Conjectures and Reputations: The Sociology of Scientific Knowledge and the History of Economic Thought

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> It is our contention, then, that the sociology of knowledge must concern itself with whatever passes for "knowledge" in a society, regardless of the ultimate validity or invalidity (by whatever criteria) of such "knowledge." And insofar as all human "knowledge" is developed, transmitted and maintained in social situations, the sociology of knowledge must seek to understand the processes by which this is done in such a way that a taken-for-granted "reality" congeals for the man in the street. In other words, we contend that *the sociology of knowledge is concerned with the analysis of the social construction of reality.*—Peter Berger and Thomas Luckmann, *The Social Construction of Reality*

1. Introduction

Joseph Schumpeter opens chapter 4 of his monumental *History of Economic Analysis* (1954) with the distinction between *Wissenschaftslehre* (the science of science) and *Wissenssoziologie* (the sociology of science). The former, he says, starts from "logic and to some extent also from epistemology" and concerns "the general rules of procedure in use in the other individual sciences," while the latter treats "science as a social phenomenon" (Schumpeter 1954, 33). Although these two distinctions are still with us and they continue to divide the study of science

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in a useful way, the last twenty or so years have not been good for *Wissenschaftslehre*, while *Wissenssoziologie* seems to have a new lease on life.

The purpose of this essay is to discuss the sociological approach's new lease on life and how it relates to the discipline of economics in general and the history of economic thought in particular. While the main focus will be the sociological turn, it is useful to briefly examine the current situation within the philosophy of science, since its difficulties have provided some of the impetus for the growth of the sociological view. In the study of science, as in science itself, alternative approaches do not gain momentum until chinks have appeared in the armor of the dominant view—and right now positivist-inspired philosophy of science has a lot of chinks.

The paper is arranged as follows. Section 2 provides the backdrop for the sociological turn. There are two subsections to this background material; one discusses the breakdown of the Received View, and the other examines two earlier approaches to the sociology of science: the Marxist and Mertonian traditions. Section 3 examines the two most influential approaches within contemporary sociology of scientific knowledge—the Strong Program and social constructivism—and also contains a section on (self-identified) criticisms and related developments. Section 4 discusses two important contact points between the sociological literature and the discipline of economics: the economics of science and the application to the history of economic thought.

2. Background

2.1 Kuhn and the Breakdown of the Received View

The breakdown of the so-called Received View within the philosophy of science has been an accepted fact of intellectual life for almost thirty years.[1] Even philosophers who would like to reclaim certain aspects of this Legend are currently willing to admit that while "it may continue to figure in textbooks and journalistic expositions, numerous intelligent

1. For most philosophers the Received View (a term popularized in Suppe 1977) means logical empiricism—the positivist-inspired mainstream within mid–twentieth-century Anglo-American philosophy of science—but I will also include Popperian falsificationism under this general rubric. While this is not the way that I personally read the Popperian tradition (Hands 1993), it remains the standard reading among economists and is unproblematic in the current context.

critics now view Legend as smug, uninformed, unhistorical, and analytically shallow" (Kitcher 1993, 5). The breakdown of the Received View is sufficiently well documented that I will not review all (or even most) of the critical arguments that have been leveled against it.[2] What is useful though, is to briefly discuss the three particular criticisms that have contributed most directly to the sociological turn.

The first of these criticisms is not really a single criticism but rather a combination of the two main difficulties that surfaced in the critical literature of the 1960s and 1970s: *theory-ladenness* and *underdetermination*. While theory-ladenness has a long philosophical history (including Karl Popper and some logical positivists such as Otto Neurath) the contemporary version is most clearly identified with Thomas Kuhn's *The Structure of Scientific Revolutions* (1970a). Kuhn argued scientists do not just "see," they "see as," and their scientific paradigm, their shared conceptual framework, determines what they see in what way. The paradigm provides the lens, or interpretative framework, by which various aspects of the world are observed, or in Philip Kitcher's words: there are no "out-of-theory" experiences (1993, 133). Theory-ladenness of course lends support to the more radical claim by Kuhn and others that different scientific theories are "incommensurable." If there is no theory-neutral observation vocabulary, and each theory determines its own domain of observation, then there is no way to directly compare two scientific theories; they are "incommensurable." Underdetermination, or what is often called the Duhem-Quine underdetermination thesis, also has a long history, but its most recent incarnation dates from W. V. O. Quine's "Two Dogmas of Empiricism" in 1951. In a nutshell, the argument is that any scientific theory can be immunized against refuting empirical evidence, that is, that no test is truly definitive. The problem is that no theory is ever tested in isolation. In order to conduct an empirical test, a number of auxiliary hypotheses must be made—hypotheses about the empirical evidence, the testing technique, the values of constants, the boundary conditions, the role of ceteris paribus, and a host of other assumptions—and when contradictory evidence is found it is not clear whether the problem is with one (or a set) of these auxiliary hypotheses or with the theory itself. While theory-ladenness and underdetermination are separate issues, they are both "arguments about the under-determination of

2. See Suppe 1977 or Callebaut 1993 in the philosophical literature, or Caldwell 1994 in the field of economic methodology.

belief by encounters with nature" (Kitcher 1992, 93), and in combination they provided a knock-down, if not knock-out, punch for the Received View of scientific knowledge.

The second sociology-inspiring criticism of the Legend also carries Thomas Kuhn's stamp: the recognition that science is fundamentally *social*. Both Kuhn's notion of theory-ladenness and his related argument about paradigm change are inexorably connected to the social character of science. The paradigm is the most important aspect of the scientific community's shared professional culture, and more than anything else it defines membership in that community; the paradigm is taught, and learned, and one comes to hold it as the result of a process of social acculturation. Although Kuhn discussed the social nature of paradigms in the first edition of *Structure*, it received even greater attention in his later work: "If I were writing my book again now, I would therefore begin by discussing the community structure of science, and I would not rely exclusively on shared subject matter in doing so. Community structure is a topic about which we have very little information at present, but it has recently become a major concern for sociologists, and historians are now increasingly concerned with it as well" (Kuhn 1970b, 252).

The third issue (or set of issues) that I want to discuss is related to the attack on the Received View, but it cannot be attributed to any one particular author; it is the general breakdown of the Legend's universal and hierarchical view of science and the scientific method. The Received View inherited the positivist belief in a unique and universal scientific method. According to this tradition there is a single universal path to knowledge—the scientific method—and this method is sanctioned by epistemology and the philosophy of science; premoderns have "knowledge" only to the extent that their beliefs can be authorized (or reauthorized) by the method science, and the relevant principles of authorizing or privileging these beliefs are universal standards. Associated with this view of epistemology as cognitive usher is a fairly rigid division between various conceptual boundaries; in addition to the demarcation between "knowledge" and "the rest of culture," the Received View also endorsed a fairly rigid separation between "empirical" and "theoretical," "discovery" and "justification," "pure" and "applied," "theory" and "practice," "science" and "technology," etc. The breakdown of the Received View has changed all of this. Science is now viewed as more heterogeneous, amorphous, and situationally embedded; when the above dichotomies are discussed at all, they are viewed more as useful, mobile, and flexible dividers, than as rigid designators. As Andrew Pickering characterizes the situation, science is now "seen as fragmented, disunified, and scrappy" (Pickering 1995, 3).

While these three issues—theory-ladenness/underdetermination, the social nature of the scientific enterprise, and the breakdown of the positivist hierarchies—were important factors in the development of contemporary sociology of scientific knowledge, they were by no means the only contributing factors. There were also historical and sociological precursors (discussed in the next section); there were substantial changes in science itself including brain science (which undermined many traditional ideas about the process of human knowledge acquisition); and finally, there were changes in the wider philosophical climate such as postmodernism and the rise of neopragmatism (Rorty 1979). The end result was that by the late 1960s and early 1970s a number of authors had begun to articulate a new sociological approach to the study of science.

As we will see, this new sociological approach—the sociology of scientific knowledge (SSK)—harbors an extremely wide range of views, but there also exists some minimal common ground. Let me just mention two of these shared presuppositions. First, they all reject traditional philosophy of science in both content and form; they reject the details of the Received View (particularly its neglect of the social aspects of science) and they also reject the general, apriorist, metamethod of traditional philosophy of science. They are "united by a shared refusal of philosophical apriorism coupled with a sensitivity to the social dimensions of science" (Pickering 1992, 2). Second, there is a general acceptance of the idea that the beliefs of scientists are social beliefs that should be explained in the same way that one would explain any other type of social beliefs. If one is trying to explain why certain Native American tribes accepted European notions of private property more easily than others, or why stockbrokers believe that a fall in the unemployment rate will lead the Federal Reserve to raise interest rates, one would use social science, and so why shouldn't social science also be used when one is trying to explain why a particular group of scientists believe that sequence pattern homology algorithms are the best way to search for genes in organism sequence data? The beliefs of scientists are social beliefs and, according to SSK, should be explained as such, as opposed to the traditional view where all beliefs are explained socially except for the beliefs of scientists, which are explained by nature. While there is a consensus within SSK about the need for the social explanation of scientific beliefs, there is disagreement about what particular social theory or vision of social belief formation should be used to guide those explanations, and as we will see this becomes an issue of much debate in the later literature.

