents seem simply to have accepted it at face value, perhaps because of its simplistic elegance. The same goes for the purported experimenter's regress, because according to Collins the regress will not appear unless the symmetry principle is justified.

The criticisms displayed here do not automatically lend support to a total rejection of social studies of science where the symmetry principle has been applied. There may be valuable insights in the case studies produced by Collins and his peers, but they should be read with caution since they are biased. The real impact of rational arguments and evidence cannot be extracted from those studies, because such factors were self-imposedly excluded from analysis.

References

Bloor, David (1976). *Knowledge and Social Imagery*. London: Routledge & Kegan Paul.

Brown, James R. (2001). *Who Rules in Science? An Opinionated Guide to the Wars*. Harvard: Harvard University Press.

Collins, Harry M. (1981). "What Is TRASP?: The Radical Programme as a Methodological Imperative," *Philosophy of the Social Sciences* 11, pp. 215-224.

Collins, Harry M. (1985). *Changing Order: Replication and Induction in Scientific Practice*. London: Sage.

Collins, Harry & Pinch, Trevor (1994). *The Golem: What Everyone Should Know About Science*. Cambridge: Cambridge University Press.

Editors of Lingua Franca (2000). *The Sokal Hoax: The Sham That Shook the Academy*. Lincoln, NE: University of Nebraska Press.

Franklin, Allan (1998). "Avoiding the Experimenter's Regress." In: Koertge 1998, pp. 151-165.

Hacking, Ian (1983). *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*. Cambridge: Cambridge University Press.

Kitcher, Philip (1998). "A Plea for Science Studies." In: Koertge (1998), pp. 32-56.

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

Labinger, Jay A. & Collins, Harry (eds) (2001): *The One Culture? A Conversation about Science*. Chicago: University of Chicago Press.

Laudan, Larry (1996). *Beyond Positivism and Relativism: Theory, Method, and Evidence*. Boulder, Colorado: Westview Press.

Weinberg, Steven (2000). "Sokal's Hoax, and Selected Responses." In: Editors of Lingua Franca (2000), pp. 148-171. (Originally published in *New York Review of Books*, 1996.)

Weinberg, Steven (2001). "Physics and History." In: Labinger & Collins (2001), pp. 116-127.