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10 Constructivism: the social construction of scientific knowledge

D. Wade Hands

Introduction

Philosophers have traditionally approached the subject of scientific knowledge from the 'What is ____?' perspective. 'Scientific Knowledge' of course fills the blank in the most general case, but a variety of more specific expressions have been inserted to cover various special topics that have been of interest to philosophers of science: 'explanation', 'testing', 'scientific inference', etc. This approach to science, scientific knowledge, and related topics, of course reflects the way that philosophers have traditionally approached most subjects of inquiry: 'What is Truth?', 'What is Beauty?', 'What is the Good?' ... While this approach to scientific knowledge is part of a grand philosophical heritage, and perhaps even has a certain edictal charm, it frankly makes it rather difficult to understand much of the work that goes on within contemporary science theory: particularly the work informed by social constructivism and its cognates.

The social constructivist view of scientific knowledge is much easier to understand if we begin with a different question than the standard philosophical point of embarkation. Rather than asking 'What is scientific knowledge?', it is more useful to begin an inquiry into the social construction of scientific knowledge by asking the question 'What determines scientific beliefs?' Scientists, qua scientists, clearly hold a wide array of different scientific beliefs. Some of these beliefs are rather mundane and are widely accepted outside of the scientific community (like the belief that the melting point of copper is 1083°C, or that the speed of light is 186000 miles per second), while others may be shared by almost every contemporary scientist and yet remain controversial within certain segments of the wider social community (like the belief in the Big Bang as the origin of the universe, or Darwinian random variation and selective retention as the explanation for the particular characteristics of homo sapiens). In addition to these widely held scientific beliefs, scientists also hold other beliefs that are much more local in nature; some of these are exclusive to members of a particular scientific group who share a common research program (punctuated equilibrium theory for example), while others are shared by almost no one outside of a specific research community (cold fusion for example). The

best way to understand the social constructivist view of scientific knowledge is to start with the question of what determines such beliefs.

Notice how different the question 'What determines scientific beliefs?' is from the question 'What is scientific knowledge?' First of all, the traditional philosophical question begs for an essentialist answer: 'What is scientific knowledge essentially (i.e. really)?' Like the question 'What is Truth?' or 'What is the Good?' the standard philosophical approach to scientific knowledge begs for an answer in terms of the underlying *essential nature* of the subject matter in question. The modus operandi for answering such philosophical questions has traditionally been the method of conceptual analysis: the method of rational (armchair) philosophical speculation. The question 'What determines scientific knowledge?' elicits a more *naturalistic* response. It is more like the question 'What determines a solar eclipse?' or 'What determines the scents of various orchids?' or 'What determines the rate of inflation?', than the question 'What is Truth?' Although certain variants of scientific realism would link the answers to such naturalistic questions to the underlying essential nature of the objects of inquiry, it is no more necessary to forge such linkage in this case (scientific knowledge in general) than it is in the context of any more specific scientific investigation.

Second, notice that the constructivist question shifts the responsibility for what counts as 'scientific' onto the relevant (in this case scientific) agents. For the traditional approach, the domain of inquiry is circumscribed by the (philosophical) inquirer; in the constructivist framework the domain of inquiry is circumscribed by the subjects themselves (the scientists). Of course in order to conduct such an investigation one must still establish who counts as a 'scientist' before their beliefs can become the subject of inquiry, but this is fairly easy to establish since both the scientists and those conducting the inquiry are members of a wider social community where there is general agreement about who does and does not count as a 'scientist.' This is of course not the case for the traditional philosophical approach, since the very purpose of such an inquiry is to delineate the 'scientific' in a (philosophical) way that is *different* from the way that is accepted by non-philosophers and others within the wider social community. One does not need philosophers of science to establish the essential character of the 'scientific' designator, when its only purpose is to designate the professional beliefs of those who everyone agrees are 'scientists.'

