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THE SOCIOLOGY OF SCIENTIFIC KNOWLEDGE

Some thoughts on the possibilities

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INTRODUCTION

During the last twenty years the sociology of scientific knowledge (SSK) has emerged as an influential new approach to the study of science. Unlike traditional philosophy of science which often emphasizes issues such as demarcation, appraisal and the logic of scientific theory choice, the sociology of scientific knowledge focuses on the inherently social nature of scientific inquiry. According to the SSK, science is practised in a social context, the products of scientific activity are the results of a social process, and scientific knowledge is socially constructed. Although there are a variety of individual points of view within the general framework of the SSK (and the SSK-inspired work in the history of science) these different perspectives are 'united by a shared refusal of philosophical apriorism coupled with a sensitivity to the social dimensions of science' (Pickering 1992b: 2). In other words: most of what philosophers of science have said about science is irrelevant, and science is fundamentally social.

Although the SSK raises a number of provocative challenges to traditional epistemology and the philosophy of science, such global philosophical issues are not the primary focus of this paper; some of these issues will surface briefly toward the end of the discussion, but they are not the main theme. The main theme of this paper is economics and what this recent literature on the SSK might mean to economics and economic methodology. In particular I want to address such questions as whether an 'economics of science' might not be as important to the study of science as the sociology of science, and whether the SSK in any sense 'leads to' such an economic analysis of science. Such an economics-based investigation into the nature of science would certainly raise a number of questions for economic methodology. While the paper will examine a wide range of issues regarding economics and the SSK, my purpose is only to provide a general discussion of these topics and not to advocate any one particular perspective on the relationship between these two fields.

The paper is arranged in the following way. The first section documents the rise of the SSK and briefly discusses some of its intellectual origins. A few of the differences among the various schools of thought within the SSK will be examined in this section. The second section will consider the issue of whether the SSK might 'lead to' an economics of science. In particular the question of 'social interests' in science will be discussed and how such 'interests' might be economically interpreted. The third section considers some of the existing literature that might be classified as 'the economics of science' or 'the economics of scientific knowledge'. Although this literature is hardly voluminous, it has existed for a long time (one of the early contributions dates from the late nineteenth century), and it is currently expanding. The fourth section considers some of the more general philosophical issues raised by the SSK (and inherited by any economics-based alternative to it): particularly the questions of 'circularity' and 'reflexivity'. In the fifth and final section, I will argue that while the economics of science seems to be a fertile area for additional research, interested economists should recognize that the social study of science (by sociologists or economists) represents a virtual Pandora's box of challenges to the beliefs that most economists hold regarding knowledge, science and even nature. These wider philosophical implications will leave many economists feeling rather uncomfortable about the whole project of an economic version of the SSK even though it would otherwise seem to be a rather obvious next step for the application of the economic method.

THE RISE OF THE SOCIOLOGY OF SCIENTIFIC KNOWLEDGE

While the recent literature on the SSK draws its intellectual inspiration from a broad range of sources, two important influences can be rather easily identified: the earlier 'sociology of science' of the Merton tradition, and the historicist (some would add 'relativist') turn within the philosophy of science initiated by authors such as Thomas Kuhn (1970) and Paul Feyerabend (1975).

The origin of the earlier sociology of science is frequently traced to Robert K. Merton's doctoral dissertation in 1935 (Merton 1970). Merton's thesis, in opposition to both 'internal' (usually inductivist) histories of science as well as Marxist 'external' histories,[1] argued that the development of natural science was promoted by the Puritan ethic of seventeenth-century England. Merton's argument that it was a particular social milieu that brought about the rise of science was quite similar to Max Weber's thesis that it was the Protestant ethic that had spurred on the development of capitalism. It is important to note that Merton's sociology of science was a sociology *of science*, and not necessarily a sociology *of scientific knowledge*. The distinction is very important. Neither Merton nor the other members of the Merton school really questioned the objective validity of our scientific knowledge. Science, for the sociology *of science*, employs a particular scientific method that provides reliable and universal knowledge about the objective world; sociology only enters in an attempt to explain the unique characteristics of the social and institutional context that allows such objective knowledge to be obtained. For this early, Mertonian, sociology of science, 'scientific knowledge' – the content of scientific theories – is not inexorably social. There are, for the Merton school, external social factors that promote or impede the development of scientific knowledge, factors that can be studied by the sociology of science, but the objective content of scientific theories exists independently of these social factors. This objective independence of scientific knowledge is not endorsed by many contemporary contributors to the SSK; this is one of the reasons why the recent literature, unlike that of the Merton school, is termed the sociology of *scientific knowledge*, rather than merely the sociology of *science*. Much of the impetus for this (much stronger) claim regarding the constitutively social nature of knowledge itself came from the work of historical philosophers of science like Kuhn and Feyerabend in the 1960s and 1970s.

Thomas Kuhn's (1962) central thesis regarding paradigms, scientific revolutions and normal science is much too familiar to summarize here, but it is useful to review a few individual parts of Kuhn's argument in order to trace the relationship between his influential ideas on the history of science and the development of the SSK. Kuhn's basic claim is that in mature science the members of a given scientific community are always in the grip of a collectively shared paradigm. In 'holding' a certain paradigm what the scientists 'see', or do not 'see', is determined by the paradigm. Observations are not independent and 'theory free', but rather are a product of the paradigm and are 'theory laden'. During a scientific revolution the scientist's way of seeing, the gestalt, changes; what was once seen 'as' one thing, is now seen 'as' something else. On this view there are no theory neutral empirical observations by which scientific theories can be independently judged. Rather it is the scientific theory itself, or more properly the scientific paradigm itself, that actually determines the observations within its domain. Two different paradigms are thus fundamentally 'incommensurable'; they constitute two incomparable ways of viewing the world.

