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COMMENT

BY

JAN GOLINSKI

Of course, I am delighted that the authors of the preceding papers have found so much of interest in my book, *Making Natural Knowledge*: *Constructivism and the History of Science*. Since the book had little to say about the social or human sciences, and nothing specifically about economics, the response has been a pleasant surprise. In a general way, I did have an interdisciplinary audience in mind when I wrote, but I did not set out to address people working in the history of economics. This means that it would be pretentious for me to tell practitioners in this field what they should make of my work: its utility really is in *your* hands.

Reflecting on the papers in this session, I remain rather uncertain exactly what it is that my book is being hailed for. As the authors of the papers have recognized, it was not written as a manifesto for a particular approach, but rather as a review of recent developments in the history of the natural sciences, showing how constructivist perspectives have been put to use to shape the direction of historical research. I defined constructivism very broadly, as the outlook "which regards scientific knowledge primarily as a human product, made with locally situated cultural and material resources, rather than as simply the revelation of a pre-given order of nature." I then summarized specific varieties of constructivist theory, including the Edinburgh "Strong Programme," the Sociology of Scientific Knowledge (SSK), and the Actor-Network approach. The bulk of the book was devoted to tracing how these principles inspired research on different themes of the history of science: careers and communities, laboratory practices, rhetoric and hermeneutics, instruments and visual images, the cultural relations of science, and so on.

I suppose there are various ways in which the book might be read as having implications for the history of economics. On a general level, it can be taken as endorsing a thoroughgoing historicism—one that begins with a rejection of "Whiggish" approaches and a determination that the past should not be seen as a teleological process culminating in the present. At an equally general level, any kind of constructivism obviously directs historians' attention to the context in which knowledge is produced, at the local resources that are used to make it, and at how it comes to be communicated from its point of origin. Context means many things, however, and it impinges on the making of knowledge in different ways. Constructivism does not give a universal answer to the question: By what means, specifically, is knowledge made and distributed? The answers could well

Department of History, University of New Hampshire, Durham, NH 03824–3586.

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be quite different in different fields of knowledge, and even in the same field at different times and places. This consideration seems likely to put a limit on how easily particular tools used in the study of the natural sciences may be carried over to the history of social sciences like economics.

To reinforce this point, consider how difficult it is to answer questions like the following: What is an *observation* in economics? What is an *experiment*, or an *instrument*, or a *laboratory*? What are the *objects* of economic knowledge? How are they represented and manipulated? The answers to these questions are not at all obvious, at least not to an outsider such as myself. Perhaps they are not even relevant questions in connection with a social science, but they have been fundamental ones for constructivist approaches to the natural sciences. The Sociology of Scientific Knowledge began by making problematic previously taken-for-granted categories like "experiment" and "instrument," looking at how they came to be defined and accorded authority among scientific practitioners. It could be that entirely different categories will prove to be the important ones in developing a constructivist approach to the history of economics.

Another point of difference between approaches to the natural and the social sciences was made clear to me by the papers and the discussion that followed them. Reflexivity appears as a more urgent issue. Sociologists and historians of the natural sciences may share some methods with the scientists they study, but they work on a distinct level, focusing on different kinds of objects. Those who are studying the social sciences, on the other hand, share the same preoccupation with explaining human behavior with those they are scrutinizing. Economists, notoriously, have their own ideas about why people act and think in the ways they do. This is bound to come to the surface when historians of economics present their ideas to economists themselves, as they usually (and commendably) wish to do. The historian then finds him- or herself put on the spot to apply their analytical perspective to the analyst. We can imagine the historian's audience inquiring: "What makes *you* think that's why economists thought so-and-so?"

Questions of reflexivity sometimes seem to open an abyss where there had seemed to be solid ground under one's feet. In a very penetrating and lucid essay review of my book, in *Social Studies of Science*, Ingemar Bohlin has accused me of being inconsistent on the issue. Perhaps that is so. I can only offer, as a defense, the thought that there might not be a general solution to the problem. Neither of the extreme positions that have been proposed seems applicable in every instance. On the one hand, the assertion that one can—and should—always rigidly demarcate one's analysis from any reflection on one's own situation overlooks instances when a degree of self-analysis can help make the historian's case. For example, we might want to prepare ourselves with a response to the taunting question mentioned previously. On the other hand, the contrary position, which proclaims that the same analytical perspective can—and should—be consistently applied to the analyst and to the object of study, threatens to lead to infinite regresses of methodological introspection. One wants to be able to draw a line somewhere, by distinguishing between what we know of the object and what the analyst can know of him- or herself, for example, or by identifying the different purposes served by discussing the history of economics as opposed to discussing the historian.

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Although I have only been able to respond briefly, the experience of thinking about how these issues arise in a field like the history of economics has been a stimulating one. I thank all of the contributors for their thoughtful and thought-provoking responses to my work.

REFERENCE

Bohelin, Ingemar. 1999. "Making History." *Social Studies of Science* 29: 459–80.

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