The constructivist literature discussed in this chapter draws on resources from a wide variety of earlier and ongoing intellectual traditions. Some of these resources are also shared by those working in contemporary philosophy of natural science, but many are unique to the social constructivist approach. The Mertonian literature on the 'sociology of science' – beginning with Robert K. Merton's 1935 doctoral dissertation (Merton 1970), and continuing to the present day by sociologists of science guided by Mertonian functionalism – is certainly

one of the approaches that has significantly influenced the constructivist literature on scientific knowledge. A second important influence was the 'sociology of knowledge' associated with the work of Karl Mannheim (1936) and others in the late 1930s. A third set of influential ideas involves the so-called Bernalist literature associated with the work of John Desmond Bernal (1939) and other (primarily British) Marxist historians of science during the 1940s and 1950s. Finally, but perhaps most importantly, Thomas Kuhn's *Structure of Scientific Revolutions* (1970) not only had a profound impact on the development of the constructivist literature, but also on the history and philosophy of natural science more generally: 'After Kuhn, philosophy of science would never be the same' (Callebaut 1993, p. 12). While Kuhn was clearly not the first to note that science in general, and specific scientific communities in particular, are actually *social communities* and that the social character of these communities conditions the observations, theorizing, and day-to-day practices of the scientists within them, his work was crucial to the spread of such ideas among contemporary science theorists. Now almost everyone writing in science theory agrees that science is fundamentally social and that understanding the character of that sociality is essential to understanding scientific knowledge.

While these, and a variety of other ideas (pragmatism, hermeneutics, postmodernism, feminism, ...) have influenced the constructivist literature on scientific knowledge, no attempt will be made to review these ideas in the following discussion. Doing justice to any of these topics is clearly beyond the scope of the current project and detailed discussions of each, and the impact of each on the constructivist approach, are available within the existing literature (see for example Barnes 1982; Barnes et al. 1996; Biagioli 1999; Collins and Restivo 1983; Golinski 1998, Hands 1997a, 2001a; Jasanoff et al. 1995; Pickering 1992, 1995; or Shapin 1982, 1988, 1992). The following discussion will focus on the social constructivist literature itself – or more realistically a small, but defining, portion of the constructivist literature – and how that work relates to disciplinary economics.

So what is this social constructivist literature? And who exactly is, and is not, a social constructivist? Given that social constructivism emerged out of the fusion of various aspects of so many different sets of ideas and approaches, it might seem a bit presumptuous to even attempt to identify a particular body of research that constituted the 'origin' of what is now called the social constructivist approach to scientific knowledge. Nonetheless, that is exactly what I would like to do. If constructivism has a point of origin, it is clearly the early work of the *Strong Program* (or Edinburgh School), particularly the contributions of Barry Barnes (1977, 1982) and David Bloor (1976/1991, 1983) in the late 1970s and early 1980s. Intellectual material came from a vast array of different sources, and the constructivist genesis spawned innumerable (and often rather contemptuous) offspring, but the Strong Program nonetheless represents

the obligatory passage point for all the ideas that funneled into, and ultimately came out of, the social constructivist approach to scientific knowledge. The next section will examine the Strong Program in some detail and also briefly indicate some of the vast and varied constructivist literature that has appeared in the decades since the first work of the Strong Program. The second section will examine the numerous points of contact between these various social constructivisms and economics.

Belief, knowledge and social construction

Let us begin by returning to the question of what determines the beliefs of scientists. Talking about the 'beliefs of scientists' is of course *not* the way that philosophers of science would frame the subject of 'scientific knowledge.' But suppose they did. In other words, suppose one could somehow persuade philosophers of science to reformulate what they have traditionally said about scientific knowledge as an answer to the question of what determines the beliefs of scientists. What would they say? The answer would probably come in two parts; the first part would involve the *scientific method* and the second would involve *nature*. The story would be that *if* the scientific method is properly followed, then *nature* will determine the beliefs of scientists. In fact, the scientific method is nothing more than a particular set of procedures that allows scientists to see/hear objective nature in a way that other human procedures (prayer, poetry, auto-mechanics, Hegelian philosophy, karaoke, ...) do not allow the practitioners of those methods to see/hear objective nature. According to traditional philosophy of science (and the scientists themselves) *nature determines the beliefs of scientists*, or at least the beliefs of scientists who have correctly applied the scientific method.