Notice how this Kuhnian view of science introduces an irrevocably social element into science. It is not simply that different scientists have different subjective perspectives that taint their observations in various ways; rather each individual scientist 'participates in' or 'shares' a collective world view – the scientific paradigm – and this collectively held world view determines what they do and do not 'see'. What is 'observed', what is and is not seen as 'evidence', becomes a social product; the 'world' the scientist participates in, the 'world' of science, is socially constructed.[2] This particular aspect of the Kuhnian story – the social construction of the scientist's world – clearly opens the door for a sociological analysis of these scientific worlds (even though such a sociological analysis was not Thomas Kuhn's main interest). Since each individual paradigm constitutes 'the facts' in its domain, it cannot be the case that theory choices are made on the basis of the 'objective facts'. But if it is not the objective evidence that determines the choice between scientific theories then what does? Enter the social studies of science. The scientific community, like any other human community, forms a culture; it is a society. This society, this culture, can be examined like any other society, and since the traditional mode of inquiry for the study of society is sociology, the result is the SSK.[3]

While there are very many different individual points of view within the SSK – some inspired more by anthropology than sociology, and some much more radical than others – there is one group that seems to be cohesive enough to be labelled a particular 'school' within the SSK. This school is the so-called 'strong programme' associated with Barry Barnes, David Bloor and Steven Shapin.[4]

One of the central theses of the strong programme is that 'social interests' determine which scientific theories are successful and which are failures. As Paul Roth has characterized this view:

> The successes of science, both in the laboratory and in the prevailing textbook account, are to be explained by citing those social factors that cause, in a given historical context, a particular scientific theory to triumph (be judged correct) in place of its competitors. More specifically, the considerations determining which scientific theory will prevail, including the standards by which any such theory is deemed better than its alternatives, are tied to perceptions of which theory best rationalizes the interests of the dominant social group. This view differentiates the strong programmers from those . . . (most prominently Karl Mannheim and Robert Merton) who hold that the process of scientific justification is not a form of ideological rationalization and so not to be explained by sociological inquiry.
>
> (Roth 1987: 155–6)

Although the strong programme generally emphasizes the macro-social interests or ideologies that 'bear on national or dynastic politics' (Bloor 1984: 79), other, more micro-oriented social factors, such as the particular interests of the individual members of a given scientific community, may also be considered.

> I mean that the social factors concerned may be ones which derive

> from the narrowly conceived interests or traditions or routines of the professional community .... Much that goes on in science can be plausibly seen as a result of the desire to maintain or increase the importance, status and scope of the methods and techniques which are the special property of a group.
>
> (Bloor 1984: 80)

Notice how much the strong programme's view of scientific knowledge differs from the traditional philosophical characterization of science. The traditional view emphasizes the world 'out there' – either the real objective 'world' of nature (realism) or the 'world' of empirical phenomena (instrumentalism) – as the determining factor in our scientific knowledge. For the strong programme it is not the world 'out there', but rather it is the particular social context – the social interests present in that context – that determines what beliefs scientists hold, and these beliefs in turn determine what comes to be scientific knowledge. The beliefs that scientists hold are shaped by their social context, the social milieu in which they live and work; since these beliefs determine what comes to be scientific knowledge, the result is a scientific knowledge that is fundamentally social, a product of (and in a certain sense 'about') its social context. To explain scientific knowledge within this framework one focuses on the beliefs that scientists hold as the cause of scientific knowledge, and to explain the beliefs that scientists hold one focuses on social context as the cause of those beliefs. This view of science not only elevates the role of 'the social' far beyond that which it has traditionally played in the philosophy of science, but also well beyond the role it played in the earlier Mertonian sociology of science.

Although the strong programme is, in certain respects, quite radical, it remains philosophically traditional in at least two ways. First, the strong programme employs a relatively traditional notion of 'cause'. According to the strong programme the beliefs of scientists have 'social causes' in the rather straightforward and commonsense way that any event A might 'cause' event B.[5] Second, the strong programme practises a type of sociology that is both empiricist and inductive, thus making it quite traditional in its scientific methodology. According to the strong programme one simply determines the social causes of scientists' beliefs by empirically examining actual science. As Bloor admits, 'I am an inductivist .... My suggestion is simply that we transfer the instincts we have acquired in the laboratory to the study of knowledge itself' (Bloor 1984: 83). Such a stance is not only inductivist it is also methodologically monist – it presupposes that social science, in this case the sociology of science, should employ exactly the same methodology as that which has traditionally been characterized as 'the method' of natural science.[6] Critics have used both of these traditional aspects of the strong programme as points of attack.

While I will not say much more about the issue of causality,[7] the questions of inductivism and methodological monism will surface again when 'reflexivity' is discussed in the penultimate section.

No other group of authors within the SSK forms such a clearly defined 'school of thought' as the strong programme; outside the strong programme the SSK is composed of a number of disparate points of view which disagree on a number of issues. Despite this disagreement, if one is willing to live with rather rough-hewn categories, it is possible to characterize one major alternative to the strong programme. This alternative school – the 'constructivist' or 'ethnographic' approach to the SSK – differs from the strong programme in at least two significant ways.[8] Both of these differences could be considered methodological in a broad sense. First, the constructivist approach differs from the strong programme with respect to the sociological categories and the theoretical entities employed in the investigation of science. The strong programme generally (though not exclusively) focuses more on the broad macro-sociological variables at work in the wider society, while constructivist authors tend to focus more on the micro-sociological factors at work in the individual laboratories and other sites of scientific activity. Second, the strong programme and the constructivist approach differ considerably regarding their basic methodology of social inquiry. The strong programme is (as discussed above) narrowly 'scientific' in its approach to understanding the social causes of scientific belief; this rather narrow meta-method does not generally characterize the work of those writing from a constructivist point of view. The constructivist authors draw their methodological inspiration from a much wider range of inquiring traditions: the participant–observer approach in anthropology, ethno-methodology and the hermeneutic tradition to name a few. The constructivist authors are broadly empirical, but it is not the simple inductivism of the strong programme. Those in the constructivist programme seek to understand the social nature of scientific activity and employ a broad range of inquiring frameworks in order to obtain that understanding; it is not simply a matter of applying the natural science method to the study of science as it often is for the strong programme. Constructivist studies in science are generally local, richly detailed and deeply textured investigations into scientific practice as a life activity; they may focus either on a particular historical episode in science or on contemporary scientific activity, but in either case the result generally involves much more contextual solicitousness than the investigations of the strong programme.[9]

SOCIOLOGY, INTERESTS AND ECONOMIC OPPORTUNITY

The SSK seems to leave the door open for an economic analysis of science. If science is done in social communities by individual scientists and we desire to employ social science to help us understand the behaviour of those scientists in that community, then economics seems to be as likely a candidate for the relevant social science as sociology or anthropology. In the SSK, both versions, there is a lot of talk about the 'interests' of those in the scientific community, and while economists do not normally use the term 'interests', they do in fact explain economic behaviour on the basis of the 'interests' of the agents involved. The economics of science, or the economics of scientific knowledge, seems to be a rather obvious next step in the study of science as the product of a social community of individual agents.[10]

Before taking this obvious next step and considering the economics of science explicitly, I would like to examine a few of the quasi-economic arguments that have been offered from within the SSK. Surprisingly there are a number of cases within the SSK where, even though the author was not consciously attempting to apply economic analysis to science, the arguments offered do in fact sound very much like economic arguments.[11] This quasi-economic argumentation is emphasized by Uskali Mäki in his recent examination of the SSK and economics:

> 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. The point of emphasis in these suggestions is on scientists' action and on the ends involved in that action.
>
> (Mäki 1992: 79)

It seems useful to examine a few of these quasi-economic discussions from the existing SSK literature before moving on to the explicit consideration of the economic approach in the next section.

