

# **Captives and Victims: Comment on Scott, Richards, and Martin**

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Scott, Richards, and Martin (1990; SRM) are almost entirely right in their description of the dilemma in which analysts of scientific controversies find themselves. In particular, they are right in saying that the “neutral” analyst will be thought of by participants as being on the side of the underdog. Thus however neutral the analyst intends to be, the work will always be drawn into the debate. Only SRM’s conclusion — that attempting to be neutral is pointless — is wrong. The argument needs to be set in context; the problem they describe is a dilemma of the social sciences as a whole, not just the sociology of scientific knowledge.

It is a commonplace among sociologists that attempts at neutral analysis are seen as subversive by dominant ideologies. For example, it is in the interest of states to minimize and disguise social and economic problems. Sociologists who suggest — through the most neutrally intentioned research — that things are worse than they appear, are branded as ideologically motivated subversives. This is what gives teeth to the idea that sociology is a critical discipline. The dominant ideology’s tactic is easily understood. It is a piece of “labeling.” The effect is to reduce the legitimacy and potency of the research as well as the discipline as a whole. For sociologists to accept the label voluntarily is for them to reduce the legitimacy of their own work. I cannot believe that SRM would want this if the topics were poverty and unemployment rather than the nature of parapsychology or vitamin C. This, then, is to set SRM’s remarks in a wider political context, but there is also the matter of the logic of the social sciences.

Though this may seem a heretical statement for a sociologist of scientific knowledge, we need to distinguish between the politics and the methodology of our work. That is, while as analysts we can understand that all science is in a broad sense “political,” as researchers we need to keep this knowledge in a separate compartment. Just as, as analysts, we would not use our findings

to prescribe the proper methodology for research on, say, gravity waves, we should not allow our analytical sensitivities to affect our own research in inappropriate ways. We need, then, to distinguish between how we do our work and its impact.

Neutrality is a methodological prescription. One may try to be methodologically neutral while accepting that one's work will have an asymmetrical impact on the world. If you think that the impact will be bad, you may well pull out of the research. For example, natural scientists have pulled out of research on nuclear weapons because they fear its consequences, even though, as far as they were concerned, the consequences did not affect the way they did their research. To turn to sociology of science, the research on parapsychology gives comfort to the parapsychologists—something that does not, as it happens, concern me—but I do *fear* that the same research may be used to legitimize the claims of charlatans; this is something that *does* worry me. The effects and the worries are, however, not related to the methodology of the studies.

SRM are wrong to conflate Mulkey, Potter, and Yearley's (1983) criticism of Collins and Pinch's work with this kind of unintended consequence. Mulkey, Potter, and Yearley claimed that our research was not methodologically neutral. We were taken over, they say, by the parapsychologists' language and ideology, and therefore the research was flawed in its execution. This is not a matter of labeling, nor is it a matter of asymmetry of use recognized post hoc; it is a straightforward accusation of methodological bias that can be straightforwardly shown to be wrong (Collins and Pinch 1983, 106-7). It is worth noting that it is possible to mount a defense against criticism such as that of Mulkey, Potter, and Yearley, whereas it is impossible to mount a defense against the way that one's work is used—except with the kind of despairing *cri de coeur* to which SRM refer (Collins and Pinch 1979, 263).

As sociologists of scientific knowledge, we are inclined to believe that all scientific research, whether it is controversial or not, is related to its social and political context. But this is a high-level analyst's claim, it is not a methodological prescription. What is more, as critics, we can see the asymmetrical effects of much scientific research, but this again is post hoc analysis rather than methodological critique. To conflate these high-level claims with methodological bias—that is, to leave no room for *methodological* neutrality—is to make a mistake about the logic of the social sciences while accepting the politics of the dominant ideology of science.

## References

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