The traditional belief-determination-by-objective-nature story itself actually comes in two different versions: realism and instrumentalism. The scientific realist version is most familiar; scientists believe in electrons and genes because *there are* electrons and genes, and the scientific method makes it possible for these existent things to be discovered. According to scientific realism, objective nature not only determines the beliefs of scientists, the theoretical beliefs of successful scientists are successful precisely because they are in fact true of nature. The instrumentalist version also has nature determining the beliefs of scientists, but at a slight remove; scientific beliefs facilitate the efficient organization and categorization of empirical evidence – the observational signals received from nature. The scientific method allows scientists to construct theories that organize and classify nature's signals – save the phenomena – in predictively efficient ways. In either case objective nature is ultimately the cause of scientific beliefs, it is just that in one case the beliefs are about the way that nature really is, while in the other case the beliefs only help organize the observational evidence that nature conjures up. In either case *if* the beliefs of

scientists are determined by *social* factors – social interests, social forces, social conditioning, etc. – then they are not truly scientific beliefs. Some scientists have allowed their beliefs to be determined by social forces rather than objective nature – Nazi eugenicists, Lysenkoists in Soviet biology, and those extolling Creation science, for example – but these are paradigm cases of erroneous (or non-)science. According to the traditional philosophical view, proper scientific beliefs are determined by nature; if social conditioning plays a role then the resulting scientific beliefs are either wrong, or simply not science at all.

The Strong Program and other social constructivists turn this traditional argument on its head. If one were studying the beliefs of a particular premodern culture – say the belief that the god Zarwa causes the crops to grow – one would never consider the possibility that the reason people in the society believe such things is because in fact Zarwa *does* cause the crops to grow (or sends off observational signals to that effect). One would explain such beliefs in *social terms* – perhaps in terms of the function that such beliefs serve in reinforcing solidarity within the society, or the perpetuation of the social interests that are served by such beliefs, or in some other fundamentally social way: but in any case the beliefs would be socially determined, not determined by the actions of the posited deity. Now suppose the culture under investigation is not in an isolated jungle, or in history, but exists within a modern scientific laboratory. Why should the explanatory strategy be any different? Why should sociologists stop seeking social explanations just because the society under investigation moves from the jungle to the science building of a modern university? When social scientists study the determination of the beliefs of any set of acculturated individuals – and since Kuhn's work, most studies of science definitely treat scientists as products of scientific, or a scientific paradigm-specific, acculturation – the resulting explanation of those beliefs is entirely in terms of social forces. Of course if the beliefs of scientists are explained socially, then they are *not* being explained in the way that philosophers of science, scientists themselves, and the (scientifically educated) general public explain them: that is by nature. In the words of one critical philosopher, this means that for social constructivists 'inputs from nature are impotent' (Kitcher 1993, p. 164).

This was essentially the position of the original Strong Program sociologists, and it continues to be a defining insight for much of the social constructivist literature. Scientific beliefs, like the beliefs of any other social agents, are socially constructed; they are the products of the particular social conditions, interests, influences, structures, and so forth, that are at work within (and around) the scientific community. While the social determination of scientific beliefs is a characteristic feature of constructivist science studies, there is much less agreement on exactly how this 'social construction' takes place. Is it class interest that determines scientific beliefs? Or professional interests? Or individual career goals? Or the structure of the operative social institutions?

Or the existing conditions of social power and domination? Or the function that such beliefs play in the overall reproduction of social life? Or ... It seems that there are as many possible stories about exactly *how* scientific beliefs get socially determined as there are different approaches to social explanation in general. According to the Strong Program, scientific beliefs are explained by *social interests* – the scientists' place in the overall pattern of social relations – but other constructivist approaches employ different explanatory frameworks. For the Strong Program, this type of science studies – explaining scientific beliefs on the basis of social interests – is not an epistemologically radical approach to inquiry. It is just the application of relatively standard techniques from social science to a particular domain of social inquiry: natural science. In David Bloor's words, the 'search for laws and theories in the sociology of science is absolutely identical in its procedure with that of any other science' (Bloor 1991, p. 21). Of course, not every constructivist sees the social study of science as simply the application of the scientific method to the subject of scientific knowledge, but that has remained the main focus of the Strong Program.

The body of literature produced by such sociological-based studies of scientific knowledge has come to be called the sociology of scientific knowledge (SSK), and the Strong Program is just one example, though perhaps the most influential example, of this sociological literature. For many, SSK is simply the application of science to the study of science, but even for those who are less scientistic (and perhaps more radical) SSK is simply the application of familiar explanatory strategies from social and human inquiry to the particular question of explaining the beliefs of natural scientists. While such constructivist approaches do *not* necessarily exclude the possibility of other, non-social, perhaps even natural, factors also playing a role in the determination of scientific beliefs – Bloor is quite explicit about the non-exclusivity of the social component: 'It does not say that it is the only component, or that it is the component that must necessarily be located as the trigger of any and every change; it can be a background condition' (Bloor 1991, p. 166) – they are frequently interpreted to be saying that scientific beliefs are socially determined *without remainder* (i.e. without nature playing any role). Some of the responsibility for this 'nature has nothing to do with science' interpretation of SSK should be assigned to the more radical authors within SSK, many of whom *do* in fact want to argue that nature has nothing to do with science; some of the responsibility should be assigned to less radical authors (like those in the Strong Program) for neglecting to emphasize the non-exclusivity of their social explanations; and finally, some of the responsibility should also be assigned to critics who choose the most extreme readings because they are the easiest to attack. In any case, regardless of how one assigns responsibility, the fact is that SSK is often characterized as claiming that *only* social forces have *any* role in the determination of scientific beliefs. That is not the view of the Strong Program,