With this brief introduction to the core ideas of SSK and the relevant aspects of the breakdown of the Received View, it might be possible to move right into a discussion of specific programs within SSK, but I want to first develop a little more background material. I want to consider two earlier sociological approaches to the study of science: Marxist histories and the Merton school. There are many reasons for discussing these two approaches, but the most important is that we are ultimately interested in economics, and both of these perspectives have a significant economic connection: they clearly emphasize how one's social/economic vision affects one's sociology of science, and they carry us back to a time when economics and more general social theory were less easily separated.

2.2 Marxist and Mertonian Views

When Nikolai Bukharin, Boris Hessen, and the rest of the Soviet delegation attended the International Congress for the History of Science and Technology in London in 1931, they presented a series of papers (Bukharin et al. 1931) that initiated an influential research program in the Marxist historiography of science. The most cited of these papers, Hessen's (1931) paper on the social and economic roots of Newton's *Principia*, undoubtedly represents the paradigm case for this genre of historical work. The basic argument, as one might guess given Soviet-Marxist thought in the 1930s, was an economically determinist view of science. Science, like education or religion, is part of the social superstructure, and must be consistent with the economic base (the forces and relations of production); science, like every other social institution, is conditioned by (and in the limit determined by) "the mode of production of material life." Simon Schaffer characterizes Hessen's argument in the following way:[3] "Hessen displayed Newton's greatest achievement as a response to the technical needs of the bourgeoise, and as conditioned by the ideological conflicts of the revolutions of the mid-seventeenth cen-

3. Schaffer defends Boris Hessen's argument against the charge of vulgar Marxism; economic relations matter, but the relationship is far more complex (and subtle) than the simple reduction of superstructure to base. This reading of Hessen brings his work much closer to some of the more recent literature in SSK: Shapin and Schaffer 1985, in particular.

tury. He went on to couple this analysis with enthusiastic advocacy of Soviet science policy, and of the promise offered by socialism for the scientific development: 'only in a socialist society will science become the genuine possession of all mankind' (Schaffer 1984, 23).

This idea—the idea that scientific institutions in a capitalist society can be explained by the fact that they reinforce (or at least are deeply conditioned by) the existing capitalist relations of production—-became the key organizing insight for a group of Marxist historians and sociologists of science in the 1930s.[4] As Pickering summarizes this view, "Marxism, after all, fixes upon production as the key moment in human activity, and upon the factory as the key site in making the modern world. And Marxist historians of science . . . were among the first to explore the intertwining of modern science with industry and capital" (Pickering 1995, 231).

The main figure in this Marxist-inspired literature was the scientist and historian J. D. Bernal (1939, 1953). Bernal's work was influenced by Hessen's paper, but Bernal and his (primarily British) school went much further; they employed the basic Marxist metanarrative to produce academic histories for other episodes in the history of science, as well as to promote a popular movement in support of the collective planning of science. Bernal's work equated eighteenth- and nineteenth-century science and technology with the process of industrialization and capital accumulation; this explained the simultaneous rise of modern science and industrial capitalism, but it also provided an intellectual foundation for the development of a new socialist science that would be adopted to human needs rather than serving the forces of exploitation and capital accumulation. For Bernal, like Hessen, the liberation of science from its capitalist servitude would require a political revolution: "We now see that though capitalism was essential to the early development of science, giving it, for the first time, a practical value, the human importance of science transcends in every way that of capitalism, and, indeed, the full development of science in the service of humanity is incompatible with the continuance of capitalism" (Bernal 1939, 409).

While Bernal and the members of his school were not the only contributors to early-twentieth-century history and sociology of science who were influenced by Marxist ideas—Karl Mannheim (1936) was certainly

4. Some surveys that emphasize this Marxist literature include Collins and Restivo 1983; Restivo 1995; Shapin 1992.

another—they were the group that had the most direct influence on later developments within SSK. The next major influence was the functionalist school associated with the work of Robert K. Merton discussed below: Merton's work, at least in its initial phase, was partially motivated by the Marxist literature of Hessen and others.

It is useful to make two points before leaving this Marxist literature. First, despite its apparent radicalism, this literature is relatively traditional about the truth and objectivity of science. While Hessen, Bernal, and the others did emphasize the impact of the capitalist mode of production (and its elimination) on the rate and direction of science (and also on the relationship between pure and applied science/technology) they did not go so far as to claim that social/economic conditions determine (wholly or even principally) the actual content of science. Science was for these authors basically true and objective; its speed, direction, and application were socially influenced, but not the actual content. Marxism was for these authors the paradigm of successful science, but it did not usurp the cognitive virtue of natural science.

Second, this Marxist literature could just as well (or perhaps more correctly) be called the *economics of science* as the sociology of science. Marxism, particularly the economically deterministic Marxism accepted by Hessen and Bernal, is an economic theory of human history; it contains what might be considered a purely sociological component, but this social component is always subservient to the economic forces. This will also be true, we will see, with some of the later literature on SSK; the economics may be different, but it will still be economics.

The second major precursor to contemporary SSK was the Mertonian school; at least in the United States this school *was* the sociology of science for about thirty years. Merton's 1935 doctoral dissertation ([1938] 1970) focused on the rise of natural science in seventeenth-century England, and it was written in part as a response to the Marxist histories of the subject. Like Max Weber's *Protestant Ethic*, Merton argued that the *ideas*, the norms and values, of ascetic Protestantism (not capitalist relations of production) created the proper cultural preconditions for the development of modern science. Merton's focus, like that of Hessen and Bernal, was on the external factors that determine the force, direction, and perhaps even complexion of science (but not its actual content); the difference was that for Merton the relevant factors were sociological, like norms and cultural values, rather than economic forces like the Marxist law of value.

The question of why science developed in seventeenth-century England led Merton naturally to the question of science's cognitive ascendancy. Science not only appeared, it never disappeared; it shifted the locus of cultural authority from God (through the clergy) to nature (through science), and vanquished all other forms of knowing. The task of finding the cultural preconditions for science involved isolating the unique cultural characteristics of science: characteristics that allowed it to ascend initially and then maintain its cognitive hegemony. For Merton the ultimate question was: what makes science unique among cultural institutions, and how do those characteristics function to legitimize and maintain science's position in society? Notice how clearly Merton's functionalism shows through in his sociology of science: one must identify the relevant cultural characteristics and then show how they *function* to maintain the institution of science.

Merton identified four such cultural values that, taken together, uniquely characterize the ethos of science:

- *Universalism*: The criteria for scientific evaluation are not specific to any particular individual or group. Scientific standards are independent of the author and applicable to all.
- *Communism*: Science is an intellectual commune. Scientists share their results and data with the wider scientific community.
- *Disinterestedness*: Scientists (qua scientist) are disinterested in the impact of their research. They do not seek political or financial rewards for their work and thus can follow the argument where it leads.
- *Skepticism*: No scientific result is accepted without careful scrutiny by empirical and logical criteria. Scientists refuse to believe any result until it has been demonstrated by scientific standards.

According to Merton these four norms function in concert to sustain and validate the community of science. They allow the scientific community to function autonomously (or at least quasi-autonomously) from the wider culture in which it is embedded, and they provide the proper social context for the production of reliable scientific knowledge. It is important to note that Merton viewed these four institutional imperatives as both normative and descriptive. They were certainly norms—they represented ideal standards that scientific communities "ought" to strive for—but he also believed that they correctly described the cultural values that were present in the most successful science:

> The ethos of science is that affectively toned complex of values and norms which is held to be binding on the man of science. The norms are expressed in the form of prescriptions, proscriptions, preferences, and permissions. They are legitimatized in terms of institutional values. They are imperatives, transmitted by precept and example and reenforced by sanctions are in varying degrees internalized by the scientist, thus fashioning his scientific conscience. (Merton 1973, 268–69)

In an important sense Merton's four norms simply *replace* the (more) a priori standards provided by traditional philosophy of science. The logical positivist criterion of meaningfulness and the demarcation criterion of Popper are also norms for the proper conduct of science—they constitute the distinctive and defining features of science—and they are norms that (at least according to some) can be observed in the best scientific practice. This seems to be exactly the same type of argument that Merton is making, except that the onus falls on social norms rather than rules for individual behavior. Thomas Gieryn makes this point and links it directly to Popper:

> His [Merton's] argument is as essentialist as Popper's, with the institutionalized ethos of science replacing falsifiability as a criterion for demarcating science from non-science. . . . In effect, the four social norms of science save the autonomy of science from external political or cultural interferences by arguing that such intrusions compromise the necessary moral conditions, which in turn make possible the extension of certified knowledge. . . . If the norms are read as demarcation criteria, then knowledge-producing activities not ensconced in that institutionalized moral frame must be nonscientific. (Gieryn 1995, 398–99)

This desire for demarcation, the desire to find something *universal and distinctive* about science—whether the scientific "method" of Popper or logical positivism, or the institutional "norms" of Merton—is present in Merton's sociology of science, but we will find that it is not present in most of the literature in contemporary SSK. Most of the contemporary literature considers even the content of science to be socially constituted and contingent (thus neither universal nor distinctive). For this reason I will make the distinction between the *sociology of science* and the *sociology of scientific knowledge* (SSK). The basic idea is that the sociology of science does not really question the objective validity of science; like Merton, such sociologists generally assume that science provides reliable knowledge about the objective world, and the sociologist is only concerned with characterizing cultural context that allows the scientific enterprise to succeed and maintain its position. For the sociology of science, only the context, not the content, of science is social. Most of the literature discussed in the rest of this paper does to endorse such a separation; it is the sociology of scientific *knowledge*. For these authors the content, as well as the context, of science is inexorably social.[5]

