

Who's a Captive? Who's a Victim? Response to Collins's Method Talk

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We are grateful to Harry Collins for clarifying his own assumptions about science and social science. He argues for neutrality as a methodological prescription. It was precisely our aim to argue against this.

We begin here by specifying three difficulties with the prescription of methodological neutrality for controversy studies: interaction with the debate, selection and deployment of methods, and social influences on methods. The latter two difficulties apply also to methods used in natural science, as we will illustrate. Then we turn to Collins's claims about the greater legitimacy to be gained from an appearance of methodological neutrality.

In the first place, we have argued that one's method affects one's interactions with partisans in the controversy. Adopting a stance of symmetry means that it is much easier to obtain information—including documents, interviews, and insider perspectives—from opponents of orthodoxy. A pre-planned "symmetrical" approach of interviewing people on both sides may not work in practice because of suspicions, polarization, or the impact of prior researchers (Scott, Richards, and Martin 1990).

In our view, the possibility of methodological neutrality in social studies of scientific controversies should be just as much a matter for empirical study as the alleged neutral method of the natural sciences. We studied it in our cases of the Australian Animal Health Laboratory, fluoridation, and vitamin C and cancer and found that the methodological prescription of neutral social analysis was as impossible in practice as the prescribed neutrality of the scientist's method. Since the publication of our article, another empirical "self-study" that supports our conclusions has been drawn to our attention (Kroll-Smith and Couch 1990).

Second, a researcher takes a position by selecting a method from the range of possible methods. For social analysts, a prescription of symmetry is a choice of method. Collins agrees with us that a symmetrical analysis of a scientific controversy usually helps the side with less scientific credibility. Knowing this, there can be no neutrality in selecting a method.

We have no wish to appear to be teaching one of the pioneers of the empirical program of relativism his business, but the same applies in the natural sciences. For example, in the debate over the effect of exhausts from supersonic transport aircraft on stratospheric ozone, different scientists chose different methods of approaching the problem and modeling the processes in ways that helped to achieve the sorts of conclusions they seemed to be looking for (Martin 1979).

A third difficulty with the prescription of methodological neutrality is the existence of numerous social influences on the development, application, and choice of methods. Most obviously, only some methods receive research funding: the funders desire certain approaches because of the answers they are likely to give. For example, much more funding is available for certain types of studies of the operation of pesticides because of funding by pesticide companies. Heavy economic investments in particular types of telescopes or particle accelerators strongly influence research methods (Pinch 1986). In the social sciences, in many cases, studies that mimic the alleged method of the natural sciences are preferentially given funding over other approaches.

There are also moral, economic, and political constraints on choice of method in both the natural and social sciences. It is not considered acceptable to explode a nuclear weapon in a city as a means of studying the biological effects. And, as the recent debate in the pages of *Science, Technology, & Human Values* attests, the method employed by Epstein (1990) in his social investigation of peer review and journal publication raised ethical and other professional dilemmas for social scientists.

Choice and deployment of method are also routinely affected by professional advancement, including jobs, promotions, grants, and status. It is a commonplace observation that many scientists have a stake in a particular method, whether it is radiocarbon dating or Markov chains. These scientists in effect represent methods in search of problems. The resulting deployments of method cannot be considered neutral. These career influences apply to the choice of methods by controversy analysts, too—to ourselves and (dare we say it?) to Collins—and to the ways in which they deploy and rhetorically defend them.

Collins says that “as analysts, we would not use our findings to prescribe the proper methodology for research on, say, gravity waves.” Why not?

Social analysis sometimes may provide insights into scientific method not easily perceived by practitioners. Richards has drawn on her study of the vitamin C and cancer controversy to make recommendations concerning the use of randomized controlled clinical trials (Richards forthcoming). Why is this "inappropriate"?

Collins draws a distinction between doing research (which is allegedly neutral) and its impact (which is not). This methodological prescription for social scientists looks remarkably like the traditional positivist picture of scientific method. As Collins notes, scientists can pull out of nuclear weapons research, but we argue that this does not mean "the way they did their research" is unaffected by social forces. It is precisely because governments fund research with the goal of producing nuclear weapons for specific military purposes that nuclear scientists are in a position to do this research in the first place. Where is the neutral method? From a sociological point of view, the claim of methodological neutrality might be better understood as a convenient myth that serves rhetorical and political purposes in dissociating the researcher from the socially contentious products of the research (Schuster and Yeo 1986). Should sociologically informed social scientists perpetuate the very mythology that their investigations consistently expose?

This brings us to Collins's argument that social scientists should claim to be methodologically neutral in order to give themselves greater legitimacy on important social issues such as poverty and unemployment. We believe the value of an appearance of neutrality cannot be assumed; rather, it too is something that warrants empirical study. Certainly, it is not automatically an advantage. It can be argued that the major impact in areas such as poverty, war, environment, and repression often comes from committed scholarship or, more likely, activism, rather than ostensibly neutral scholarship.

We disagree with Collins's claim that an appearance of neutrality is invariably a good way to undermine dominant ideologies. Since the majority of intellectuals both support dominant ideologies and trade on the claim of neutrality, it may be more subversive to challenge the possibility of neutrality. Again, this should be a matter for empirical study. Might it be the case that, for a social scientist, an appearance of neutrality adds legitimacy principally in the eyes of mainstream peers?

Collins says that stating connections between scientific knowledge and its social context "is a high-level, analyst's claim." To the contrary. It is Collins who makes a high-level claim with his assertions about methodological neutrality. Our case studies dealing with issues having serious policy implications show that the connections between knowledge and context are very down to earth indeed.

We are less worried that our work may be used to “legitimize the claims of charlatans” (after all, as Collins surely would agree, the label of charlatan is a socially negotiated one) than we are by Collins’s rigid segregation of method from politics. His comments are a worrying reflection on some work in the sociology of scientific knowledge that, because of its reluctance to engage with policy issues, is in danger of becoming only an intellectual exercise for armchair philosophers.

References

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