but nonetheless it has become a standard interpretation of the SSK position (and in some cases it is accurate).

Returning specifically to the Strong Program, suppose that one were to undertake an investigation into the social forces determining certain beliefs held by a particular group of scientists. What methodological rules would be most appropriate for such an investigation? First, it seems that one would need to commit to providing a *causal explanation* of the relevant beliefs; one would want to explain the cause of the particular scientific beliefs. Let's call such a maxim *causality*. Second, since one desires a social explanation of such beliefs, one would require *impartiality* about whether the beliefs are true or false, rational or irrational, etc. Going back to the case of the god Zarwa, one would want the social explanation of such a belief – an explanation in terms of the role that such a belief plays in the overall pattern of social relationships – to be the same whether there is in fact a god Zarwa or not. Third, not only should those engaged in SSK be impartial between the social explanation of true and false beliefs, the resulting social explanations should be *symmetric*; the same type of causes (social interests, factors, conditions, relationships, etc.) should be at work in the explanation of true and false beliefs. The discovery that Zarwa actually exists should leave unscathed the sociologist's explanation of the social role of Zarwa-belief within the community of Zarwa-believers. So too for similar discoveries within the community electron-believers, gene-believers, or utility function-believers. Finally, since the social scientists doing the SSK are themselves scientists – albeit social, not natural, scientists – the explanations offered for scientific beliefs should be *reflexive*; they should apply equally well to those who are actually doing SSK.

According to David Bloor's influential statement of the Strong Program, *Knowledge and Social Imagery*, these four tenets – causality, impartiality, symmetry, and reflexivity – essentially *define* the program's approach to the study of scientific knowledge (Bloor 1991, p. 7). Although recent restatements of the Strong Program – (Barnes et al. 1996 in particular) – have criticized the 'methodological idealism' of contemporary SSK, and have intentionally reopened the door for nature (or at least our experiences of nature) to play a significant role in the determination of scientific beliefs, Bloor's original goal of providing causal, impartial, symmetric, and reflexive explanations for the *social determination of scientific beliefs* remains the defining strategy of the Strong Program and many other approaches within the sociology of scientific knowledge.

While the Strong Program may have been the first systematic and self-conscious research program within SSK, it did not remain alone for very long. Beginning in the late 1970s, the SSK literature, as well as the science studies literature in general (where science studies is a broader category that includes a vast array of different approaches that draw inspiration from discourse theory,

cultural studies, classical rhetoric, neopragmatism, feminism, and a host of other traditions, in addition to the primarily sociology-based SSK) has exploded onto the intellectual landscape. In addition to the constructivist research that appeared as an immediate response to the original Strong Program (for example Collins 1985; Knorr Cetina 1981; Latour and Woolgar 1979/1986; and Latour 1987) and the work of later Strong Program-inspired authors (MacKenzie 1990, 1998, 2001; Shapin and Schaffer 1985, and Shapin 1994 for example), the field has also been populated by a number of different approaches that, while clearly inspired by social constructivism, also deviate from it in sufficient ways to warrant their own individual labels. These literatures include the reflexive school (Ashmore 1989 and Woolgar, 1988 for example), Actor Network Theory (Callon et al. 1986 and Latour 1993, 1999, for example), the mangle of practice (Pickering 1995), the rhetoric of science (Gross 1990 and Gross and Keith 1997, for example), science as discourse (Gilbert and Mulkay 1984 and Lynch 1985, for example), certain types of feminist science studies (Haraway 1991, for example), as well as some of the literature emphasizing the role of instruments and technical equipment in science (Galison 1987 and 1997, for example). Perhaps it goes without saying that social constructivism has also generated an extensive critical literature, most of it written by philosophers of science, but even some practicing scientists have joined the fray (Gross and Levitt 1994 and Gross et al. 1996 for example). While each of these various constructivist literatures is interesting and important enough to warrant a detailed discussion, I will not provide it here (a number of detailed sources were cited above). At this point I will leave the reader to their own investigation of the various renditions of social constructivism (and its critics), and turn to the question of the relationship between the constructivist literature and the particular science of economics.