While Merton's reputation in the sociology of science was established primarily on the basis of his four norms of science and the surrounding literature, he is also known for a number of other contributions to the field that were more narrowly focused and more empirical.[6] On the empirical side Merton was basically the founder of the American empirical school in the sociology of science. Merton, his students, and others in the school employed a number of different statistical techniques in the investigation of "the interplay between social formations of scientists and cognitive developments in a field of science" (Merton 1977, 23). Empirical approaches such as citation analysis, content analysis, and a type of historical analysis that Merton called "prosopography" were applied to a myriad of different questions pertaining to the structure of science. These studies played much the same role that applied econometrics played in post–World War II economics; the theoretical frame of Mertonian functionalism (like the neoclassical synthesis) posed a multiplicity of empirical puzzles, and these puzzles in turn provided the fodder for a barrage of different empirical studies (dissertations as well as journal articles), which combined some particular theoretical twist in functionalist pro-

5. While it is useful to maintain the distinction between sociology of science and SSK (Hands 1994a), I must also admit that the distinction is not very crisp and can be difficult to apply in particular cases. One problem is that these attitudes fall along a continuum and often do not fit into either one of these two distinct categories, and another problem is that there is frequently a lot of slippage within a particular text or between an author's works at two different points in time. As I said, I think it is a useful, but imperfect, conceptual tool.

6. In fact there was a time during the 1970s, before the explosion of the post-Kuhnian literature, when Merton's later and more narrow work was considered to be much more important than his earlier investigations into the scientific ethos. Jonathan Cole and Harriet Zuckerman described the situation in the following way: "Sociologists of science found in Merton's later work . . . greater 'potential for elaboration' and a reasonably clear program of research. . . . Close inspection of the papers by newcomers to the field who appear on the list of most cited authors in the 1960s and 1970s shows that much of their empirical work begins with a problem posed in one or another of Merton's later papers" (Cole and Zuckerman 1975, 157).

gram with a particular data set and a slightly new statistical tool. The difference between this work and most applied econometrics during the same period was that in sociology generating the data was considered to be the main contribution and the statistical techniques were relatively mundane, while in economics the data was usually provided by the government (or central bank) and the econometric technique was considered to be the main contribution.

In addition to parenting this empirical work in the sociology of science, Merton also introduced a number of (sometimes paradoxical) ideas that have become standard within the literature. Merton (1936, 1948) popularized the idea of a "self-fulfilling prophecy" in social science: the idea that in social science, unlike natural science, one can make a prediction that changes behavior in such a way that the prediction becomes true, even though it would not have been true had the prediction not been made. Economic examples of this phenomenon range from the story of the late-night talk-show host who created a shortage of toilet paper by predicting it on a popular television show, to the more technical question of asymmetric expectations in New Classical macro models (Hands 1990). Merton was also responsible for introducing the "Matthew Effect" (Merton 1968): the idea that scientists with established reputations are more likely to get things published (or credited to them) than lesser-known scientists, even if the work of the relative unknown is superior. The term Matthew Effect now seems to have become an accepted part of the academic culture.[7] A third Mertonian contribution pertained to the role of independent and multiple discoveries in the history of science (Merton 1961), an idea that later appeared in the work of Kuhn and others.[8]

Whether we are considering Merton's four norms of the scientific ethos, the empirical work of the Merton school, or these interesting little insights into scientific culture, Merton, like Bernal, leaves science safely on the epistemic high ground. To exploit David Edge's (1995, 12–15) notion of recurrent tension between the "technocratic" and the "critical" impulse in the sociology of science, the work of both Merton and Bernal falls squarely on the technocratic side. Bernal is critical of capitalism,

7. This includes economics. For example, Nicholas Georgescu-Rogen (1992) uses it to explain why Paul Samuelson's name, rather than his own, was attached to the substitution theorem, and R. D. Tollison (1986) used it to explain why Keynes got credit for the multiplier.

8. Again there are a number of economic applications. One example is Patinkin (1983) where the author argues that, contrary to popular professional opinion, there were not multiple discoveries (by Michel Kalecki and the Stockholm School) of Keynes's General Theory.

but not really natural science, and Merton is not particularly critical of either. This changes drastically as we move forward into the post-Kuhnian literature; there is a critical rumble in the Strong Program, but it turns into a roar with those who follow.

3. Schools Within SSK

In this section I will examine the two most influential programs in SSK—the Strong Program and Social Constructionism—as well as some of the more recent literature that has grown up in response to perceived problems within these two programs. The Strong Program (or Edinburgh school) is perhaps the most cohesive of all of the various approaches to SSK; social constructionism is also quite influential, but it is less cohesive and more difficult to capture in a brief summary. The final section discusses some of the main themes that emerge in more recent work.

3.1 The Strong Program

More than any other approach within post-Kuhnian sociology of science, the Strong Program has been self-conscious; other programs are held together by shared strategies and commitments, but the Strong Program has members. This cohesiveness is enhanced by the fact that early works in the program were often methodological, laying out the program's basic approach and contrasting it to other frameworks within the philosophy and sociology of science. While this methodological self-consciousness contributed to the program's cohesiveness, it also made it an easy target for critics. The Strong Program's framework for understanding science was novel, but its metamethod, its intellectual idiom, was quite traditional; early contributors to the Strong Program basically said, "Here is the way that one ought to study science and here is exactly what you will gain by doing it this way." This is the same rhetorical strategy that had dominated the study of science for years, and comfortable on their home field, philosophers and historians of science were quick to attack.[9]

9. It is much more difficult for philosophers to get a bead on later programs in SSK (and the later the slipperier). The other programs discussed in this section, for instance, are much less likely to lay out their methodological approach in advance and are much more likely to intermingle what they are doing with the act of doing it. The difference is in part that the Strong Program was on the cusp of a radical change in science theory; the message of Kuhn and others had been received but the accepted idiom was still foundationalist and analytical. Later authors were more likely to be influenced by postmodern and neopragmatist ideas and more likely to

The Strong Program formed around the work of Barry Barnes (1974, 1977, 1982), David Bloor ([1976] 1991, 1983), Donald MacKenzie (1990), and Steven Shapin (1982). Despite the program's relative cohesiveness, differences still remain among the various authors. The closest the program comes to an official manifesto is Bloor's *Knowledge and Social Imagery* ([1976] 1991), a book that argues the Strong Program's case aggressively and with the methodological self-consciousness mentioned above.

The Strong Program, unlike the Mertonian school, is concerned with the content of scientific knowledge; earlier views, Bloor argues, left "untouched the nature of the knowledge thus created" (Bloor [1976] 1991, 3). The general approach of the Strong Program is naturalistic; it "is concerned with knowledge, including scientific knowledge, purely as a natural phenomenon" (Bloor [1976] 1991, 5). As Bloor puts it, "In delineating the strong programme in the sociology of knowledge I have tried to capture what I think sociologists actually do when they unselfconsciously adopt the naturalistic stance of their discipline" (Bloor [1976] 1991, 157). The project is empirical without being empiricist; "knowledge" under consideration is not some special certified-as-privileged-by-a-philosopher knowledge, it is what the relevant agents say is knowledge. "Instead of defining it as true belief—or perhaps, justified true belief—knowledge for the sociologist is whatever people take to be knowledge" (Bloor [1976] 1991, 5). For Bloor, "scientific knowledge" is stuff that occurs naturally within the community of scientists and it, like all natural and empirical phenomena, is subject to scientific explanation. The goal is simply to apply (social) science to the scientific investigation of a particular type of social phenomenon: scientific knowledge. For Bloor the Strong Program just does science; it is a particular type of science (social) aimed at a particular domain of inquiry (scientific knowledge), but it is just science: "Throughout the argument I have taken for granted and endorsed what I think is the standpoint of most contemporary science. . . . The overall strategy has been to link the social sciences as closely as possible with the methods of other empirical sciences. In a very orthodox

view their own approach, like the science they were studying, as relatively disunified.

way I have said: only proceed as the other sciences proceed and all will be well" (Bloor [1976] 1991, 157).

But does the application of the method of science to any topic (including scientific knowledge) require that one know what the scientific method *is* before one can apply it, and does that carry us immediately out of the purely naturalistic world and back into the hands of traditional philosophers of science who legislate what science is rather than practicing it? No, not really, according to Bloor: "The student of the piano may not be able to say what features are unique to the playing of his teacher, but he can certainly attempt to emulate them. In the same way we acquire habits of thought through exposure to current examples of scientific practice and transfer them to other areas. . . . My suggestion is simply that we transfer the instincts we have acquired in the laboratory to the study of knowledge itself" (Bloor 1984, 83).

So Bloor considers the Strong Program to be just successful (social) science, and it is possible to practice such science by rote or induction and it is never necessary to abandon the basic naturalistic stance. If this is the metamethod, then what is the method; what are the details of his approach?

Bloor presents *four methodological tenets* for the Strong Program, and these four tenets effectively *define* the program (Bloor [1976] 1991, 7):

- *Causality*: Seek the causal conditions that bring about the beliefs of scientists.
- *Impartiality*: Be impartial between true and false, or rational and irrational, beliefs.
- *Symmetry*: The same type of cause should be used to explain both true and false beliefs.
- *Reflexivity*: The explanations offered should also be applicable to the sociology of science.

