

# MAKING ECONOMIC KNOWLEDGE: REVIEW OF JAN GOLINSKI'S *MAKING NATURAL KNOWLEDGE*

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## I. INTRODUCTION

When I went through my graduate training in economics at Stanford University, I learned that economics consists of ideas. These are often expressed in mathematical terms and can be found in books and articles. To become a successful economist, you have to understand these ideas and come up with variations on them. So, during my first year of graduate studies, I spent the majority of my time working my way through the many books and articles assigned for my classes in microeconomics, macroeconomics, and econometrics. During their lectures, our instructors would walk us through any difficult mathematical manipulations that we encountered in our readings. Surely, I thought, I was on my way to becoming a respected economist. Still, I needed to specialize in a few fields, which I did in my second year.

Given the interests I had developed during my undergraduate education at the University of Amsterdam, I decided to take a class in the history of economic ideas. To be sure, I understood that the history of economic ideas did not matter all that much, since ideas from the past were already incorporated into economists' present ideas. In addition, each contribution improved upon an existing theory, resulting in a progress of thought. Still, I felt it would be interesting to learn more about the great ideas of the great economists of the past. So, instead of struggling through mathematical manipulations, I got to read about the life and works of the likes of Adam Smith, David Ricardo, and Karl Marx in Jürg Niehans' (1990) textbook. I have to confess that the discussions I liked the best were those of the economists' private lives. At the same time, I understood that a sharp division had to be drawn between the lives and works of these economists. Still, it was good to find out that these men were not robots but had at one time been real living people. Yet, my focus was on working my way through the ideas of the past and their progress to the present.

After having specialized in alternative economic approaches, of which history of economic ideas was a part, development economics, macroeconomics, and

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international economics, I felt ready to come up with variations on the ideas I had learned. This eventually resulted in a successful completion of my Ph.D. and a position as an assistant professor at the University of Notre Dame. And then, after an academic life that had consisted of immersing myself in books and articles, I found that I was in for a little surprise.

Perhaps I had come up with interesting ideas, but nobody knew about them. I quickly learned that I had to send these ideas out as articles to journal editors and books to publishers and respond to the referee reports I received in return. Also, I had to present them at academic conferences and other universities and interact with the colleagues in my field. And I started organizing my own conferences and coordinating my research area for international associations. I had to subscribe to listservs, design webpages, and stay on top of the correspondence I received via e-mail and snail mail. I was asked to write book reviews and referee reports. In addition, I discovered that I was expected to apply for grants through organizations such as the National Science Foundation. And my academic training had prepared me for none of this. On top of these research activities, I had to teach undergraduate and graduate students, serve on thesis committees, learn about technology in the classroom, and counsel my students in not only the academic aspects of their life, but sometimes the personal ones as well. At the same time, I had a hard time juggling the direct demands I received from my students with the more indirect research activities in which I was engaged. Finally, I was expected to perform service duties for the department, which included selecting incoming graduate students, interviewing job candidates, and participating in honesty committee hearings.

Clearly, something was wrong with the picture I had had of academia. I was not an isolated individual developing ideas that would somehow be picked up by others. Instead, I was a social being playing a role in the social construction of economic knowledge. Naturally, this realization also influenced my perspective on the history of economic ideas. Perhaps there was more to the history of economics than its ideas. Maybe there was no unambiguously progressive path from the past to the present. Possibly there was an intricate connection between economists' lives and works. These are the kinds of considerations that are addressed in Jan Golinski's (1998) *Making Natural Knowledge: Constructivism and the History of Science*.

## II. MAKING NATURAL KNOWLEDGE

Golinski's "aim is to explore the implications of what I have called a 'constructivist' view of science for the question of how its history is to be written" (p. ix). According to Golinski, constructivism "draws attention to the central notion that scientific knowledge is a human creation, made with available material and cultural resources, rather than simply the revelation of a natural order that is pre-given and independent of human action" (p. 6). In other words, there is more to science than the isolated ideas I had learned during my graduate education.