The first case I would like to consider is one that is also discussed by Mäki (1992): Bruno Latour and Steve Woolgar's *Laboratory Life* (1986). This work, a work that is generally (and fittingly) considered one of the more radical positions within the SSK, has a surprisingly large amount of economic argumentation. In chapter 5, where Latour and Woolgar discuss the motivation of scientists, there are many references to the 'quasi-economic terms' (p. 190) that scientists, particularly younger scientists, use to describe their own work and professional involvements. The scientists interviewed by Latour and Woolgar repeatedly used the term 'credit' to describe that which was being sought through scientific activity as well as that which participation in science would distribute to those who were successful. This scientific 'credit' clearly has a component that is direct reward, but the scientists in Latour and Woolgar's study seemed to be motivated by, and interested in, more than simply the direct rewards from credit. Latour and Woolgar expand the notion of credit beyond the simple notion of a reward to a broader issue of professional 'credibility'. They argue that when scientists are viewed as 'engaged in a quest for credibility, we are better able to make sense both of their different interests and of the process by which one kind of credit is transformed into another' (Latour and Woolgar 1986: 200). After elaborating on this expanded notion of credibility (pp. 198–208), Latour and Woolgar embed the concept in a general economic characterization of science.

> 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 which may increase the power of their own inscription devices, 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. Taking into account the fluctuation of this market, scientists invest their credibility where it is likely to be most rewarding. Their assessment of these fluctuations both explains scientists' reference to "interesting problems," "rewarding subjects," "good methods," and "reliable colleagues" and explains why scientists constantly move between problem areas, entering into new collaborative projects, grasping and dropping hypotheses as the circumstances demand, shifting between one method and another and submitting everything to the goal of extending the credibility cycle.
>
> (Latour and Woolgar 1986: 206, italics in original)

For Latour and Woolgar it is the market for credibility that determines what scientists work on, what they find interesting, what is considered good work and ultimately what becomes scientific knowledge.[12]

The second example of an economic-like characterization from within the SSK is Karin Knorr-Cetina's 'exchange strategy' representation of the scientific activity in experimental particle physics. In her studies of particle physicists she found two basic strategies; one of these was the very economic-sounding 'exchange strategy', which she characterizes in the following way:

> 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 then "sell off" those which they think that, on balance, they may not be able to afford. Particle physicists refer to this commerce with research benefits as "trade-offs." In the experiment we observed, they traded off tracking particles against electron identification; time needed for calibration against granularity of the detector; performance of the calorimeter against cost; dead time against background reduction; and so on.
>
> (Knorr-Cetina 1991: 112–13)

Here again, as in Latour and Woolgar's *Laboratory Life*, scientific activity is described in terms that are quite familiar to economists. It should be noted that this 'exchange strategy' was not the only strategy that Knorr-Cetina found among particle physicists – and she also found a totally different strategy among molecular biologists – but nonetheless it clearly is an economic story about the practice of science.

One very important point about these two examples (and perhaps other examples that one might find in the SSK[13]) is they *are not attempts to 'apply' economics to science*. Economists often find themselves in the position of trying to model something 'as an X'; we might, for example, try to model sticky wages 'as a rational response to asymmetric information', or we might try to model the demand for children 'as the outcome of a noncooperative game'. This is not what is going on in these economic stories about science. The economic argumentation that appears in the literature on the SSK is not a result of the various authors trying to model science 'as a competitive market process'. The intention of these three authors in particular was certainly not to demonstrate the robustness of the economic method; it was simply to examine the social nature of science through careful ethnographic investigation into the actual practice of science. As we will see in the next section, there are in fact some studies that are motivated by an attempt to 'apply' economics to science, but neither Latour and Woolgar nor Knorr-Cetina are such cases. In fact, although it is purely speculation on my part, a reasonable conjecture would be that all three of these authors consider neoclassical economics to be naively reductionist, narrowly individualist, and in general a quite uninteresting approach to studying (any) social process. The point is, despite the fact that none of these authors intended to apply economics, and perhaps do not even particularly like the discipline, the stories that emerge from their ethnographic investigations of science look very much like the product of economic analysis.[14]

For the third and final example of economic analogies in the SSK I will take a slightly different approach. Rather than simply showing that what was produced in a particular sociological or ethnographic study looks very much like what an economist might say about science, I will discuss a case where a particular study in the SSK has been criticized precisely because the study characterizes the behaviour of scientists in the way that a neoclassical economist would characterize individual behaviour.

Andrew Pickering's *Constructing Quarks* (1984) is an influential sociological history of high-energy physics from 1960 to 1980. The study is self-consciously constructivist and it focuses on the intricate details of the 'dynamics of practice' rather than the more general 'social interests' that motivate many studies in the SSK. Pickering's basic claim is that scientific activity in particle physics is best understood as 'opportunism in context'; he characterizes this opportunism in the following way:[15]

> Perhaps the single most conspicuous departure of *CQ* from the philosophical tradition is that, in *CQ*, I paid great attention to the dynamic aspect of scientific practice. I advanced a general schema for thinking about this dynamics under the slogan of "opportunism in context." The idea was simple enough. Doing science is real work; real work requires resources; different scientists have different degrees of access to such resources; and resources to hand are opportunistically assembled as contexts for constructive work are perceived. My claim, exemplified many times over in *CQ*, was that if one understands scientists as working this way then one can understand, in some detail, why individuals and groups acted as they did in the history of particle physics.
>
> (1990: 692)

Many commentators have been critical of Pickering's 'opportunism in context' precisely because it sounds so much like economic haggling in the marketplace. Peter Galison, for instance, comments that experimentation should not be 'parodied as if it were no more grounded in reason than negotiations over the price of a street fair antique' (1987: 277). Similarly, in Paul Roth and Robert Barrett's lengthy critical examination of Pickering's book they make the following remarks about his economic approach:

> Pickering's model of scientific decision-making is thus fundamentally an economic one – scientists invest their expertise in areas promising them the most useful return for this investment. Justification of decision-making is dictated, in this model, by factors completely outside of the purview of traditional philosophy of science.
>
> (1990: 594)

While commentators like Roth and Barrett, and Galison, are critical of the economic aspect of Pickering's story, this is not their only, or even their major, concern. In both cases, the fact that Pickering has characterized science as an economic process is a relatively minor infraction compared to the fact that he has almost totally excluded objective reality as a constraint on the experimental behaviour of scientists.[16] However, in the same series of papers that contains Roth and Barrett's criticism, Steve Fuller (1990) attacks Pickering specifically on grounds that are relevant to microeconomics: the way he characterizes the agency of the individual scientists. In Pickering's story the scientists behave essentially in the way that neoclassical agents behave – they make intentional choices on the basis of their beliefs and desires. For Fuller, this argument (common to all microeconomic explanations) presupposes a teleological framework from folk psychology that is just as philosophically suspect as any of the standard philosophical charaterizations of the epistemologically moral character of scientists. Fuller says:

> The problem is that, contrary to his own intentions, by attributing agency to the scientists, Pickering has already supposed that they have the sort of *post facto* knowledge that he finds so objectionable in the philosophical accounts. The difference is that his scientists do not foresee hidden entities but hidden opportunities; they are master prognosticators of their own interests, if not the state of the external world.
>
> (1990: 671)

While neither Pickering in his original presentation, nor Fuller in his criticism, even mentions neoclassical economics, this criticism of Pickering by Fuller is very relevant to the general question of the economic analysis of science. The point of Fuller's criticism, the implicit folk psychology of Pickering's story about the behaviour of scientists, is precisely the same criticism that certain philosophers of science, particularly Alexander Rosenberg (1988, 1992), have levelled at microeconomics.[17] Thus, not only is it the case that certain ethnographic and historical studies in the SSK have (without any explicit consideration of economic theory) come to characterize the behaviour of scientists and scientific activity in a very (neoclassical) economic way, the similarities to economics are so great that critics of these 'sociological' studies (again without explicit consideration of economics) attack them on exactly the same grounds that economists have recently been attacked by philosophers of science.

In summary, it seems that the SSK exists in some intellectually parallel universe to the universe inhabited by most economists. It is a world in which economic explanations of individual behaviour as well as the social phenomena that emerges from that behaviour (along with some of the criticism of these explanations) clearly exist – not only *do* such explanations exist, but they are generally considered to be both credible and persuasive – and yet it is a world that seems to be totally without economics.[18]

THE ECONOMICS OF SCIENCE AND/OR THE ECONOMICS OF SCIENTIFIC KNOWLEDGE

The previous section makes clear that many of the studies in the SSK describe science in a way that is much like the way that it might be described if it were approached from an explicitly economic perspective. In this section I would like to discuss a few of the attempts to do just that: to approach science from an explicitly economic perspective.

The first work to consider is perhaps the first work ever written on the topic of the economic approach to science: a paper by the American pragmatist philosopher Charles Sanders Peirce (1967). In this rather amazing paper, Peirce discusses the 'economy of research' in a way that not only employs marginal economic analysis but does so in a very contemporary manner; this paper, originally published in 1879, would not seem too far out of place in a modern economics journal.[19] Peirce's approach is basically to maximize the utility obtained from various research projects subject to the cost constraint imposed by each project The first-order conditions require that the marginal utility per dollar of research cost be equated for each of the research projects undertaken; such a result, while certainly not surprising from the viewpoint of modern economics, seems rather astounding for 1879. As Wible (1992b) shows, Peirce's paper really amounts to a modern cost–benefit analysis of research project selection.[20]

Since nothing short of an actual (and extended) quotation from Peirce's paper could possible convey its contemporary style, the following contains most of the first three paragraphs of the paper.

> The doctrine of economy, in general, treats of the relations between utility and cost. That branch of it which relates to research considers the relations between the utility and the cost of diminishing the probable error of our knowledge. Its main problem is, how, with a given expenditure of money, time, and energy, to obtain the most valuable addition to our knowledge.
>
> Let r denote the probable error of any result, and write $s=1/r$. Let $Ur \cdot dr$ denote the infinitesimal utility of any infinitesimal diminution, dr, of r. Let $Vs \cdot ds$ denote the infinitesimal cost of any infinitesimal increase, ds, of s . . . . Then, the total cost of any series of researches will be
>
> $$\Sigma_i \int V_i s_i \cdot ds_i;$$
>
> and their total utility will be
>
> $$\Sigma_i \int U_i r_i \cdot dr_i.$$
>
> The problem will be to make the second expression a maximum by varying the inferior limits of its integrations, on the condition that the first expression remains of constant value.
>
> (Peirce 1967: 643)

As I said, this was a rather amazing paper for the time that it was published.

While Peirce's paper was clearly 'way ahead of its time' in many respects, it was, if examined in isolation from the rest of his pragmatist philosophy, only an early contribution to the *economics of science*, rather than an early contribution to the *economics of scientific knowledge*. If we mirror the distinction between the sociology of science and the sociology of scientific knowledge, then the economics *of science* would be the application of economic theory, or ideas found in economic theory, to explaining the behaviour of scientists and/or the intellectual output of the scientific community. That is, given the goals of the individual scientists or those of the scientific community (for example, the 'pursuit of truth') the economics of science might be used to explain the behaviour of those in the scientific community or to make recommendations about how those goals might be achieved in a more efficient manner. In this way the economics of science would relate to science in precisely the way that microeconomics has typically related to the firms in the market economy. Peirce's 1879 paper is a very early example of such an economics of science.[21] On the other hand, the economics *of scientific knowledge* (ESK) would involve economics in a philosophically more fundamental way. The ESK would involve economics, or at least metaphors derived from economics, in the actual characterization of scientific knowledge – that is, economics would be involved fundamentally in the epistemological discourse regarding the nature of scientific knowledge. Like the SSK argues that scientific knowledge comes to be constructed out of a social process, the ESK would argue that scientific knowledge comes to be constructed out of an economic process.