These four tenets constitute the methodological heart of the Strong Program and they have generated a massive critical literature. Rather than trying to discuss (even a portion of) this critical literature, or the Strong Program's response to these criticisms, I will just consider one of the issues that are particularly relevant to the later literature in SSK and economics.

This issue is the controversial question of the role of *interests* in the Strong Program. Bloor's four tenets do not necessarily mandate exactly *how* one should go about explaining scientists' beliefs as long as one

(1) seeks the cause of those beliefs, (2) is impartial and symmetric about the truth or falsity of those beliefs, and (3) is willing to apply the same causal arguments to one's own work. Suppose, just for the sake of argument, that one is a Freudian and makes the argument that scientists believe in universal laws of nature because they are unconsciously seeking to (re)gain control over the universe they ruled as a child. It seems that such an argument could be made in such a way that it was consistent with Bloor's four tenets; it is a causal story, it could be applied to a scientist's beliefs about a theory whether the theory is true or false, and it could be applied to sociologists seeking general laws as well.

Of course I am not seriously proposing such a Freudian story; the point is simply that the four tenets do not necessarily mandate that any particular type of social/human science be used to explain scientists' beliefs, as long as whatever story is employed complies with the four tenets. This might lead one to suppose that the Strong Program employs a wide range of different social/human science theories in its efforts to explain the beliefs of scientists, but that supposition would be wrong. Those in the Strong Program rely almost entirely on *one specific approach* to explaining scientific beliefs; they explain these beliefs on the basis of the *social interests* of the scientists. These interests are based on, and emerge from, the scientists' particular place in the overall pattern of social relationships; therefore, at any particular point in time, the relevant interests could take a variety of different forms—personal, group, professional, class, national, etc.—but regardless of the specific form, the Strong Program's story always reduces to the argument that certain beliefs were in the "interests" of the relevant scientists and that such interests explain (causally, impartially, symmetrically, and reflexively) why the scientists have the beliefs that they have. This is not the only social/human science approach that could be used (consistent with the four tenets), but it is effectively the only approach that *is* used. Bloor explains this use of interests as just his own preference: "To keep the issue simple let me merely state my own preferences on this question. One way of grounding our reasoning behavior in society is to study the way in which it is harnessed to particular social interests. The "interests" model has been shown to work convincingly and in detail in a large number of cases. It certainly does not say everything that needs to be said, but it says a lot" (Bloor 1984, 88).

The last line of this quote seems to indicate the Strong Program's attitude on social interests in more recent years; the attitude is that interests stories are good social science stories with a good empirical track record (and satisfy the four tenets) and so they should be used. Statements are frequently made that not everything that is social can be couched in interest terms, and even that there are some factors affecting scientists' beliefs that are not social. An example is Bloor's remark: "It would, however, be fatal only to the claim that knowledge depended *exclusively* on social variables such as interests. Such a claim would be absurd, and has certainly not been defended in this book" (Bloor [1976] 1991, 166). Nonetheless, despite such disclaimers, it is the case that almost all Strong Program explanations *are* social interests explanations, and the use of social interests to explain scientists' beliefs has become *the identifying characteristic of the Strong Program.* The standard interpretation of the Strong Program is that beliefs are explained exclusively by social causes and that the only relevant social causes involve social interests. Perhaps this is not the only way to satisfy Bloor's four tenets, but it does seem to be the way that the Strong Program chooses to do it.

3.2 Social Constructionism

In recent years "social constructionism" has received a lot more attention than the Strong Program, even though it is less easy to pin down exactly what the constructionist approach entails. Major works in the social constructionist spirit include Collins 1985, Knorr Cetina 1981, Latour 1987, Latour and Woolgar [1979] 1986, and Lynch 1985. While there are substantive differences among these various authors, and while some of their positions have evolved over time in response to later developments and criticism (see next subsection), these works from the 1980s form the backbone of the constructionist genre within SSK. It is perhaps easiest to get a fix on constructionism by examining some of the ways in which the work of these authors differs from the Strong Program.

First of all constructionist studies are foremost *studies* of scientific practice and only secondarily discussions about the proper method or approach for doing science studies. The proportions of these two concerns seem to be reversed from their relationship within the Strong Program. The Strong Program (particularly early work) was programmatic; the emphasis was on *how* to do such work first, and then some examples of what such work might look like came second. Constructivists generally get right to it; they do the studies and keep the methodological or programmatic commentary to a minimum. Perhaps we should call this the *hands-on* aspect of constructionist SSK.

Second, constructionist studies are very local, very specific, and situated at one particular site of knowledge production. There is much less concern with scientific revolutions or general research programs; the focus tends be much more *micro*: one lab, one instrument, one result. This localness and microfocus blend in with the third feature of such studies: the emphasis on field-work, ethnographic inquiry, and participant observation. The social science that undergrids constructivist studies is more likely to be anthropological field-work than general social theories like those of Marx or Weber. The term "social construction" comes from Berger and Luckmann (1966), and many constructivists within SSK share their general vision, but contemporary constructivists put that vision to work in the context of specific, empirical, detailed, and richly textured case studies to a much greater degree than these earlier constructivists. Unlike the Marxism of Bernal, the functionalism of Merton, or the interests sociology of the Strong Program, constructionist studies *do not generally start from tight priors*; the theoretical framework of the sociologist, like that of the scientists being studied, tends to be negotiated, contingent, and context-sensitive. Science is viewed as a process involving real agents in real time doing real work—pursuing goals, interacting, utilizing resources, producing scientific artifacts—and the sociologist conducting the study is seen to be doing many of these same things.

The fourth feature of social constructivist studies of science is actually implicit in the last few sentences; constructivism views *very little as fixed and almost everything as up for grabs (or at least open to negotiation)*. In a sense this follows directly from the problems of underdetermination and theory/social-ladenness that plagued more traditional philosophical approaches to science, but it is also related to flexibility of the social framework (no tight priors) and the specificity and localness of the approach. Not only are "genes" and "electrons" something socially negotiated, so are social concepts like "class" or "interest." As Karin Knorr Cetina describes the situation,

> Almost everything is negotiable in the making of scientific knowledge; what is a microglia cell and what is an artifact (Lynch, 1982, 1985 . . .), who is a good scientist and what is an appropriate method (Latour & Woolgar 1979, pp. 161ff.), whether one measurement is sufficient or whether one needs to have several replications (Knorr

> Cetina, 1981, chap. 2.2), . . . , what is the best environment for good physics (Traweek, 1988, chap. 5) and what counts as a proper experimental replication (H. Collins, 1985, chaps. 2–3). As the last reference shows, not only laboratory studies but empirical studies of scientific work in general have demonstrated the negotiability of the elements, the outcomes, and the procedures in knowledge production. (Knorr Cetina 1995, 151–52)

The fifth feature of constructivist studies follows from this negotiative flexibility, and it is perhaps the feature that is most unsettling to critics: *nature plays little or no role in scientific knowledge*. As Kitcher phrased it, "Inputs from nature are impotent" (Kitcher 1993, 164). The world that practicing scientists and traditional philosophers of science viewed as "discovered" by science is, for constructivist SSK, "constructed" rather than discovered; scientists "make" knowledge, they do not "find" it. As Knorr Cetina put it in a recent interview,

> Since we constructivists believe that the world as it is a *consequence* rather than a *cause* of what goes on in science, we have reverted the arrow between the scientific account and the world, considering the latter as a consequence rather than a cause of the former. The focus of attention has shifted to *what goes on in science when it produces these accounts*. . . . [Scientific findings] are not just *found*, as the notion suggests, but are *fabricated*—the Latin root of the word fact is *facere*, "to make something." When you observe scientists in the laboratory, you find processes of *negotiation* at work, processes of decision making, which influence what the scientific findings are going to look like. In a sense, the scientific finding is construed in the laboratory by virtue of the decisions and the negotiations it incorporates. (Knorr Cetina in Callebaut 1993, 180)

Now the construction of scientific facts, findings, and artifacts does *not* necessarily entail an idealist ontology. There can exist an independent material world, even one that influences the activities and beliefs of scientists by offering up various resistances to/within the knowledge production process, but what cannot be said is that the world described by science is the way that it is merely, or simply, because of the way the world (really) is. Again Knorr Cetina:

> All of us constructivists, I think, are what they call *ontological realists*: We believe in the existence of the material world "out there," and we

> believe in the fact that this material world offers resistance when we act upon it. It will resist; we can't just do everything with it. So in that sense we are all realists. . . . Negotiating, for example, when they can stop the measurement, at what point they've got enough data, and at what degree or position they can say, "Now it is real!" . . . This *interpretative flexibility* . . . prompts me to doubt that you can ever get at the real world as it really is. You can get *resistances* in the laboratory; but in order for these resistances to make sense, they have to be interpreted. The very moment you interpret them, you enter the realm of the social world. (Knorr Cetina in Callebaut 1993, 184–85)

Finally, all of these features add up to a *debunking* of science, or at least a debunking of the unique and universal cognitive privilege of science endorsed by traditional philosophy of science and most practicing scientists (and, I might add, our popular culture). Science, these laboratory studies claim, is a social environment in which agents work, interact, negotiate, and ultimately constitute the world of scientific knowledge. This claim is not, for most constructivists, presented in a particularly pejorative way—it is exactly the same thing that goes on in any other artifact-producing site of human social activity—but given the traditional status of science, it is almost always *taken* to be pejorative.