Golinski traces the rise of constructivism back to arguments in the philosophy

and sociology of science that emerged in the 1960s and 1970s and outlines the subsequent development of constructivism from Thomas Kuhn's paradigms to Barry Barnes and David Bloor's so-called Strong Programme to sociology of scientific knowledge to Michel Callon and Bruno Latour's actor-network theory. Instead of dwelling on the fierce theoretical debates among these different proponents of constructivism, Golinski focuses on the practical implications of constructivism for historical studies of the scientific community and of the place of individuals within it. In particular, Golinski singles out five themes through which constructivism connects with historical research.

First, constructivism has brought the issue of the locations in which scientific knowledge is produced to the attention of historians of science. In addition to analyzing local specificity of knowledge, therefore, historians of science can also evaluate how the knowledge constructed by scientists travels into an external realm. Second, constructivism has pointed out that science is a linguistic activity, embodied in a variety of different kinds of discourse. This allows historians of science to study not only the rhetorical construction of scientific discourse, but also the processes of interpretation undertaken by actual audiences in specific contexts. Third, constructivists have argued that because it is a practical activity as well as a linguistic one, science employs resources to create knowledge. Hence, historians of science ought to consider not only interventions in the form of manipulations of materials and apparatus, but also the nondiscursive means of representation employed by scientists. Fourth, the way in which scientific knowledge acquires authority in general culture has been highlighted by constructivism. This allows historians of science to examine the diversity of the cultural elements of which the extensive networks that enable scientific facts and artifacts to travel are comprised. Finally, constructivism influences the kinds of narratives that may be produced by historians of science. Instead of allowing them to tell simple stories of scientific progress, it forces them to admit to the artifactual quality of their narratives.

Though I would rather focus on the practical implications of *Making Natural Knowledge* for historians of economics, I cannot keep myself from making a few critical comments. To be sure, Golinski protects himself against a lot of criticism by including many disclaimers to the effect that his book is not intended to provide an exhaustive survey of the intricate interconnections between constructivism and history of science. Still, he cannot avoid the sharp debates about symmetry among constructivists. Let me give a brief overview of these.

The problem of establishing a symmetric or an asymmetric relationship between the analyst and the actor is one of the hottest areas of controversy in constructivism (see McMullin 1992a, 1992b; Mulkay 1979; Pickering 1992a, 1992b). After the growing confidence with which scholars had argued that natural scientific knowledge is a social construct, the next step was an increased interest in the consequences of applying this same argument to knowledge generated by the social sciences. In particular, Malcolm Ashmore (1989) and Steve Woolgar (1988, 1992) questioned how constructivists can claim to give an objective account of science if they deny a similar ability to natural scientists in their pursuit of natural knowledge. In their explorations of reflexivity, Ashmore and Woolgar challenged the assumption that the science studies scholar enjoys

a privileged position *vis-à-vis* the subjects and objects that come under the authorial gaze. If the analyst of science and the scientist were truly distinct epistemologically, the inquirer would have no way of knowing the characteristics of the latter in advance of studying it. If, on the other hand, the means of study and the object of study are not distinct in an epistemological sense, their interdependence suggests that our research process assumes the answer it sets out to find. The researcher is required to participate, in the course of her research, in activities that are also the object of that research. She produces knowledge claims about the production of knowledge claims; she aims to explain how explanation is done, to understand how understanding is produced, and so on.

Reactions to this investigation of symmetry in constructivism were varied. According to its advocates, reflexivity may provide an occasion for exploring new ways of addressing long-standing questions of knowledge and epistemology. The reflexivity approach self-consciously seeks to capitalize upon the strains and tensions associated with all research practice that can be construed as part of its own phenomenon. The idea is to take such tensions as the starting point for exploration of the questions and issues that arise, to use them to direct our attention to, say, the particular form of subject-object relationship that our research conventions reify and reaffirm. This gives us the capacity to revisit taken-for-granted assumptions that underpin particular phases or research perspectives. One source of antipathy to the reflexive project was the assumption that such work is incompatible with good research practice because of its self-regarding quality or because it leads to a regress of metastudies (see Pickering 1992b, pp. 16–22; Collins and Yearly 1992a, 1992b). Critics claimed that the serious reflexivist just leaps into the skeptical regress of deconstruction without a parachute and is left with nothing constructive or positive to say. It has no message about anything apart from itself. Its signposts lead nowhere. There is nothing to stop this philosophically progressive regress, which has led some science studies scholars to argue that we should all appreciate the potential endlessness of the this regress, while being content to recognize the problem and deal with it pragmatically.

