

This text was the basis of my remarks in the plenary roundtable, “Why Do Historians of Economics Hate The Sociology of Scientific Knowledge?”, held at the History of Economics Society Meetings, York University, Toronto, on June 29, 2008

My remarks are not meant to argue why one *should* oppose the use of Sociology of Scientific Knowledge (SSK) in the history of economics. Instead, they are meant to be descriptive: why it might be the case that some historians have been reluctant to embrace with open arms SSK approaches to the history of economics. Broadly speaking, I see four impediments. Call them the what-is-it problem, the bundling problem, the problem of authority, and the anti-realism problem.

First, the what-is-it problem. Science studies is not monolithic.¹ It is, in fact, a rubric for a wildly heterogeneous collection of social scientific approaches to the study of science. Among sociologists, there are the Bath School, the Strong Program, and those who focus more on technology than on science. Other scholars use anthropological, ethnographic and ethnological methods; still others deconstruct texts (discourse analysis) or decode science’s sign systems (semiotics).

Amid these various approaches, SSK is both historical, looking at science after it has happened, but also contemporary, looking at science as it happens (laboratory studies). SSK approaches are, moreover, at once descriptive and normative. And, SSK approaches as practiced in the history of economics are also quite plural.

So, what about the SSK as practiced in the HET community? First, SSK approaches in HET should be seen as one response to a larger and I think mostly valid critique of traditional HET practices: the tendency toward ideological axe-grinding, the construction of Whiggish genealogies, and the practice of heterodox economics *sans* history.

Second, there is the bundling problem. Some variants of SSK approaches in HET have tended to conflate or to bundle “historians’ values” with a particular subset of sociologists’ values, one that embeds a radically skeptical epistemology. The historian’s values argue that we should better situate economic ideas in the personal, social and

¹ What is science studies? Broadly, it’s the enterprise of using social science to understand what scientists do, what they produce, and especially the (social) causes their of scientific beliefs.

intellectual contexts in which they were created. We should produce historical writing that is thicker, richer . . . and creamier, what I have called Betty Crocker historiography (Leonard 2004). There aren't too many who object to these wholesome, virtuous and somewhat bland historiographic injunctions.

But the Betty Crocker virtues have come to the HET community bundled with an additional argument: that historians of economics should also embrace Critical Science Studies epistemology – a radically skeptical theory of knowledge that I have called The Position. Whatever your views of The Position, there is no necessary connection between the two, as Wade Hands (1997), among others, has pointed out. There is thin SSK and there is thick history without a paramount concern with or a prior position on “the social” as the exclusive determinant of what scientists believe.

Third, some historians of economics see a tension or incongruity between a putative SSK ecumenism on the one hand -- let a 1,000 flowers bloom; there is no best way to write the history of economics; there are only different ways, etc. – and, on the other hand, a forceful advocacy for the superiority of a sociologically informed method of historical writing about economics (and other sciences).

This incongruity, or the problem of authority, has two aspects. First, SSK proponents, though critical of other approaches to science studies, are sometimes uncritical with respect to their own. Esther-Mirjam Sent and Philip Mirowski, for example, have criticized the use of economic ideas as resources in the study of science. Economics is entirely fair game, and I concur with some of their criticisms. But so, too, should sociology be fair game: my own view is that (well-founded) critiques of economics of science apply with equal (or even greater) force to the SSK. I could be wrong, but the point here is to argue that the relevant question before us is “how does SSK compare with the alternative intellectual resources for the scholarly work of historians in our community?” And any useful answer requires that we be as critical with respect to the SSK as we are with respect to its alternatives.

The second incongruity concerns the matter of standards of practice. By whose standards are sociologically informed methods of historical writing deemed superior? One answer: the “historical community’s standards,” or that community’s “notions of acceptable historical practice.” This reply strikes me as oddly uncritical, and even

ahistorical. *Which* historical community? From what era? And, what sort of practice: political history, social history, intellectual history, feminist history, history of science, cliometrics, etc?

We rightly insist that historians of economics ask contextualizing questions: “what constitutes a good explanation in economics?”; “what constitutes serious empirical work in economics?”; “which post-War community of economists do you refer to: Chicago, MIT-Harvard, Cowles, Cambridge?” etc. Good Betty Crocker questions all. And the same contextualizing scrutiny should be applied when one appeals to the putative authority of a scholarly community or a canon of practice. Put somewhat differently, we can no more take as stable, invariant, foundational, etc. the discipline we use to investigate than we can the scientific discipline under investigation.

The fourth impediment concerns the anti-realist epistemology of The Position, and its implications for the relevance of evidence to historical writing. Leading practitioners of the SSK have tended to run together two distinct philosophical claims: statements about reality are socially constructed, and reality itself is socially constructed. Says Harry Collins, for example, “the natural world has a small or non-existent role in the construction of scientific knowledge” (1981: 3). “[W]e can never use . . . Nature”, argues Bruno Latour, “to explain how and why a [scientific] controversy has been settled” (1987: 99). And, argue Collins and Yearly, “explanations [in science studies] should be developed within the assumption that the real world does not affect what the scientist believes about it” (1992: 372).

Latour and Woolgar’s *Laboratory Life* argues plainly that things (such as viruses) don’t exist independent of scientific inquiry. Viruses are, they say, literally constructed by the empirical devices of science (such as electron microscopes). The Latour and Woolgar species of anti-realism, which makes facts wholly social creations, dispenses with the idea of empirical evidence performing any sort of epistemic function. And, they frankly acknowledge the paradox created by their enterprise, *which provides empirical evidence for the thesis that empirical evidence doesn’t matter*.

Not all SSK practitioners adopt such an extreme anti-realism. But the fact that so many do may be an impediment to the wider adoption of SSK approaches in the history of economics. And this is despite many natural affinities on other fronts.

Historians naturally incline to a kind of relativism. Our work leads us to appreciate that the social world can have a multiplicity of interpretations. We also tend to be skeptical, not least from long experience reading the tendentious and self-serving accounts of scientists (exact and other) with respect to priority, motive, etc.

But historians also believe that evidence matters. Our inclination to multiplicity and to skepticism ordinarily stops well before the thoroughgoing anti-realism of the leading SSKers just cited. Thomas Kuhn rejected anti-realism. He believed that “observation and experience can and must drastically restrict the range of admissible scientific belief, else there would be no science” (1962: 4).² For Kuhn, as, I think, for most historians of economics, to say that science is social is not to make an argument-ending claim in epistemology. It forms, rather, a mandate to inquire into how the social character of science, exact and other, promotes or undermines knowledge.

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² References are available in Leonard (2002).