While it is admittedly very difficult to capture the essence of the constructivist project in a single short list, and while many authors in the program would protest some of the entries on the list and/or add others, I do believe that these six things—hands on, micro, no tight priors, everything negotiable, impotence of nature, and the debunking of the traditional view of scientific knowledge—go a long way toward capturing the main characteristics and most significant results of the social constructivist approach. Since there have been so many case studies in the literature, I will not attempt to survey constructivist SSK. Instead I will just provide one example to demonstrate how these six characteristics play out in an actual study. The example is Harry Collins's work on replication (Collins 1985). There are a number of reasons why Collins is a good choice: first his clarity of exposition, second his application of the argument to economics (Collins 1991), and finally his recent criticism of some of the more radical (hyper-reflexivist) variations of social constructivism (Collins and Yearley 1992a, 1992b).

What Collins (1985) investigated was the issue of *replication* in science, in particular, the question of the extent to which replication does, or does not, perform the demarcational function that it has traditionally been assigned by philosophy of science. According to the traditional view (and scientists' own stories), the fact that a particular result can be replicated (at least potentially) is sufficient to grant it scientific status. In Collins's words: "Replicability, in a manner of speaking, is the Supreme Court of the scientific system. In the scientific value system replicability symbolizes the indifference of science to race, creed, class, color, and so forth. It corresponds to what the sociologist Robert Merton . . . called the 'norm of universality.' Anybody, irrespective of who or what they are, in principle ought to be able to check for themselves through their own experiments that a scientific claim is valid" (Collins 1985, 19).

Collins examined three specific cases of replication: the TEA-laser, detection of gravity waves, and parapsychological research on the emotional life of plants. If the last example seems to be outside the realm of traditional science, it is by design; if replication plays such an important role in science, it should play a substantially different role in pseudoscience. What Collins found was that there was no transcontextual way to decide what was and was not a legitimate replication. The factors that entered into scientists' decisions about the authenticity of a replication or potential replication were contextual and open to ongoing negotiation. In general, replications were simply not done; if as the result of negotiation it was determined that the original observation was legitimate, then there was no need to replicate, and if negotiations suggested that the observations were not legitimate, then there was no reason to reproduce a result that it was already agreed had no scientific import. Collins coined the term *experimenter's regress* for the version of the underdetermination problem related to replication. As he described the situation in the study of gravity waves, "What the correct outcome is depends upon whether there are gravity waves hitting the Earth in detectable fluxes. To find this out we won't know if we have built a good detector until we have tried it and obtained the correct outcome! But we don't know what the correct outcome is until . . . and so on *ad infinitum*" (Collins 1985, 84). Experimenter's regress means there is no natural (or nature-given) stopping point; replication, like all other aspects of the knowledge production process, is contingent, context-dependent, and negotiated. Collins's work on replication, like most SSK-inspired case studies, depicts science as socially constructed, nature as relatively impotent, and cognitive significance as effectively debunked. "There is no realm of ideal scientific behaviour . . . the canonical model of science—exists only in our imaginations" (Collins 1985, 143).

3.3 Contemporary Developments

Given its general tone and what SSK says about science, it should not come as any surprise that the program has been attacked by a dizzying array of critics. SSK has drawn fire from the scientific community itself (Gross and Levitt 1994); it has been criticized by philosophers who want to replace the Received View with their own version of naturalized epistemology (Goldman 1986; Kitcher 1993; Munz 1993); it has been attacked for its "insinuating, exposé style" (Susser 1989, 248); it has ostensibly been "refuted" by scientific discovery programs (Slezak 1989); it has been called "voodoo epistemology" by Paul Roth (1987) and harshly criticized by other philosophers of social science (Hollis 1982; Kincaid 1996; Rosenberg 1985); and finally it has been called "deconstruction gone mad" by Kuhn himself (1992, 9). And the list could go on and on.

Since any one of these criticisms and the SSK response probably deserves an entire article, I will not examine any of these arguments in detail. The two criticisms that I will discuss—reflexivity and relativism—are more general, and they are the two issues that have been major concerns *for those within SSK*. Much of the second-generation SSK literature (third generation if you start with Merton) has focused on trying to answer, or fix, or circumvent in some way, these two problems. These are self-identified concerns for contemporary SSK, and the particular ways in which these concerns get addressed are often the most important factors differentiating the various programs that have appeared during the last decade. I will try to clarify these problems first and then examine how some recent approaches have attempted to deal with them.

The *reflexivity* problem comes about because SSK should also apply to those who are doing the sociology of science. If scientists make decisions on the basis of their individual or group interests, then that should also be the case for the social scientists who study science. The sociologists doing SSK also constitute a scientific community, and their beliefs, like those of the scientists they study, should be socially determined. This presents a rather paradoxical problem. Consider the core SSK claim that scientific beliefs are socially determined; now *either* that claim says something about the world "out there" that the sociologists study (the world of the actions and beliefs of scientists) and tells us something that is the case for that world, *or* that core claim only says something about the world "in here," the world within the SSK community. Take the first case—the claim that scientists' beliefs, out there, are socially determined. This would mean that sociologists are epistemically privileged in a way that other scientists are not. If sociologists can really find out what is going on out there in the world of science (that it is socially determined), then it means that they have the power to discover (not just construct) the nature of the objects in their domain (the social actions and beliefs of scientists), *but this is precisely ability that they deny to the scientists they study*. On the other hand, if the argument that beliefs are socially determined is not about what is out there, but is only about what is in here, constituted by the collective belief and actions within the SSK community, then it is not clear what it has to do with scientists at all: thus undermining the debunking claims of SSK. In either case, the idiom of veracity seems to come back and bite SSK from behind. If they can say something about what really happens in science, they must be claiming the epistemological high ground; alternatively, if social interest is all that is going on in SSK, then it is not clear on what grounds anyone should accept the claim that social interest is all that is going on in science. There are certainly other ways of characterizing the reflexivity problem, but the other characterizations just seem to be variations on this general theme: a very important and (by self-admission) problematic theme.

The problem of *relativism* was discussed above as the problem of nature's impotence. Since the scientific social group constructs its beliefs about nature in the same way that any other social group constructs its beliefs about religion, art, etiquette, politics, or anything else, then the fact of the matter, say, the inverse square law, is not the reason (or at least not a sufficient reason) for its acceptance of that belief by the scientific community. This seems to entirely remove (or in less radical versions encourages the displacement of) the material world from its traditional role as the cause of, and a sufficient explanation for, the beliefs of scientists. As with reflexivity this issue has many variants, but all bear a strong family resemblance to this view of relativism as the causal ineffectiveness of the material world.

Before examining some of the second-generation SSK responses to these problems, let me briefly mention how the Strong Program handles these two issues.[10] First consider reflexivity. Recall that reflexivity was

10. While relativism is not an issue for the Mertonian school, the school has produced some

one of the four tenets of the Strong Program; they have always recognized the issue, but never considered it to be a problem. Those in the Strong Program believe themselves to be following a rather straightforward (if generic) scientific method that can be, and should be, applied to sociology as well. The fact that the (social) scientific descriptions of the beliefs and actions of scientists are not the same descriptions of those beliefs and actions that the scientists themselves would provide is simply not a problem (they say nature; the Strong Program says interests); it is not a problem in the same sense that a sociologist would not be troubled by the fact that his or her study of welfare mothers revealed that their beliefs and actions were caused by (social) factors that were substantively different than the factors the welfare mothers themselves believed to be affecting their lives. This view of reflexivity is related to the Strong Program's response to the question of relativism. Those in the Strong Program openly embrace relativism, but it is a particular kind of relativism. The argument is that there are no supralocal standards of rationality, truth, or anything else; there are only local and context-dependent standards of valuation. But while these standards are local, they *are* relevant, important, and binding on those agents in that particular local context; no universal standards does not mean no standards. In the words of Barnes and Bloor,

> The relativist, like everyone else, is under the necessity to sort out beliefs, accepting some and rejecting others. He will naturally have preferences and these will typically coincide with those of others in his locality. The words "true" and "false" provide the idiom in which those evaluations are expressed, and the words "rational" and "irrational" will have a similar function. . . . he accepts that none of the justifications of his preferences can be formulated in absolute or context-independent terms. In the last analysis, he acknowledges that his justifications will stop at some principle or alleged matter of fact that only has local credibility. (Barnes and Bloor 1982, 27)

This local-but-rational view of relativism is consistent with the Strong Program's views on reflexivity because *our local belief system, our culture, is the scientific form of life*. This says nothing about what is "true" in some absolute, universal, for ever-and-ever sense, but it does matter

reflexive exercises; see Cole and Zuckerman 1975 regarding the rise of (Mertonian) sociology of science in Mertonian terms.

what we consider to be true, and that is what we can explain in causal, empirical, scientific terms. In fact, Bloor argues, *it is this type of relativism* that shows the most confidence in, and comfort with, the scientific form of life (1991, 81); those who need/want to justify its practices on some other "higher" grounds are those who still harbor some doubt (or at least leave the door open for those metaphysical types who do).