Whether he likes it or not, Golinski is embroiled in this symmetry debate in at least two instances. First, though he sets out to balance constructivist sociology and historical narrative, in other words, to treat the two symmetrically, the scales are tipped in favor of the implications of the former for the latter with little or no discussion of the implications of historical narrative for constructivist sociology. Golinski seems to be on the side of the critics of reflexivity, despite his claim that one “is obliged to make some acknowledgment of the problems of reflexivity that are consequent upon the historian’s own position within the hermeneutic circle” (p. 132). Second, recall that, for Golinski, constructivism “draws attention to the central notion that scientific knowledge is a human creation, made with available material and cultural resources, rather than simply the revelation of a natural order that is pre-given and independent of human action” (p. 6). In other words, Golinski does not treat the social and the natural symmetrically. Instead, he privileges the social, without defending this asymmetric position for the social, despite his claim that “Pickering’s vision seems an important one for

historians to ponder” (p. 45). I will return to this in the following section. For now, let me just note that Andrew Pickering’s vision is based on treating the social and the natural symmetrically.

Rather than dwelling on these theoretical criticisms, it is much more productive to explore the practical implications of Golinski’s book for historians of economics. After all, Golinski urges: “The value of the view I offer has to be judged by its utility” (p. x). However, where does this utility come from? And is the value the same for everyone? As historians of economics have illustrated, the interpretation of value and utility is not universal, which complicates the use of these ideas as a serious analytical device. And does Golinski suggest that science can be equated with knowledge and knowledge with information, while simply presuming that the treatment of knowledge is unified within economics? Instead, probably the most contentious debate within economic theory revolves around the proper treatment of learning and information. From “rational expectations” to bounded rationality, from Claude Shannon’s information theory to the “complexity” of computational capacity, from Bayesian statistical inference to evolutionary learning, from the treatment of “common knowledge” in noncooperative game theory to negotiated understandings in cooperative game theory, differences of opinion concerning the economics of science can often be resolved down to differences in the theoretical approach to knowledge (Mirowski and Sent, forthcoming). Instead of getting embroiled in this discussion, let me follow up on my promise to explore practical implications rather than elaborating on theoretical disagreements.

### III. MAKING ECONOMIC KNOWLEDGE

As noted in the previous section, Golinski’s definition of constructivism is rather unfortunate in that it does not treat the social and the natural symmetrically. Instead, his appeals to Andrew Pickering’s work are much more promising in providing practical implications for historians of economics. For, much like Golinski, historians of economics cannot avoid the controversies surrounding symmetry. If historians of economics are asymmetric with economists, then how can they provide an intimate view? And on what is their privileged position based? On the other hand, if historians of economics are symmetric with economists, then how can they provide a cool assessment? And why would we take their word over that of economists? Fortunately, Pickering’s (1992b, 1993, 1995a, 1995b) framework allows historians of economics to circumvent these problems as a result of the fact that it not only balances human and nonhuman agency but also gives an account of the emergent structure of economists’ research.

First, the framework argues for a so-called posthumanist decentering of the human subject. Next to economists, there is also a role for nonhuman agency, including tools, concepts, models, and so on. While economists are agents who are doing things in the world, the world consisting of data, theories, and techniques “fights back” when economists attempt to control it. While economists make so-called free moves in an attempt to satisfy their interests, they are

not in full control, as witnessed by the resistances called forth by the so-called forced moves. Elaborations with respect to the data, theories, and techniques used unveil the difficulties economists encounter in serving their interests. As a result, the framework captures both the structure and the contingency of economists' research. The structure follows from the insight that the world could only resist economists by virtue of their moves to achieve their goals. The contingency follows from the realization that economists' attempts at accommodation through new moves to achieve their goals evolves reciprocally due to the emergence of resistance. The interlinking of human and nonhuman agency points toward the importance of a culturally situated historicism.

