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MAKING ECONOMIC KNOWLEDGE: REFLECTIONS ON GOLINSKI'S CONSTRUCTIVIST HISTORY OF SCIENCE

BY

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While most scientists and philosophers of science privilege scientific knowledge, and have sought demarcations of science from non-science to justify the privilege, sociologists of science, small numbers of philosophers of science, anthropologists, and some scientists themselves have been attracted to a new way of talking about science. Prefigured by Ludwik Fleck (1935/1979) and Gaston Bachelard (1934/1984), nurtured by the controversies over Thomas Kuhn's work, and instantiated in the Edinburgh School's Strong Program, the naturalistic turn portrays science as a human activity, part of the woof and warp of culture itself. Yet curiously historians of science have been less involved in this recent reconceptualization of both science and scientific knowledge.

Perhaps this is because the history of science is rather less "theorized" a subdiscipline than social history, or feminist history, or queer history. If by "theorization" we mean the adaptation of an overarching perspective on the activity itself, a perspective shaping one's approach to the field, then the history of science is even undertheorized when compared to fields like literary studies, or science, or art, or economics. Economists call such meta-perspectives on economics "methodologies," and in that sense the history of science has not really had a defining methodology. To be sure, there is a wealth of material written on historiography. Historians teach historiography in their graduate programs, and for working historians its value frequently is associated with self-consciousness about the activity called "doing historical research." The larger set of historiographic issues is alive for the professional historian who has wrestled with Marxist history, psychohistory, social history, quantitative history, cliometrics, gendered history, etc. All these alternative histories force awareness of the contested nature of ideas of historical truth, and meaning, and narrative. So philosophies of history, or theories of history, remain in the collective mind of the history profession: for instance, contributors to the *Journal of History and Theory* regularly make provocative forays into the history discipline's self-awareness.

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Nevertheless, the history of science has been relatively unaffected by such historiographic entanglements. The one book-length treatment called *An Introduction to the Historiography of Science* (Kragh 1987) is a melange of advice to researchers, alternative approaches to using scientific documents, and related matters. Another recent work, *The Historiography of Contemporary Science and Technology* (1997), likewise takes on a number of specific questions that researchers might have to deal with like "Using Interviews to Write the History of Science" (Chadarevian 1997). Historians of science have been somewhat disengaged from the naturalistic turn taken in studying science by other disciplines. Within the history of science community, there has been no historiographic fracas to compare with the controversy concerning the pragmatist or anti-foundationalist arguments about epistemology presently worrying philosophers of science. For instance, a historical work like Shapin's and Shaffer's *Leviathan and the Air-Pump* (1985) on Boyle's controversy with Hobbes over the vacuum was an outsider's work, for the two authors are sociologists of science and the book was mostly directed to a science studies audience. "Is this really history?" asked some historians of science.

Jan Golinski's book *Making Natural Knowledge*: *Constructivism and the History of Science* (1998) intervenes in these discussions with sensitivity and clarity. Addressing his fellow historians of science for the most part, Golinski presents the new ideas in philosophy, history, and science studies and holds them up to historians of science under a banner saying, "How might our project be different were we to take these ideas seriously?"

Since I believe that with respect to their practices as human activities, there is no useful distinction to be made among economics, chemistry, cell biology, statistics, painting, or music, I can see no particular reason why science studies should not address economics. As the philosophy of science is to the philosophy of economics, so, too, the history of science must be to the history of economics. Thus I have a natural interest in Golinski's book. Since I am a historian of economics, a historian of a particular social science, I believe that Golinski's book is directed to me and to my subdiscipline within economics. So I ask: If we take seriously the concerns that Golinski addresses, what difference will it make for our enterprise?

For historians of economics there are at least three paths into consideration of these matters. First, scholars of the naturalistic turn have been reconsidering the nature of scientific knowledge, and science, under the science studies "banner." Thus if economics is a science, how one understands both economic knowledge and the activity called "doing economics" should be affected. Since historians of economics especially are concerned with the study of economics and the structure of economic knowledge, they should be interested in the emergence of this set of ideas. The path here might be through recent works like Barbara Herrnstein Smith's *Belief and Resistance* (1997) or Andrew Pickering's *The Mangle of Practice* (1995) or Mario Biagioli's *The Science Studies Reader* (1999).