A very different approach to these two issues is offered by the hyper-reflexivity school, a brand of SSK that evolved out of certain elements of the social constructivist literature during the 1980s. The most visible representatives of this view are Malcolm Ashmore (1989) and Steve Woolgar (1988, 1992). For the hyper-reflexive school, *reflexivity is the most important thing about SSK.* The bottom line for authors like Ashmore and Woolgar is that reflexivity is not a problem at all; it is a wonderful opportunity. It is an opportunity to push the limits of our discursive strategies, to abandon the stultifying framework of the monologue and the single author, to explore the critical "dynamic of iterative reconceptualization" (Woolgar 1992, 333). As Woolgar explains this reflexive opportunity, "Reflexivity asks us to problematize the assumption that the analyst (author, self) stands in a disengaged relationship to the world (subjects, objects, scientists, things). It asks us to push symmetry one stage further, to explore the consequences of challenging the assumption that the analyst enjoys a privileged position vis-a-vis the subjects and objects which come under the authorial gaze. It does so, needless to say, in recognition that its own privilege is temporary" (Woolgar 1992, 334).

This is one of the most radical views within the SSK, and authors often push the deprivileging theme so far as to deconstruct the standard conventions of academic discourse. Academic argumentation is reconceptualized as art, or entertainment, and the entire enterprise of "studying science" is transformed into an exercise in reflexivity, irony, and aporia. While the main focus is on reflexivity, this school treats relativism in roughly the same way, and it could just as easily be called hyper-relativism as hyper-reflexivity.

Like Woolgar and some of the other senior authors in the hyper-reflexivity group, Collins did constructivist studies in the late 1970s and early 1980s, but he is now taking a firm stand *against* hyper-reflexivity and the related literature. Collins argues that these exercises in hyper-reflexivity—however much fun they are—have essentially amounted to SSK shooting itself in the foot. In two much-discussed papers with Steven Yearley (1992a, 1992b) Collins characterizes the general trend toward hyper-reflexivity as a game of "epistemological chicken." Each new wave of SSK is more daring, more skeptical, more relativist, more willing to celebrate reflexivity, but in the end this "game of epistemological chicken as played by relativists and their successors has been destructive" (Collins and Yearley 1992a, 323). The problem is that this escalating radicalism ends up with everyone deprivileged, and no one has any place to stand, in particular, no one has any place to stand in order to either *critique or explain*: "The philosophy may be radical, but the implications are conservative. Where there are no differences except the differences between words there are no surprises left—no purchase for skeptical levers to shift the world on its axis. If anything moves it is the world as a whole. It slides unnoticed; nothing is realigned, nothing trembles, nothing falls" (Collins and Yearley 1992a, 303).

The solution for Collins and Yearley is to go back to SSK's roots in social theory and be *social realists*. There are causes of what is observed in science—it is not just a matter of reconceptualization—but the causes are not "nature speaking in a loud clear voice" as the Legend or the scientists themselves would have it; the causes are *social*. Natural scientists are naive realists, and social scientists are (or should be) social realists; natural scientists have ontologies inhabited by things like quarks and genes, and social scientists have ontologies inhabited by things like social interests and social structure. The reflexivists can never answer the question of why "only some actors have been able to get away with enforcing their view of the world" (Collins and Yearley 1992a, 323) while others have not: but according to Collins and Yearley, such success is precisely what SSK should (and can) be able to explain. There are reasons why the scientific form of life in general (or a particular theoretical strategy within it) is capable of expanded reproduction while other forms of life are not, and the answers are to be found in society, not in nature: "Our world is populated, we admit, by philosophically insecure objects, such as states of society and participant's comprehension. . . . But all worlds are built on shifting sands. We provide a prescription: stand on social things—be social realists—in order to explain natural things. The world is an agonistic field (to borrow a phrase from Latour); others will be standing on natural things to explain social things. That is all there is to it" (Collins and Yearley 1992b, 382).

A very different approach to the concerns about reflexivity and relativism is presented by the author mentioned in the above Collins and Yearley quote: Bruno Latour. Latour (1990, 1992, 1993), Michel Callon

(1986), and others (Callon, Law, and Rip 1986) have popularized a view of science called Actor Network theory (ANT).[11] Latour, like Collins, was a producer of early laboratory studies; in fact, Latour and Woolgar's *Laboratory Life* (1979) is perhaps the most famous of all such studies, but in recent years the research programs of Latour and Woolgar have diverged. Latour is also concerned about the chicken debate—and he is critical of the hyper-reflexivity of his former coauthor (although not as critical as Collins)—but he finds epistemic solace in what is perhaps a more French way, in semiotics, rather than in social realism.

Latour's basic approach is to think of science as a field for the interaction of human and nonhuman agency, with no particular priority assigned to either set of actors on the field. This is a very radical form of symmetry—the symmetry of human and nonhuman agency—that allows, for example, the scallops of St. Brieuc Bay to actively negotiate with research scientists about their anchorage (Callon 1986). The argument is that science is coproduced by the interaction of these two types of "actants" (any entity that has the ability to act); it is the product of the interdependency and negotiations of these two forms of agency, but cannot be reduced to either one. In a certain sense Latour wants to preserve the intuitions behind *both* the traditional view of science and the social realism of authors such as Collins—to simultaneously hold the agency aspects of each of these two ways of thinking about science in a single vision—but without assigning priority to either dimension. The traditional story about science was that "nature did it," that nature was the actor that had its way and etched its will on the beliefs of scientists (and the rest of us in scientific culture). On the other hand, most of SSK supports the view that "society did it," that society, social interests, and/or social structures have their way and etch their will on the beliefs of scientists. Latour wants to preserve both types of agency, but thinking in semiotic terms, neither side needs to be privileged or even ontologically nailed down. The natural and the social are constantly shifting and being renegotiated so that in the end science emerges from the field of play of these two types of agency, but as the game is played the membership of each team, that is, who is on which side as well as the total numbers, are constantly renegotiated on the field.

Supporters of ANT generally argue that their approach is just good empirical practice; both the traditional view and other versions of SSK

11. Like social constructivism(s), there are multiple ANTs (see McClellan 1996).

rely on unobservables, things that are "behind" the networks that we observe—"nature" for the traditional view, and "social factors" for SSK—but ANT requires neither. "We never see either social relations or things. We may only document the circulation of network-tracing tokens, statements, and skills. This is so important that one of us made it the first principle of science studies (Latour 1987, chap. 1). . . . It is the basis of our empirical methods" (Callon and Latour 1992, 351).

This approach may have a certain postmodern flair with all its talk about shifting agency loci and actant renegotiation, but for Latour it is not; it is simply amodern (Latour 1990, 1992, 1993). Postmodern is where hyper-reflexivity gets you, which for Latour is a dead end; amodern simply denies that either nature or society is a fixed point for understanding or existing in the world:

> We did not come to this position for the fun of it or to play the deadly game of chicken, as we have been accused of doing, but because the field is cornered in a dead end from which we want to escape. . . . This debate occurs in social studies of science and technology and only there, since this is about the only place in social science where the number of border cases between "nature" and "society" is so great that it breaks the divide apart. Classical social theory, or philosophy of science, never faced this problem, since they ignored either the things or the society. (Callon and Latour 1992, 351)

The final approach that I would like to discuss is Pickering's "Mangle of Practice" (1994, 1995). Pickering's work owes much to ANT, but he is also, like Collins, interested in questions about the relationship between realism (in his terms "the material world") and SSK. Even before he had completely articulated the "Mangle" view that has appeared in recent literature, he emphasized material "resistances" to scientists' construction and endorsed a "pragmatic realism" based on the "dialectic of resistance and accommodation" (Pickering 1990, 702). The dialectic of resistance and accommodation, while firmly denying the traditional representational view of scientific knowledge, is broadly realist in its metaphysical focus; scientists engage in the material world and construct knowledge, but the material world resists in various ways, frustrating the scientists' intentions, and these resistances must be accommodated: "This dialectic of resistance and accommodation in material practice sure justifies calling the resulting picture of scientific practice a realist one. But, I repeat, 'realist' here means something different from 'realist' as it appears in the standard realism debate. It points to a constitutive role for 'reality'—the material world—in the production of knowledge, but it carries no necessary connotation of correspondence (or lack of correspondence) for the knowledge produced" (Pickering 1990, 706).

The "Mangle" has expanded this basic argument, but the focus on construction with a role for the material world remains the same. The term "mangle" comes from the British use of the term (as a noun) for a clothes wringer, although the more common American usage, as a verb, works equally as well to capture the basic idea.

> The practical, goal-oriented and goal-revising dialectic of resistance and accommodation is, as far as I can make out, a general feature of scientific practice. And it is, in the first instance, what I call *the mangle of practice*, or just the mangle. I find "mangle" a convenient and suggestive shorthand for the dialectic because, for me, it conjures up the image of the unpredictable transformations worked upon whatever gets fed into the old-fashioned device of the same name used to squeeze the water out of washing. It draws attention to the emergently intertwined delineation and reconfiguration of machinic captures and human intentions, practices, and so on. (Pickering 1995, 22–23)

Pickering argues that the problems of relativism and reflexivity are avoided by his approach; he focuses not on representation but on performance; the entire story is couched in the "performative idiom" (Pickering 1995, 7). The result may be some type of relativism—in fact he calls it "hyperrelativism" (1995, 207)—but it is not the traditional relativism of SSK. Pickering follows the ANT theorists in avoiding both "the social" and "nature" (or anything else) as fixed points; not everything changes (gets mangled) at any particular instant of time, but no general pattern can be discerned regarding what aspects of the scientific culture will or will not change over time. His approach to reflexivity is much the same; he considers reflexivity as it is commonly discussed in the literature on SSK to be "an intensification of the representational idiom in science studies" (Pickering 1995, p. 11, n. 17) and as such is not a real concern for the mangle view of science.