Although in isolation Peirce's 1879 paper is more an application of the economics of science than the ESK, Peirce's more general pragmatist philosophy of science does in fact contain elements of an economic theory of knowledge. For Peirce truth is simply that which the community of inquirers converges to over an infinite period of time. His notion of truth is inherently social – a community of inquirers is involved – but it is not merely a product of existing social conditions; it has an independent existence in that it is the limit (ultimate limit) of a process of inquiry by the inquiring community. As Thomas Haskell has characterized Peirce's position:

> The ultimate consensus to be reached by his community of inquiry is of a very special kind, and his theory of reality, though indubitably social, is not at all relativistic, as twentieth-century analogues have tended to be. Like Thomas Kuhn, he regarded science as the practical accomplishment of a community of researchers. Unlike Kuhn, however, he supposed that the universe was so made that an ultimate convergence of opinion was virtually predestined and that the reality

> toward which opinion converged was utterly independent, not of thought in general but of what any finite number of human beings thought about it.
>
> (Haskell 1984: 205–6)

The economic element in Peirce's view of science becomes clear when he discusses the nature of this 'convergence' towards communal truth; his story is basically a competitive story. For Peirce it is the 'economy of inquiry' that drives the inquiring community towards truth. The same economic principles that governed the choice of research projects in his 1879 paper, the maximization of return on our collective cognitive investment, that propels the scientific community towards its goal. Again quoting Haskell:

> Indeed, the entire process that causes what Peirce called "the most antagonistic views" to converge in the ultimate consensus is strangely reminiscent of the price mechanism in economic markets. There, in accordance with the natural laws of supply and demand, the jockeying of rival consumers and producers looking out for their own interests generates for each commodity a convergence towards its "natural price." In the community of inquiry the clash of erring individuals produces eventually a convergence of opinion about reality. No one in Peirce's community need feel love toward the other members, nor even love of truth, strictly speaking (since no individual's present ideas can be said to correspond with that opinion which the community will ultimately settle on).
>
> (1984: 211)[22]

Charles Sanders Peirce thus seems to be an earlier contributor to both the economics of science and the ESK. He discussed the 'economy of research' as a way of optimally selecting scientific research projects, but he also integrated this economic argument into his basic philosophy of science and theory of scientific truth.

The second author I would like to discuss, Gerard Radnitzky, is also a philosopher, although a contemporary one, and his view of science is also one that involves economics in both ways: as a tool for explaining what scientists actually do, and as an integral part of his characterization of scientific knowledge. Radnitzky has presented his 'economic theory of science' in a series of recent papers: Radnitzky (1986, 1987a, 1987b, 1989). His basic purpose is '*to investigate what may be gained from applying the economic approach, in particular cost–benefit thinking, to the methodology of research*' (Radnitzky 1986: 125, italics in original).[23] Radnitzky argues that by taking cost–benefit analysis (CBA) as our general point of departure it is possible to clarify a number of lingering controversies within the philosophy of science. In particular, he argues that CBA can help illuminate such philosophical questions as: why do certain scientists hold on to established paradigms even in the face of negative empirical evidence?[24] How are decisions made regarding what is and what is not to count as part of the empirical basis for a scientific theory? And when is it rational to prefer one scientific theory over another? Radnitzky does not say that the economic approach can conclusively 'solve' any of these philosophical problems, but rather 'that the CBA-frame may provide an organizing scheme that helps the researcher to see what sorts of questions he should take into account when dealing with such problems' (1986: 125).

These particular papers that apply the 'economic method' directly to questions of science and economic methodology are really only the tip of the iceberg regarding the importance of economics to Radnitzky's philosophy of science. Radnitzky subscribes to a particular brand of Popperian philosophy; he supports the 'critical rationalist' interpretation of Popper's philosophy associated with the work of W. W. Bartley III.[25] Since the critical rationalist interpretation of Popper is discussed elsewhere in this volume (in the paper by Lawrence Boland) there is no reason to reproduce the argument here.[26] All I would like to note is that in the Bartley/Radnitzky characterization, 'knowledge' emerges from the competitive process of scientific criticism in the same way that economic welfare emerges from the competitive market process. Their view essentially amounts to an invisible-hand argument for the growth of scientific knowledge and it depends fundamentally on their notion of 'criticism' and its role in error elimination. The Bartley/Radnitzky view also depends heavily on arguments from evolutionary epistemology to connect up these competitively emergent theoretical structures with the underlying physical world; their theory, unlike Peirce's for example, endeavors to hold on to scientific realism. In any case, Radnitzky's view, and to a lesser extent critical rationalism more generally, represents a direct application of economic reasoning to questions about the nature of scientific knowledge; it thus constitutes a version of ESK.

Finally, I would like to mention a few of the studies, some quite recent, and some not so recent, where economists, rather than philosophers, have undertaken various exercises in the economics of knowledge. These are primarily studies in the economics of science – applying the tools of economic analysis to the behaviour of a particular type of economic agent: a scientist – although some of these economists also consider issues that might be part of the ESK.

One of these works is Gordon Tullock's *The Organization of Inquiry*, published in 1966. This book, inspired by 'six months spent working with Karl Popper' (1966: v) applies an early public choice-type analysis to the study of science. For Tullock one of the identifying characteristics of science is that it is conducted in a scientific community and the book is primarily 'devoted to a discussion of this community, the organization which controls inquiry', the main feature of this community is that it is 'a system of voluntary co-operation' (1966: 63). Tullock discusses the incentives of and constraints on this community, and also reflects on a few traditional philosophical (particularly Popperian) questions.[27]

Boland (1971)[28] takes a different approach. He uses economics (particularly welfare economics) to attack what he calls 'conventionalist' methodologies – methodological approaches that attempt to choose the 'best' theory from a set of competing theories. On Boland's view any methodology is considered conventionalist if it recommends choosing scientific theories on the basis of the fact that they are 'more simple', or 'more general', or 'more verifiable', or any other criterion other than truth. In the paper Boland makes an analogy between these conventionalist theory choices and the choices involved in welfare economics. He then uses well-known problems in welfare economics, such as the Arrow impossibility theorem, to show that the conventionalist economic problem is 'unsolvable on its own terms' (1971: 105).

Finally, I would like to mention three quite recent papers that have directly applied 'the economic method' to the practice of science: Diamond (1988) and Wible (1991), (1992a). All three of these papers employ a Gary Becker-based model of rational choice to the problem of science (or a particular problem in science). Diamond (1988) applies Becker's general 'economic approach' to the problem of a rational scientist. The rational scientist is one who maximizes a utility function with 'scope' and 'elegance' as arguments, and faces constraints imposed by time and production functions. Diamond argues that such a 'maximization-under-constraints model holds promise of being able to account for scientific progress in a way consistent with the history of science' (1988: 150).