Second, as historians of economics consider themselves historians generally, and historians have engaged these ideas about objectivity, about perspective, about the nature and construction of evidence, and about how beliefs are

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transformed into knowledge or truth-making in particular communities, so should historians of economics engage these ideas in their histories of communities of economists. The path here might be through recent works like Peter Novick's *The Noble Dream* (1988) or Hayden White's *Tropics of Discourse* (1990), or through a reader like *History and Theory* (Fay *et. al.* 1998).

And third, from the perspective of the history of science, historians of economics are in fact historians of a particular social science. Thus to the extent that historians of science take the constructivist moves seriously, so, too, might historians of economics. The path here might lead through work done by individuals like Theodore Porter (1995) or M. Norton Wise (1995), who have been writing on areas that touch on economics from a history of science perspective informed by "science as culture." But certainly the path is lighted by Golinski's book.

Let's ask the apparently simple question: why are we doing the history of economics? What is our project in the service of? Ron Stanfield has written (in an HES-List message) that, "I study cultural and intellectual history in an attempt to reveal the sources of the distempers we suffer daily. I seek to propose and to advocate, so to help in my small way invigorate the democratic and liberal (free) process of making a living and living in an orderly and sound society."

Stanfield's perspective is similar to that of Philip Mirowski who introduces his own project of tracing the connection of mid-nineteenth century physics with the nascent neoclassical revolution in economics by asserting, "There is a wealth of important work to be discussed and assimilated into economic theory; however, each of these innovations has been obstructed by the dominant conception of economic value rooted in the imitation of physics" (Mirowski 1989, p. 10). He then concludes, after his path-breaking historical and polemical inquiry, that:

> In order to truly understand the impasse of neoclassical economic theory, we must appreciate that the importation of physical metaphors into the economic sphere has been relentless, remorseless, and unremitting in the history of economic thought. Simple extrapolation of this trend suggests that it will continue with or without the blessing or imprimatur of orthodox neoclassical economic theory" (Mirowski 1989, p. 395).

Yet this argument is a curious one. Its difficulties were pointed out by Stanley Fish in his Clarendon Lectures at Oxford (1995, pp. 74–75):

> to think that by exposing the leaks in a system you fatally wound it, is to engage in a strange kind of deconstructive Platonism—strange is because Platonism is what deconstruction pushes against—in which the surface features of life are declared illusory in relation to a deep underlying truth or non-truth. It is in the surfaces, however, that we live and move and have our being (it is surfaces all the way down) and no philosophical demonstration of their ephemerality will loosen their hold ... [T]rying to figure out what a poem means will be quite a different activity from trying to figure out which interpretation of a poem will contribute to the war effort or to the toppling of patriarchy.

Fish continued by noting that (p. 106): "[R]eflection is either (a) an activity

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within a practice and therefore finally not distanced from that practice's normative assumptions or (b) an activity grounded in its own normative assumptions and therefore one whose operations will reveal more about itself than about any practice viewed through its lens."

To which as a historian of economics I gloss: If you write history, write it well for whatever reasons you choose, and we will or will not be persuaded to its argument by your skill and craft as a historian. But as historians, do not expect us to *thereby* attend to your politics.

In contrast to Stanfield and Mirowski, I do not believe that it is my task as a historian to argue with economists about the right way to do economics, or about whether mainstream economics is on the wrong track or the right track. As a historian my task is to construct histories of economics, not to reconstruct the discipline of economics. Historical reconstruction—writing histories of economics that respect the contingencies of time and place and individual and context—is both a difficult and an important task for historians of economics. Attending to notions of the constructed record, the interpretive community in which economics is done, the fluidity of texts, and the cascades of representation and re-representation in which texts are formed and reformed, aids in producing histories which the community of historians of economics finds useful, illuminating, and interesting. As Roger Backhouse remarks, "it [must be] taken for granted that the constructivist perspective on the history of economic thought has much to contribute. It forces us to ask new questions and to look at history in a new light" (Backhouse 1992, p. 31). I agree with Backhouse.