While this concludes our quick tour of recent developments in the SSK literature, aspects of these four views—hyper-reflexivity, Collins's social realism, ANT, and Pickering's mangle—will appear again in the next section on economics. Before moving on though, it is useful to remind the reader that the above discussion was not intended to be an exhaustive survey of the recent literature of SSK. I think that it does in fact provide a fairly good taste of the central themes, but it is just a taste.

4. Two Points of Contact with Economics

4.1

After this fairly long introduction to SSK, it is finally time to talk about economics. What does all this literature on SSK, particularly all the attention that has been given to reflexivity and relativism, have to do with economics, either current economic theory or the history of economic thought? Well, the short answer is that *it has quite a lot to do with economics*, and I will try to defend this assertion in the next two subsections.

Before I undertake this defense, let me briefly mention one contact point that is more indirect than the two relationships that I will discuss below. This indirect contact point is the relationship between SSK and economic methodology. I say indirect because if one defines economic methodology in the way that it has been defined for most of the twentieth century—the search for the proper economic method: the search for a relatively small set of epistemologically justified rules governing the conduct of proper scientific inquiry in economics—*then there is no contact*. SSK simply says there are no such rules; the Legend is dead; quit doing (an economist's version of) philosophy of science and do sociology. Now, while the exhortation to "just quit" is hardly direct contact, the presence of SSK in the science studies literature can have (is having) an indirect impact on traditional normative economic methodology. Let me just mention two of the ways in which this occurs. One is simply that philosophy of science is not there to be used in the way that it was available in the halcyon days of the Received View. The traditional approach to economic methodology—what I have elsewhere called the *shelf of scientific philosophy* view of economic methodology (Hands 1994c), where economists simply take items off the shelf of scientific philosophy without too much reflection or reconfiguration—is going to have problems. The changes that have taken place in the philosophy of science, of which SSK is of course one of many contributing factors, have effectively removed everything from the shelf (or what remains is old, broken, and never even worked that well in the job for which it was originally designed); economic methodologists are going to be forced to take a different approach. Second, the current disarray in the philosophy of science has sent many philosophers looking for new approaches—primarily naturalized epistemologies based on either cognitive science or biology—but a few of them seem to be attracted to economics. I will not discuss the details here (see Hands 1994c, 1995, 1996; Mirowski 1995, 1996; Sent 1996 and the works cited there) but this literature opens up a range of interesting possibilities, including some very reflexive possibilities.

4.2 SSK and ESK

One thing that is obvious from the above discussion is how much certain types of SSK or earlier sociology of science looks like the *economics of scientific knowledge* (ESK).[12] First, consider heterodox economics, Marx in particular. Of course the Marxist literature of Hessen and Bernal is based on an economically determinist view that could count as a type of economics of science, but this explicitly Marxist literature is not the only such case. A number of commentators have suggested (Callon 1995, 38; Hands 1994a, 82–83; Mäki 1992, 79–81) that Latour and Woolgar's ([1979] 1986) model of credibility accumulation among scientists looks a lot like the Marxist model of capital accumulation. (Aspects of Latour and Woolgar's argument can also be expressed in neoclassical terms.) It is also fairly clear that one of the reasons the Strong Program has always had such a difficult time finding an appropriate definition for the key term "social interest" is because their notion was originally derived (more directly for some members of the program than others) from the Marxist notion of "class interest," and since "class" seems so inappropriate in the scientific context, the program has continuously struggled with the question of exactly how such interests should be defined.

Finally, on the topic of heterodox economics, Pickering's attempt to return the material world to SSK certainly has Marxist overtones. Pickering talks a lot about the shop floor as the site for the mangle of science—the factory as the "double surface of emergence" for science and society (Pickering 1995, 232). He refers to Taylorite management techniques as an intensification of the "sociocyborg of production" (Pickering 1995,

12. I will follow the earlier distinction between the sociology of science and the sociology of scientific knowledge (SSK) with the distinction between the economics of science and the economics of scientific knowledge (ESK): where ESK concerns the content of scientific knowledge as well as the social-economic factors affecting its growth and development. The caveat in note 5 holds here as well (perhaps more so).

161), and more specifically to the efforts of General Electric's management to computerize production as an attempt "to try even harder to squeeze the human out of the cyborg of production" (Pickering 1995, 166). Pickering explains that his performative approach is closely related to the Marxist historiography of Bernal and others, but there are significant differences; for Bernal the content of scientific knowledge was independent of the social, the social forms were fixed at any particular point in time, and those forms were (teleologically) destined to evolve in a particular way (Pickering 1995, 251–52). For Pickering the economic process of industrial production is a major factor in the mangle, but it affects the content as well as speed of science, and the relevant social forms are much more complex and dynamic than those envisioned by Marx's basic modes of production. While these distinctions do separate Pickering from Marx, a historian of economic thought might also ask about the relationship between Pickering's mangle and views presented by other heterodox economic theories; in particular, there seems to be an obvious question about how effectively Pickering's mangle might fit a *Veblenian* (or Ayresian) view of the relationship between technology and economic institutions.

When we include orthodox economics, the list of contact points grows much larger. As noted by Mäki, "It is interesting from our point of view that much of recent sociology of science is built upon analogies drawn from economics. In these suggestions science is viewed as analogous to a capitalist market economy in which agents are maximizing producers who competitively and greedily pursue their self-interest" (Mäki 1992, 79). As mentioned above, a number of authors consider Latour and Woolgar's ([1979] 1986) discussion of scientists' attempt to compete for credibility, and the development of a market for such credibility, to be a clear example of ESK:

> Let us suppose that scientists are investors of credibility. The result is the creation of a *market*. Information now has value because . . . it allows other investigators to produce information which facilitates the return of invested capital. There is a *demand* from investors for information . . . and there is a *supply* of information from other investors. The forces of supply and demand create the *value* of the commodity, which fluctuates constantly depending on supply, demand, the number of investigators, and the equipment of the producers. (Latour and Woolgar 1986, 206)

Another constructivist author who employs economic argumentation is Karin Knorr Cetina. She discusses two research strategies in particle physics: the "framing" strategy and the "exchange" strategy. The exchange strategy sounds like a group of scientists engaging in cost-benefit analysis to maximize the utility of their group research efforts: "I have defined contingency in terms of a negative relationship of dependence between two desired goals, or research utilities, such that one utility can only be obtained or optimized at the cost of the other. In this situation particle physicists resort to a strategy of commerce and exchange: they balance research benefits against each other, and they 'sell off' those which they think that, on balance, they may not be able to afford" (Knorr Cetina 1991, 112–13).

One of the more recent approaches that seems to carry with it a lot of economic baggage is the ANT of Latour and others. Chris McClellan (1996) has recently argued that as ANT evolved out of Latour and Woolgar's *Laboratory Life* ([1979] 1986) and Latour's *Science in Action* (1987) it carried with it a strong dose of the capitalistic market analogy, an analogy that has been a source of continual tension within ANT. For some within ANT the scientist should be seen as an entrepreneur who builds a network by enrolling actants through efficient investment of scarce resources; success is market success and knowledge, like capital, is accumulated in the process. Others involved in ANT accept some aspects of this market story but consider it to be primarily a critique of the encroachment of market rationality into every aspect of human life (including science). Thus, as McClellan explains, elements of ANT pull in both directions of "the great divide which separates *homo sociologicus* and *homo economicus*" (McClellan 1996, 203). Latour's recent work (1993) seems to spend a lot of time denying the importance of both the economic argument and the other side of the ANT literature that considers it as critique. It is interesting to note that in a recent paper presenting "Four Models for the Dynamics of Science," Callon (1995) implicates economics (either Marxist or neoclassical) in two out of the four views he discusses, but does not mention economics directly in the "extended translation" approach that includes ANT. Nonetheless at the end of the paper he lists two general areas for future research. The first involves the application of ideas from industrial organization theory such as "barriers to entry, differentiated return on investments, imperfect competition, diversification and differentiation strategies" (Callon 1995, 61). The second concerns the links between technology and economics and "in particular with the economics of technical change whose recent results show a remarkable convergence with those of the sociology of science" (ibid.).

So it seems pretty clear that SSK has a strong dose of economics: both heterodox and orthodox. So what are we to make of this? I am certainly not in the position to provide anything like a definitive answer to this question, but there are some things to consider. The most important development is that along with sociologists applying economic-like arguments in SSK, there is also a growing literature that *starts from economics* in its analysis of science. First, there is a growing literature on the *economics of science*: where economic tools (microtheory and econometrics) are used to describe, predict, and explain the behavior of scientist agents in the context of various scientific institutions, in much the same way that say public choice theory describes, predicts, and explains the behavior of political agents in the context of various governmental institutions. As Paula Stephan (1996) makes clear in her recent survey of this literature,[13] the connection to the sociology of science is to the Merton school. This growing field in the economics of science is basically an attempt to use contemporary economic tools to answer Merton-type questions about the industrial organization, the incentive structure, the public funding, and the dynamics, of science and scientific institutions. It seems that within a few years the economics of science will be an established field in economics: as established as, and similar in focus to, areas like the economics of the family, public choice theory, or law and economics. Like the work in these fields, there will be some minimal contact with social scientists from other fields working on the same questions, but the "economic approach" will have its own separate theoretical focus, empirical tools, and publication outlets.