The discussion of SSK and economics will be divided into two parts. The first part will briefly note a few of the many indirect points of contact between the two literatures. The second set of topics involves more direct, self-conscious, contact and will be examined in more detail.

Economics, the history of economic thought, and social construction

One area of indirect contact between economics and social constructivism concerns the role of 'the economy' in constructivist histories of science: the constructivist-based literature in the history of natural science that emphasizes the relationship between the ideas and values of the scientific community and the surrounding (or underlying) economic conditions. Of course it is always dangerous to try to draw a very crisp line between 'the economy' and 'disciplinary economics'. Is 'the British economy in the 1840s' something totally separate from 'Ricardian economics'? Or is the 'New Deal' totally independent of Keynesian and/or Institutionalist economic theory? Of course not. But even though the economy and the associated economic ideas involve deep and

fundamental interdependencies, they can often be separated for the purpose of certain types of analysis; if the subject is the work of Charles Dickens, the emphasis is 'more' on the economy (than Ricardian economics), and if the subject is the work of William Whewell it is 'more' on Ricardian economics (than the economy). So too for the impact of 'the economy' and 'disciplinary economics' on the construction of scientific knowledge. The impact of economic conditions on the development of science was of course the main subject for the Marxist historians of science like J.D. Bernal (1939) and Boris Hessen (1931), but there is also an extensive contemporary literature that links various aspects of science and the scientific community to particular economic, actually political economic, conditions. This literature draws inspiration from a vast array of sources, and elaborates a wide range of different connections, but they all in some way link the emergence of scientific knowledge (or particular forms of scientific knowledge) to particular economic conditions. Examples of such work include Hadden (1994); Poovey (1998); Smith and Wise (1989); and to a lesser extent Shapin and Schaffer (1985) and Shapin (1994).

Another point of indirect contact stems from the fact that much of the 'social analysis' employed within the SSK literature looks, prima facie, more like economic analysis than sociological theory. Sometimes the particular economics involved in SSK is heterodox in orientation (particularly Marxist), and sometimes it looks a lot more neoclassical or rational choice-theoretic, but in either case, some version of (disciplinary) economics informs (implicitly or explicitly) much of the work of those writing within SSK. Numerous authors have examined this connection in detail (Mäki 1992; McClellan 1996, and Mirowski and Sent 2002 among others), and I have discussed it in a number of previous works (Hands 1994, 1997a, 2001a and 2002).

The final indirect connection, concerns economic methodology. While SSK is only one of many forces that contributed to the demise of the Received View within mainstream philosophy of science, it was certainly an important factor. Given the profound impact that the demise of the Received View (and the associated falsificationist version of Popper's philosophy) has had on the literature in economic methodology during the last thirty or so years, SSK's role in that demise certainly suggests that SSK has also had a significant impact (at least indirectly) on the complexion of recent methodological writing about economics. Once one is exposed to the SSK literature, it is very difficult to accept the standard philosophical vision – the view that the philosophy of natural science provides the *rules* for the proper conduct of scientific inquiry (rules that are relatively simple, universal, and provide adequate epistemological grounding for the resulting science) – as the only game in town for understanding the character of scientific knowledge: natural or economic. This is also a topic examined in more detail in Hands (2001a).

In addition to these, and perhaps other, relatively indirect contacts, there seem to be two areas where the intellectual border crossing has worn a much deeper trail. The first is the literature on the 'Economics of Scientific Knowledge' (ESK) and second involves using SSK as a resource for the history of economic thought. Let us consider ESK first.