Second, the framework captures the temporally emergent structure of economists' research. Economists are oriented toward some future goal that does not presently exist. In their attempts to satisfy their interests, the nonhuman agency that economists encounter is temporally emergent in practice, because its exact shape is not known in advance, needs to be explored, and gives rise to problems that economists have to solve. That is, it does not arise from features that were already there, but emerged in economists' scientific practice. As a result of the temporal emergence of resistances and accommodation, economists' different interpretations of their work are temporally specific. Therefore, a serious analysis of the temporality of economists' practice further points toward a historicist understanding of economists' scientific knowledge. Hence, their journey of discovery is located in real-time history, being neither free of nonhuman agency nor predetermined.

These two characteristics, the mix of human and nonhuman agency and the temporality of practice, justify historians of economics writing about economists. As agents not involved in the process, they can follow economists through real-time practice. They can track the interlinking of human and nonhuman agency in, and the temporal emergence of, the structure of economists' practice. Of course, after the fact, economists can and do offer highly persuasive technical accounts of why their work has developed in specific ways. Yet, for the purposes of real-time accounting, the substance of such retrospective accounts is one aspect of what needs to be analyzed. It would be antithetical to Pickering's framework to bow to economists and project their retrospective accounts backward in time as part of our explanation. While economists are led to reflect on their own agency, historians of economics can understand their practice as a mixing of human and nonhuman agency.

This kind of framework allows historians of economics to explore the themes developed by Golinski. First, where is economic knowledge produced and presented to appropriate audiences? How did Herbert Simon's affiliation with the Cowles Commission shape his work? What was the influence of his stay at the Rand Corporation on John Nash's research? What happens when economists like Joseph Stiglitz, Lawrence Summers, and Laura Tyson move from their ivory tower to the public arena? Second, which literary means do economists employ to persuade their readers and how can their texts be interpreted? Why is the general public eating out of Paul Krugman's hand? Why does nobody read John Muth's articles, while at the same time they claim to understand his contributions? What could a rhetorical and hermeneutic interpretation of Deirdre McCloskey's

work reveal? Third, what is the role of material practice in experimental economics and how are research findings visualized? How does the experimental economics of Vernon Smith relate to abstract theoretical work? What are the practical implications of tests with rats in experimental economics? Where does economists' fascination with phase-diagrams come from? Fourth, what is so special about the culture of economics and what means do economists employ to establish and extend their networks? Why do Chicago economists find jokes about Keynesian economics hilarious? Why is a publication in the *American Economic Review* so sought after? How did Alan Greenspan become chairperson of the Board of Governors of the Federal Reserve? Finally, what is the role of narrativity in history of economics? In a sense, historians of economics need to adopt three languages (see Gale and Pinnick 1997; Merz and Knorr Cetina 1997b): participant's language (economists report in their own language through the use of quotations), observation language (a clarification of economists report in historians' language), and explanatory language (an account of the balance of human and nonhuman agency and the emergent structure of economists' research using the dialectic of resistance and accommodation).

#### IV. CONCLUSION

I have a confession to make. The history with which I started this review, though truthful, highlights certain aspects of my career and leaves out others. Now, this confession should come as no surprise. Much like economists constructing stories about the economy, I am a historian constructing stories about history. Much like economists trying to persuade others, I attempt to draw in my potential audience through the story I construct. It is for my discussants, conference audiences, readers, editors, and referees to judge whether they find this story convincing.

As historians of economics, we had better get used to constructing narratives about how economics is temporally emergent through an interlinking of human and nonhuman agency. In addition to considering what Adam Smith meant, we need to look at how his ideas were received. In addition to elucidating David Ricardo's thought, we need to consider his practice. In addition to exploring Karl Marx's theories, we need to examine his culture. In addition to focusing on these "big men," we need to look at the "little women." In addition to studying Herbert Simon the individual scientist, we need to consider Herbert Simon the social being. And Golinski's *Making Natural Knowledge* points the way to how we may go about developing these kinds of narratives. To be sure, historians of economics such as Robert Leonard, Philip Mirowski, and Roy Weintraub are starting to explore the making of economic knowledge. Hopefully, there will soon be enough material to survey it in a book called *Making Economic Knowledge*.

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