The papers by Wible are of a similar genre. Wible (1991) discusses the question of replication in science by means of Becker's model of the allocation of time, and Wible (1992a) discusses the question of fraud in science by means of the Becker/Ehrlich model of the economics of crime. All three of these papers are replete with formal economic analysis: first-order conditions, qualitative comparative statistics, Kuhn–Tucker conditions, and even optimization under uncertainty. The following quotation from Wible captures the general flavour of the analysis in, and the results available from, these three papers.

> Reconsidering the first-order condition describing the individual scientist's optimization under uncertainty, equation 5 has additional behavioral implications. The more negative or the greater the slope becomes in absolute value, the less fraud will be committed by the individual. This will occur if the probability of being discovered, P_f, increases, or if the penalty F_f associated with time spent in illegitimate activities increases. Either of these reduces the incentive to engage in

> fraudulent activities in science. If the marginal return to fraudulent activities increases relative to legitimate activities, then the individual would be expected to increase the proportion of time spent on illegitimate activities.
>
> (1992a: 19)

These papers are clearly *the economics of science* and they are presented in the language and discursive format of contemporary economics. These papers *may not actually be* any more of a direct application of microeconomics to science than the earlier work by Tullock, Boland or Radnitzky (or even Peirce for that matter), but they certainly *seem* to be, perhaps because of the mathematics, the most direct application of economic analysis to science available in the literature.[29]

The discussion of these quite recent Becker-based models ends the survey portion of the paper. After discussing many of the quasi-economic arguments available in the sociological literature, and also discussing a number of the direct applications of economic theory to science, it is time to return to more philosophical issues.

CAVEAT ECONOMIST: CIRCULARITY, REFLEXIVITY AND EPISTEMIC ANOMIE

Criticisms of the SSK (particularly the strong programme) are legion.[30] From outside the SSK most of this criticism has come from philosophers, but a few philosophically oriented historians of the various scientific disciplines have also been involved. Within the SSK, disagreement has come primarily from one school criticizing the other, although the constructivist approach is sufficiently diverse that serious disagreements often occur between various individuals who share this general point of view. Out of all of the many criticisms that have been, or could be, raised, I would like to focus on only one: the problem of *reflexivity*. The reason for focusing on this one problem is two-fold. First, this seems to be the *major problem*; it gets the most attention in the literature, and many of the other controversies seem to be derived from it. Second, it is also a problem that will trouble any attempt to apply economics to the study of science.

The problem of 'reflexivity' arises in the following way. The SSK argues that science is the product of its social context, either the social interests of the scientists involved or other social factors that constitute the social context of science. What scientists observe and the theories they propose are not simply given by the external world 'out there', but rather it is constituted by the social context 'in here'. Now if one accepts this basic claim of the SSK, then should it not also be true for the sociologists who are doing the SSK as well? The sociologists doing the SSK are a community of scientists; if what a community of scientists produces is constituted by its social context, then the output of the SSK will also be a product of, and constituted by, its social context. As Alexander Rosenberg expresses it: 'This sort of sociology pulls itself down by its own boot straps' (1985: 380). Focusing particularly on the strong programme, Rosenberg characterizes the problem in the following way:

> Proponents of the "strong program" in the sociology of science [Bloor 1976; Barnes 1977] suggest that science is nothing more than such a social institution, and must be understood as such. But if this argument is correct, it must be self-refuting. If scientific conclusions are always and everywhere determined by social forces, and not by rational considerations, then this conclusion applies to the findings of the sociologist of science as well.
>
> (Rosenberg 1985: 379)

Reflexivity is an extremely important issue for the SSK. Many of the advocates of the SSK claim to undermine the hegemony of the natural sciences by showing that what is purported to be objective and 'natural' is neither one of these things, but rather simply a product of the social context in which it is produced. If this is true for all human inquiry, then it must be true for the SSK as well; this makes everything socially/context dependent and thus *relative*.[31] This leads many of those writing the SSK to a version of 'sociological skepticism' (Kitcher 1992) where individuals are always trapped in the categories of a particular social context (or in Wittgensteinian terms, trapped in a particular language game); individuals are either unable to escape these social categories, or if they can escape, there is no way of rationally deciding among the various frameworks that are available.

Collins and Yearley (1992a,b) – both advocates of a version of constructivism, but critical of sociologists who revel in such relativism – refer to this tendency within the SSK as 'epistemological chicken'; they characterize this relativist tendency in the following way:

> In sum, following the lead of the relativists, each new fashion in SSK has been more epistemologically daring, the reflexivists coming closest to self-destruction. Each group has made the same mistake at first; they have become so enamoured of the power of their negative levers on the existing structures as to believe they rest on bedrock. But this is not the case. Though each level can prick misplaced epistemological pretensions, they stand in the same relationship to each other as parallel cultures; no level has priority and each is a flimsy building on the plain. Accepting this we can freely use whatever epistemological "natural attitude" is appropriate for the purpose at hand; we can alternate between them as we will. That is what methodological relativism is all about – the rejection of any kind of foundationalism

> and its replacement, not by permanent revolution but by permanent insecurity. To reverse the vertical scale of the metaphor, while SSK showed that science did not occupy the high ground of culture, the newer developments must be taken to demonstrate not the failure of SSK, but that there simply is no high ground.
>
> (Collins and Yearley 1992a: 308)

The particular solution offered by Collins and Yearley is 'social realism' – let social scientists 'stand on social things' to explain natural things (1992b: 382) – but other authors have other solutions.[32] Bruno Latour for example, one of the SSK authors who seems to delight in the problems (opportunities?) of reflexivity, sees the situation quite differently than do Collins and Yearley:

> A few, who call themselves reflexivists, are delighted at being in a blind alley; for fifteen years they had said that social studies of science could not go anywhere if it did not apply its own tool to itself; now that it goes nowhere and is threatened by sterility, they feel vindicated.
>
> (Latour 1992: 272)[33]

There are obviously a number of different responses to the reflexivity issue within the SSK. My purpose is not to attempt to defend one of these positions over the others; the purpose is only to point out that *all* of the authors involved in the recent SSK feel impelled to give *some response* to the question of reflexivity and the relativism (that many suggest) it implies. While these questions are clearly relevant to the SSK, it could be argued that if one were only concerned with the *sociology of science*, rather than the SSK, then the problems of reflexivity and relativism would not be relevant (since the cognitive content of scientific theories remains unexamined in the sociology of science). But even here, in the sociology of science, there is a problem if the tools are turned on the carpenter. Even when the focus is only on the culture of science and not on the content of the theories offered, there is clearly a potential problem of regress and circularity when the sociology of science looks at the science of sociology.