The constructivist perspective that knowledge is local, contingent, and associated with the stable beliefs of particular communities at specific times is helpful to historians of economics interested in producing historical, as opposed to rational, reconstructions of economic ideas. Obsessed as many historians of economics are with questions about the progress of economic knowledge, a result of the historical contingency that historians of economics are mostly trained as economists, not historians, Golinski's guide to the naturalistic turn is most welcome. For after the naturalistic turn, notions of scientific knowledge, and the operations of scientific activity that produce knowledge, become themselves problematic. As Steven Shapin tells us:

> We traditionally and formally warrant scientific truth by pointing to individual empirical foundations, yet nothing recognizable as scientific knowledge would be possible were that knowledge actually to be individually sought and held. Nor would the paradox be resolved if we were to conceive of scientific knowledge as the aggregate of what individuals hold in their heads. To the aggregate of individuals we need to add the morally textured relations between them, notions like authority and trust and the socially situated norms which identify who is to be trusted, and at what price trust is to be withheld. The epistemological paradox can be removed only by removing solitary knowers from the center of knowledge-making scenes and by replacing them with a moral economy (Shapin 1994, p. 27).

Unlike some who fear that without the older certainties about scientific knowledge, science will lose its value to human society (*vide* the "Science Wars"), I welcome the richer and more complex historical discourse that is opened by the

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constructivist move. I see so many more opportunities for our projects—our work as historians of economics—to flourish in this new discursive space. How do ideas gain currency, gain assent, gain epistemic power? We are so very accustomed to argue that "X took idea A from Y, modified it into B, and thus was created the theory of ..." Our histories so very often study whether X "really" understood what Y was saying, or whether Y "really" was talking about A* instead of A, or whether indeed X was the first to think about B since "precursor Z" had thought the thought earlier, but had been ignored, etc. But the mechanics of how beliefs are transformed within intentional communities, such as scientific communities, into public knowledge remains quite mysterious. Our favored techniques of chronological narrative and sequential accretion of knowledge hardly penetrate the mists of personal and social contingency. As Golinski warns us:

> [T]he narrator needs to abandon the pretense of telling it just like it was and to admit to the artificial quality of his or her narrative ... There is another reason for this, namely, to differentiate the historian's account from the self-serving narratives deployed within scientific discourse. Historians of science disavowed those romantic plots that are oriented strictly toward the present, and they ought also to disavow the pretense of the purportedly unplotted narrative as a means of sustaining their own credentials ... The historian's text needs to display, to some degree, "the awareness of its own inadequacy as an image of reality" (White 1973, p. 10). The "naturalism" of narrative can scarcely be employed unproblematically by those who claimed to have shown how the "natural" is made (Golinski 1999, pp. 204–205).

Golinski reminds us that a constructivist perspective in the history of science involves different ways of talking and writing:

> The transition from the state of uncertainty, when it is not clear what is "real" and what is artifact, what is "signal" and what "noise," to the subsequent crystallization of a distinction between the phenomenon and its incidental human framework is a mysterious one. In retrospect, it is most often glossed as a discontinuity in the flow of time, when the pre-existence reality is suddenly revealed. Latour talks about "this miraculous emergence of new things that have always already been there" (1993, p. 70). To overcome this retrospective view, and to try to recapture what the experience was like, is a formidable and unsettling task ... [T]his is nonetheless the task of a historical narrative that takes the constructivist outlook as its point of departure. Such a narrative has to register both the uncertainties of the laboring investigator and the emergence of a solution, in the light of which looking back everything is suddenly clear. Tracing a passage through an experienced temporality that is fractured and reflected back upon itself in this way seems to require a departure from naturalistic narrative and the model of time that it assumes. "Newtonian" time, which flows uniformly and independently of human action, does not seem adequate for such narratives ... rather, we have to chart how time itself is fragmented and realigned by the work of the construction of knowledge (Golinski 1999, p. 205).

Something indeed will be lost to historians of economics who adopt the

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constructivist perspective, and that is the single-minded focus on "progress" in economics or, for critics of economics-as-it-has-become, "degeneration."

> Perhaps we are now at a stage when history of science—the history of human engagement with the material world and of the embeddedness of knowledge in time—can in turn shed light on the fundamental categories of human experience. Rather than regretting the passing of the comforting old stories of scientific progress, we should therefore embrace with eagerness the prospect of entirely new ones (Golinski 1999, pp. 205–206).

Jan Golinski's book highlights such new questions we might address, new ways of thinking about the stories we might want to tell, and even new ways of looking at all the old stories our elders have told before. Is this not an exciting time to be a historian of economics?

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