If science is fundamentally social and should be understood as such, then why not employ the resources of the social science of economics rather than functionalism, interests sociology, social psychology, or one of the other sociological approaches employed in SSK? In many respects economics seems to be a 'natural' for the job. On one hand, economists are particularly ambitious (some would say aggressive or imperialistic) in their efforts to apply economic analysis to various social phenomena outside their traditional subject domain of prices, markets, consumers, and firms. The literature on the economics of the family, law and economics, and public choice theory, are just a few of the many examples of this general – let's see if we can explain it as the equilibrium outcome of the actions of rational agents with well-ordered preferences – approach to various social phenomena. Why not the economics of science? Since this literature touches on a number of substantive philosophical issues, I previously (Hands 1994) emphasized the distinction between the 'economics of science' (analogous to Mertonian sociology of science) and a more philosophically engaged 'economics of scientific knowledge' or ESK (analogous to SSK), and while this is a useful distinction for many investigations, it does not seem to be necessary to pursue it in the current context. Here I will use the term ESK for both, with the only relevant distinction being the difference between the minimally philosophical literature produced by economists, and the more self-consciously philosophical literature written by philosophers of science. See Dasgupta and David (1994); Diamond (1996); Mirowski and Sent (2002); Sent (1999); Shi (2002); Stephan (1996); and Wible (1998) for a range of different perspectives on the economist-produced side of this rapidly growing literature.

The main focus of the philosophers doing work that might be labeled ESK, has been to recruit economics into the battle against the relativism, particularly SSK-inspired relativism, of recent science theory. One of the main themes of the later SSK literature has been to undermine or 'debunk' the traditional philosophical (and scientists' own) view of the epistemic and/or cognitive privilege of science. If science is social all the way down, then it is literally 'just like' other aspects of social life, and is thus denied the special epistemic place that it has traditionally been assigned within the post-Enlightenment world. This role of SSK – essentially kicking the epistemic pedestal out from underneath natural science – has not been (surprise, surprise) particularly well-received by either philosophers of science or by the scientists themselves. Philosophers of science were relatively quick to notice that economics might be an effective

tool for mounting a response to this debunking and relativist aspect of the SSK literature.

Even if one accepts that science is social, and that scientists do not actually follow the methodological rules set down by positivist or Popperian philosophers of science, the lesson one gets from economics is that the resulting scientific knowledge may still be (epistemically) just fine. The professional reputation of the economics has been built on the construction of economic models, often intimidatingly mathematical models, that show how it is possible for the right stuff (economic efficiency, Pareto optimality, social welfare ...) to emerge from the actions of self-interested individuals with even the worst of motives. This seems to be the perfect counter to the potential relativism of SSK; accept along with Kuhn and others that science is fundamentally social and that scientists do not follow 'the' scientific method, but then show the social institutions of science are such that these sullied activities produce legitimate scientific knowledge (cognitive efficiency) anyway. As Philip Kitcher, a philosopher of science who has employed economic resources in this way, summarizes his argument:

> Much thinking about the growth of science is permeated by the thought that once scientists are shown to be motivated by various types of social concerns, something epistemically dreadful has been established. On the contrary, as I shall repeatedly emphasize, particular kinds of social arrangements make good epistemic use of the grubbiest motives. (Kitcher 1993, p. 305)

While a substantial critical response to this anti-debunking philosophical literature has been offered by both economists and philosophers (see, for example, Downes 2001; Fuller 1994; Hands 1995, 1997b, 2001b; Kincaid 1997; Mirowski 1995, 1996; Roorda 1997; Solomon 1995; and Wray 2000, 2001), all that can be said at this point is that the debate remains open regarding the success of these endeavors to recruit economics into the philosophical fight against the relativist implications (most philosophers would say corrosive implications) of SSK. Regardless of how it turns out, the fact is that it represents a body of literature that combines ideas from economics and SSK in a number of new and substantive ways; and yet unlike most of the ESK literature produced by economists, this philosophical literature drops economics squarely into the center of the fray within contemporary science theory.

The second significant point of contact involves the use of SSK, or SSK-inspired historical approaches, in the history of economic thought. Since SSK has been so influential in the recent literature on the history of science – changing both the standard interpretation of major episodes within the history of science, and also shifting the historical focus away from such major episodes and more toward smaller scale, more situated, and more contingent sites of scientific activity – then why not apply a similar approach to the history of economic science? A number of those writing in the history of economic thought have

begun to do precisely that; the relevant papers are too numerous to list (see Hands 2001a, p. 211 for a partial listing), but book-length studies include Klein 1997; Mirowski 1989, 2002; Sent 1998; Weintraub 1991, 2002; and Yonay 1998. There seem to be a number of reasons why such approaches might be, and have been, well-received among historians of economic thought.