Let us now return to the economics of science literature from the previous section; does the reflexivity question also impact the economics of science? The immediate answer is clearly yes. Suppose that one is engaged in a public choice-type analysis of science; for example, suppose that we view individual scientists as acting in their own rational self-interest given the market for professional credibility. This public choice (scientific choice) analysis could just as easily be applied to practising economists – in fact, it could even be applied to the specific public choice economist that was examining the behaviour of scientists. The circularity or reflexivity problem thus occurs in the economics of science just as it does in the sociology of science.

Much of the previously considered work on the economics of science encounters such reflexivity problems. For example, in Gordon Tullock's discussion of science, he characterizes (and implicitly criticizes) fields where teaching responsibilities dictate a larger number of participants than research opportunities can accommodate in the following way:

> One symptom of the existence of this condition is the development of very complex methods of treating subjects which can be readily handled by simple methods. Calculus will be used where simple arithmetic would do and topology will be introduced in place of plane geometry. In many fields of social science these symptoms have appeared.
>
> (Tullock 1966: 57)

In this case Tullock is using his economic approach to science to explain the rise of mathematical economics. Now in this particular case Tullock would not recognize a reflexivity problem since he would probably consider himself to be applying the right kind of economics to the question of how the discipline came to pay so much attention to the wrong kind of economics. On the other hand, suppose that Tullock had actually produced a formal model of the behaviour of scientists in an overpopulated field and mathematically deduced his prediction regarding their tendency to formalize. In this case the reflexivity problem would be much more obvious, though no more present, than it was in Tullock's original.

The reflexivity problem also surfaces in the contributions by Wible (particularly 1991 and 1992b). In Wible (1991) the economic approach to science is applied to the question of empirical replication in science, but the particular case he considers is replication in econometrics. Wible is quite explicit about his desire to apply the economics of science to the science of economics.

> While the rational expectations and public choice revolutions have been extremely successful at the theoretical level, there is another domain of economic activity to which the postulate of economic rationality could be applied. This category of human behaviour virtually has been ignored by economic theorists. The domain of human behaviour I have in mind is none other than positive economics itself. Economic methodologists and most others who study the professional behaviour of economists usually do not presume that economists are economically rational.
>
> (Wible 1991: 165)

There is something very curious about explaining the activity of economists on the basis of an economic theory of behaviour. Should we not then be able to explain the behaviour of the economist who is trying to explain the behaviour of economists on the basis of economic theory, on the basis of economic theory too? Where do such explanations end?[34]

Wible's most recent contribution (1992b) contains a related, and perhaps ever more unsettling, argument. In Wible (1992b) the (neoclassical) economic argument of Radnitzky and Bartley (1987) – the argument that competition in the marketplace of ideas is good for the growth of knowledge – is actually used against neoclassical economics. Knowledge requires competition, but, according to Wible's analysis, there has not been any real competition in the marketplace of economic ideas; rather than competition, economics has been dominated by the monopoly of the neoclassical approach. Thus a neoclassical-based philosophy of science is used to argue for the elimination of the neoclassical hegemony in economics. This seems a bit like throwing oneself out with one's own bath water.

Wible (unlike some of the other contributors to this literature) actually recognizes the self-referentiality and the possible methodological circularity raised by his economic approach. He ends with the following paragraph.

> After presenting the model and its applications, some possible intellectual problems arising from this point of view were raised. Specifically how far can an economic explanation of economics and other sciences be taken? The logical extreme would be that the rationality of science is completely explainable in terms of economic analysis – that economic science itself is primarily an economic phenomenon. The problem with such a position is that it would reduce all philosophies and methodologies of science to economics. Logically this would deprive economics of an independent philosophical or methodological standard of scientific objectivity. Objective knowledge would be impossible. The alternative is to recognize that economics construed as rational maximizing behaviour is incomplete and cannot be universally applied – that it can add greatly to an understanding of professional scientific conduct but not explain everything.
>
> (Wible 1991: 184)

If one stops just before the last sentence, this quotation contains a clear statement of the 'relativity' issue from the SSK. In this case it is a relativity induced by the economic argument that all science is simply the maximizing behaviour of individual scientists, rather than the relativity induced by the sociological argument that all science is socially constituted, but it is relativity nonetheless. Because of the individualism of (neoclassical) economics, the economic approach will characterize science as the product of individual rather than social interests, but the result is a scientific practice that is just as devoid of the traditional cognitive virtues as is the case for the SSK.

None of this discussion of reflexivity or relativism is meant to suggest that the economics of science is not a worthy endeavour, or that economists should be scared off by some of these potentially unsettling epistemological implications of this work. The argument is only that we should *recognize what is at stake and enter into such studies with our philosophical eyes wide open.* Economists interested in the economics of science should be knowledgeable and informed regarding the SSK, and in particular, they should be aware of the potentially quite radical implications of some of this work. If one wants to employ economics, and yet remain safely ensconced within a traditional view that allows for only one universally valid 'scientific method', then something akin to the Peirce or Bartley/Radnitzky approaches would seem to be required. On these views knowledge has (some) objective status, but the (universally valid) way of obtaining that knowledge depends on a process that is fundamentally economic. On the other hand, if one is willing to abandon the traditional philosophy of science, and to accept a characterization of knowledge that is fundamentally local, tentative and contingent, then economics, along with a variety of other framework and discursive strategies, could be involved in the inquiry.[35] The point is that whichever of these two general approaches one chooses to pursue, there is serious philosophical work to be done; the economics of science is more than looking at first-order conditions for scientists' optimization problems and/or noting that the marketplace of ideas actually looks like a marketplace. If economists are to make any contribution to human understanding of science or scientific knowledge then they need to scrutinize the sociological literature quite carefully. Economists bring a number of tools to the project that are quite different from the tools of the sociologist; economists think in terms of optimization, information, incentives and equilibrium; economists also think of individuals creating societies rather than societies creating individuals. While the tools are different, and in some cases perhaps mutually exclusive, neither the general task nor the pitfalls entailed are necessarily different. Those involved in the SSK have travelled through much of this wilderness before us, and to neglect their signposts would surely be a folly.