Second, there is a less developed, but also growing, field of the *economics of scientific knowledge* (ESK) which, like SSK, is more critical, more skeptical, more concerned with recent philosophical developments, and most importantly wants the social-economic analysis to endogenize the content of science. This literature is currently quite amorphous; it includes the parts of SSK where the relevant background theory is economic (Marxist or orthodox), certain works of various vintages within the philosophy of science where economic arguments have been used to discuss (positively or negatively) the content or cognitive significance of

13. Mirowski and Sent 1995 contains an extensive bibliography of this literature.

science, and a ragtag array of other work linking the content of science to economics or economic activity.[14] The entire area is currently quite contested ground, and how this contest plays itself out will ultimately have implications for how we think about knowledge in general as well as about economics and economic institutions. The traditional view that there is a unique and universal scientific method that can be captured by philosophers of science and packaged up for distribution to economists—a one-way flow of information from the epistemically privileged to the epistemically challenged (but eager to learn) by way of philosophical messengers—is potentially up for radical revision. Perhaps there will be a significant change in this fundamental vision, and perhaps it will endure, but in either case, it is quite clear that economics will be right in the heart of the fray.

4.3 SSK and HOPE

All of this speculation about the radical revision of the way we think about science and knowledge is fine, but for many historians of economic thought engaged in their own individual projects, it is mostly irrelevant speculation (perhaps mildly interesting) but ultimately removed from the day-to-day intellectual tasks of figuring out what past economists have said and why they said it. On the other hand, there is a much more down-to-earth question about SSK and its relationship to economics that we might consider. That question is simply whether we can, or should, do the history of economic thought in the same way that sociologists and historians of science informed by SSK do the history and sociology of science. Can we, or should we, *apply SSK to the history of economic thought*?

Before considering these questions, let me quickly discuss some of the existing work in the field. First, there are a few sociologists who have explicitly applied SSK to economics; in addition to Collins 1991 discussed above, there are a few SSK-based studies in applied fields like health economics (Ashmore, Mulkay, and Pinch 1989), as well as studies on various historical episodes such as Yuval P. Yonay's (1994) paper on Institutionalist economics. Economists have produced at least three different types of research that explicitly apply the sociology of science

14. Examples include Baltas 1993; Lynch and Fuhrman 1991; Mirowski 1992, 1994; Sohn-Rethel 1975

and/or SSK to various topics within the history of economic thought. First, there is research that discusses the question of SSK and economics at a very general level: works such as Coats 1993a, 1993b; Hands 1994a; and Mäki 1992. Second, there are studies that apply the work of one particular sociologist or approach to economics; in addition to the Mertonian studies mentioned above, the list would also include papers such as Davis 1997 and Hands 1994b. And finally, there are works in the history of economic thought that are generally informed by SSK but do not focus on any one particular school or approach: Hands and Mirowski 1996; Mirowski 1989; Weintraub 1991; or Weintraub and Mirowski 1994 are a few examples.

In addition to this relatively recent work where SSK is directly applied, there are certainly many other, much earlier, works in the history of economic thought that, while not directly applying the sociological literature discussed above, could, with hindsight, be considered sociological approaches to the history of economic thought. Certainly Marx's own *Theories of Surplus Value* (1963) or later Marxist work in the history of economic thought such as Bukharin's *Economic Theory of the Leisure Class* (1927) might count as works in the Marxist sociology of science. Actually these are very interesting cases. Although it was not mentioned in the above discussion, there is one way in which the traditional view of scientific knowledge does allow for a sociology of science that considers content and that is as a *sociology of error.* According to the traditional view, scientists will get things right when they apply the proper scientific method, but one way to explain why they once in a while get something wrong is to blame it on social factors which interfere with this scientific method. This is the standard story about the Lysenko affair in Soviet biology; the social factors interfered with (good) scientific practice and thus these social factors can be used to explain the presence of error. Successful science is explained by nature, while erroneous science is explained by sociology. Now the sociology of error would seem to be the approach in Bukharin 1927: Marx did real science and discovered the laws of capitalist development, while economists who came after him did not do real science; they simply produced apologetics for the capitalist class. They produced error and that error can be explained by social (economic) factors. Marx himself is more subtle. For Marx, one of the reasons that David Ricardo and Adam Smith did not get it all right was simply that in their day the historical conditions of capitalism had not developed to the point where the contradictions were as clear as by the middle of the nineteenth century. Now this is a kind of social-economic reason for the content of Smith and Ricardo's economics, but it is not the sociology of error story (Marx did apply the sociology of error story to some economists of his own time); this distinction between "caused by the social conditions but good science" (Smith and Ricardo) and "caused by social conditions that led to error" is a distinction that would never appear in the study of natural science. These comments on Marx's own history of economic thought might also apply to other particularly context-sensitive histories of economic thought such as Wesley Mitchell's *Types of Economic Theory* (1967), or perhaps even parts of Joseph Schumpeter's *History of Economic Analysis* (1954), but right now this suggestion is just speculation on my part. This entire topic deserves much more serious attention.

Returning now to the can or should question: the answer is that of course you can, but it is up to each historian of economic thought to decide for themself if they should. What I have tried to do in the previous pages is to clarify the available options—to make it clear what SSK is, where it comes from, what some of the potential problems are, and how those in the field view their own work—so that historians of economic thought *can* decide for themselves how much they want to integrate SSK into their own research. In closing though, I do think there are at least three important things to keep in mind.

First, there is clearly a sense in which SSK seems to be less radical in economics than in natural science. The idea that economists' beliefs are determined by "social factors" is hardly surprising. Of course economists' beliefs should be determined by social factors; they should be determined by social factors like the rate of interest, the level of unemployment, time-series data on relative prices, etc. On the other hand, their beliefs should not be determined by social factors such as who pays for the study, which political party will gain from the report, or whether the work cites the author's department chair. In the case of economics the issue is not social versus nature; it is the right kind of social versus the wrong kind of social. Now it seems that almost everyone—scientists, the general public, writers on economic methodology, perhaps even most practicing economists—seems to accept that it is far easier (for even the epistemically well intentioned) to slip from "right social" to "wrong social," than to slip from "nature" to "society." The debunking aspect of SSK is far easier for most people to accept when applied to a field like economics than to one of the natural sciences. Now this has both a good and a bad aspect for historians of economic thought who might be interested in applying SSK to their own work; on the positive side it makes the story easier to sell, but on the negative side it makes it much less interesting.

Second, SSK is *not* equivalent to "rich, deeply textured, thick history." The two are certainly correlated—most SSK-inspired histories are relatively thick histories—but they are neither necessary nor sufficient for each other. There are various interest-based explanations in SSK that are relatively thin, and there are many thick histories outside of science (political history, biography, etc.) that are certainly not SSK—they are not about knowledge production and they are not based on any particular sociological theory. SSK is not a general approach to history; it is only interesting when applied to historical episodes that purport to involve the production of knowledge. We already know that most historical events outside of science are determined by social factors, social interests, and contingency. What is new and interesting about SSK is its claim that such factors are at work (and significantly at work) in natural science too.

This brings me to the third and final point: why might someone equate SSK with thick history? The reason, I think, is that so much non-SSK history of science (unlike political history, etc.) is so thin. Remember, SSK grew up in contrast with, and juxtaposed to, a hagiographic vision of the scientific enterprise. It was in part the positivist influence, in part sociological factors like the cold war, and undoubtedly many other factors as well, but mid–twentieth-century history of science (excluding the Bernal school, the Mertonians, etc.) was pretty thin; science was on the fast track, old science was wrong science, and the history of science was only of interest to historians. Without being too Hegelian, SSK comes into being and exists only in contrast to this relatively pristine and hagiographic vision of the scientific enterprise; debunking, like satire, works most effectively against the self-assured. This has relevance for the history of economic thought. The place where SSK would seem to be the most effective would be where the relevant economic theory was most stridently scientistic. An SSK-inspired approach to the work of List, or Veblen, or Henry George, while it could be done, just does not seem very informative; on the other hand, an SSK-inspired study of Walrasian general equilibrium theory, New Classical Macro, or 1970s econometrics, seems to present economics in an interesting new light. In fact this is borne out in the existing historical work; the most self-consciously social constructivist book in the history of economic thought is Roy

Weintraub's *Stabilizing Dynamics* (1991). SSK is most incisive when juxtaposed against a historical record that is thin and self-congratulatory. General equilibrium theory seems to be an obvious choice.

5. Conclusion

I have provided a rather long and somewhat leisurely stroll through SSK: its inspirations, its precursors, its (at least self-recognized) problems and tensions, its variations, and finally, how it might relate to economics and the history of economic thought. I have generally tried to be an expositor and information source rather than an advocate (although in this field where tempers run high, not being an aggressive critic is usually taken as an endorsement), but it should also be clear that I think that SSK is a very useful approach to certain topics in the history of economic thought. I hope I have provided some useful and interesting information, and left readers, who I am assuming are generally historians of economic thought, in a much better position to evaluate SSK-inspired work in economics and decide about its role in their own research. There are many important issues that were not mentioned at all and others that were passed over far more quickly than they deserve, but not everything can be done in one paper. I am assuming this essay will inspire and incite a number of responses and extensions. This is an exciting and important area, and there is certainly a lot of work to be done.

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