For one thing, the whole idea that knowledge is socially constructed seems to be far less radical in a social science like economics than in a natural science like physics. To say that physicists' beliefs about electrons are socially determined is to not only to say something contrary to the view of most philosophers of natural science, it is also at odds with how the general public and physicists themselves view the determination of such physical beliefs. This is less the case in economics. Of course economists' beliefs about, say inflation, are socially determined; even if an economist strictly adheres to the scientific method as laid down by positivism or falsificationism, it is still the case that the numbers involved in the proper scientific determination of such beliefs are *constructed* by human agents to serve human purposes. Even in the (epistemically) best case the source of economists' beliefs come from society (not nature), the relevant empirical evidence is constructed not given, and no such beliefs (about say inflation) would exist at all if it had not been recognized as a substantive social problem about which theories, evidence, and social action were required. Of course economics is social: no society, no economics. Now this still leaves open the question of *proper* versus *improper* social determination – having one's beliefs about inflation socially determined by the (socially constructed) CPI is proper, while having them determined by the political party that paid for the study is not – but the general notion that the beliefs of economists are socially determined is hardly a radical idea.

For another thing, there is a grand tradition within the history of economic thought regarding the impact of social conditions (separate from the social character of the empirical *facts*) on the history of economic thought. How would one tell the story of Ricardo's *Principles* in the absence of the associated (social) story about the class structure of early nineteenth century England and the debate over the Corn Laws? How does one tell the story of the Keynesian revolution without the great depression? Given the proto-constructivist character of so much of the traditional literature within the history of economic thought, the two main changes initiated by the recent spate of SSK-informed studies have been simply to narrow the focus of the subject matter (moving away from the study of major 'revolutions' in economic thought), and to look seriously at the history of twentieth-century, and thus highly mathematical, economic theory (a previously rather Whiggish subject).

Finally, it seems that historians of economic thought might turn to SSK because the philosophy of science and traditional economic methodology

has been so trenchantly unhelpful. The relationship between the history of economic thought and economic methodology is certainly very complex, but the bottom line is that while historians have often looked to philosophy of science (through the conduit of traditional economic methodology) for guidance regarding the character of scientific knowledge, they have seldom been the recipient of anything very useful; the philosophical programs of positivism and falsificationism have provided almost no help on the type of questions that interest historians of economics. These traditional approaches boil the whole continuum of questions about scientific knowledge down to a few simple methodological rules – like 'make bold conjectures and subject them to severe empirical tests' – and such rules offer little help to the historian, whether they have actually been followed by the relevant economists or not. The consensus among economic methodologists is of course that such rules have not generally been obeyed, but for a moment suppose they were. What would the historian do with such information? Such rules, if actually met, would *exhaust* the reasons for why a particular theory was accepted or rejected, leaving nothing else to say about the episode in question – nothing about the relevant personal lives; nothing about the political, social, or even economic context; in essence nothing historical at all. Now suppose that it is discovered (as it usually is in methodological studies) that a 'successful' economic theory did not follow the strict rules laid down by some particular philosopher of natural science. What would be the response in this case? If one remains within the traditional philosophical context all one can do is to reprimand the economists in question for not being 'scientific'; and once the complaint is filed, there is nothing else much to say. Again there is no real reason to do the history of economic thought. On the traditional view, if economists did not follow the rules of the scientific method then the results were not legitimate economic science, and while an investigation into the causes of such erroneous beliefs might be of interest to the social or political historian, they have no place within the history of scientific economics. In either case, whether the rules are, or are not, followed, there seems to be little to guide, or even any particular need for, the history of economic thought. On the other hand, SSK starts with precisely the question of the complex and contingent social determination of the beliefs of (even proper) scientists. It thus seems to be a far more useful framework for understanding the historical development of various economic fields than the framework provided by traditional economic methodology.

To conclude, I have discussed three indirect connections between economics and the social constructivist literature (the role of the economy in the history of natural science, the role of economic analysis within the social studies of science, and SSK's role in helping to undermine rules-based philosophy of science), and also two connections that are more direct, and perhaps more substantive (ESK, and SSK in the history of economic thought). While there are undoubtedly

many other points of contact between SSK and economics, these five subjects certainly cover a large portion of the rapidly growing literature connecting these two overlapping domains of inquiry. Of course the next development, or the next connection, between these two fields is yet to be determined. What is clear, is that there has already been a substantial amount of fertile interaction, and that the interaction will continue to produce interesting and important results for a long time to come.

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