CONCLUSION

In this paper I have traced the history of the SSK and discussed the main themes of its two principal schools. I have also discussed a number of places where the SSK produces narratives that sound much like what one would expect from an economic analysis of science. In addition, I have surveyed a number of attempts to explicitly apply economics to science, some by philosophers and some by economists. Finally, I examined the problem of reflexivity and discussed some of the philosophical difficulties it raises for the SSK. I argued that such problems will also be encountered by anyone pursuing the economics of science, and that while such problems are not so great as to deter entry, they need to be recognized and that there is much to learn from the literature on the SSK. In conclusion I would like to note that while the ESK (and the SSK for that matter) represents a great opportunity for those interested in economic methodology and the history of economic thought, this is not an inquiry that will come easy for most economists.

In one respect of course, the economics of science is an inquiry that *should* come easy for economists. As stated above, it seems to be an obvious next step for 'the economics of': a veritable gold mine for the Chicago and Virginia schools. There are a myriad of opportunities, not only for Becker-type economics and public choice theory, but also game theory, principal-agent analysis, incentive compatibility, and so on. For years economists have undermined and delegitimized the self-righteousness of politicians – 'you are not acting in the societal or national interest, but in your own self-interest' – now the same argument can be applied to scientists. Economics is also a discipline of 'unintended consequences' and 'invisible hands'; it is not necessary that politicians or scientists be motivated by 'higher' values in order to have the emergence of results that are consistent with such higher values. For economists, unlike for most others in modern intellectual life, the ubiquitousness of narrow self-interest in science or elsewhere, does not necessarily initiate a wringing of hands or lamentations about lost utopias; it only initiates a conversation about proper prices, compatible incentives and binding constraints.

Despite all of this, I suspect that the economics of science will be quite difficult for most economists to accept, even those normally engaged in economic methodology. The main problem is that most economists are epistemologically quite traditional. For most economists there is simply a world 'out there' and good science represents that world accurately. Such good science has something to do with letting the data decide between theories – confirmation for some authors and falsification for others – but 'the facts' in either case. This method is, for most economists, the way real science proceeds; it is the way that economics should proceed, and whatever problems the discipline has are generally a result of failing to follow this method of good science. Such views by economists are worlds away from the 'negotiation over facticity' and 'the social construction of nature' that pervades much of the SSK. There are many places in the SSK where the *a priori* distinction between the 'natural' and the 'social' is totally abandoned; the world is socially co-produced by a variety of actants (agents who may or may not be humans) who engage in negotiation over its construction; in one recent case these actants included the scallops of St Brieuc Bay who actively negotiated with researchers regarding their anchorage.[36] Such stories involving negotiation by non-human actors are unusual of course, even in the more radical versions of the SSK, but the point is simply that almost anything is up for grabs. Not all of the SSK has such a post-modernist flair, but much of it does, and such argumentation cannot be ruled out of court from the general perspective of the social construction of knowledge. As I argued above, any attempt at an ESK will require economists to take the arguments and the insights of the SSK quite seriously. This is what should be done and the prospects are exciting; it will not be easy for many economists to do.

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NOTES

1 The work of Merton is often characterized as a reaction to the Marxist external histories of science written during the 1920s and early 1930s (Hessen 1931, for example). The Marxist influence on the early development of the sociology of science is emphasized by Collins and Restivo (1983) and Bunge (1991, 1992).

2 It should be noted that Kuhn did not stress an ontological interpretation of this collectively constructed world. For Kuhn, unlike for some of the more radical among the recent sociologists of scientific knowledge, there always seems to be something 'out there' that is fixed and unchanged as the scientific paradigm, and thus what the scientists 'see', is transformed. When the 'duck' in the optical illusion is transformed into a 'rabbit' before our very eyes, the paper and the marks on it remain unchanged. As Kuhn stated in 'Second thoughts on paradigms':

> In *The Structure of Scientific Revolutions*, particularly chap. 10, I repeatedly insist that members of different scientific communities live in different worlds and that scientific revolutions change the world in which a scientist works. I would not want to say that members of different communities are presented with different data by *the same stimuli*. Notice, however, that that change does not make phrases like 'a different world' inappropriate. The given world, whether everyday or scientific, is not a world of stimuli.
>
> (Kuhn 1977: 309, n. 18, italics added)

3 Other philosophical influences on the SSK include L. Wittgenstein's theory of the social/conventional nature of language use and the under-determination thesis associated with the work of Pierre Duhem and W. V. O. Quine. See Bloor (1983) for a discussion of Wittgenstein in this context, and Roth (1987, esp. ch. 7) for a discussion of Duhem and Quine.

4 The literature on the strong programme (and the literature critical of it) has become rather extensive over the last two decades. Major contributions to the strong programme include: Barnes (1977, 1982), Bloor (1976, 1983) and Shapin (1982). The relationship between the strong programme and the discipline of economics is considered in Coats (1984) and Mäki (1992, 1993).

5 This of course is not to suggest that the notion of 'cause' is a philosophically

simple notion. It is only to say that the strong programme does not make any original contribution to, nor does it try to make any contribution to, the philosophical discourse on the nature of causality.

6 As Mäki states in his recent discussion of the sociology of scientific knowledge and economics:

> It is believed by Bloor that all this amounts to applying the principles of science to science itself. The program is radically pro-science. More particularly, it is based on a naturalistic methodological monism. Unlike some other currents in the sociology of science, Bloor's programme is strongly anti-hermeneutic.
>
> (Mäki 1992: 68)

7 One causality question that has been raised regarding the strong programme is the programme's failure to specify any explicit causal mechanism connecting social conditions with the formation of beliefs (see Roth 1987: chapter 8 on 'Voodoo Epistemology'). Another criticism has been that if there actually is an implicit causal mechanism in the strong programme, it is a type of 'social behaviorism' that is replete with its own philosophical problems (see Slezak 1991).

8 This alternative school – if 'school' is even the appropriate term – is fairly amorphous. It would include such disparate views as the micro-sociological approach of Collins (1985), the ethnographic approaches of Latour and Woolgar (1986), Latour (1987) and Knorr-Cetina (1981), and the 'science as practice' approach of Galison (1987) and Pickering (1984). The McMullin (1992) and Pickering (1992a) volumes contain an excellent collection of papers discussing the similarities and differences among these various views. The papers in the McMullin (1988) volume relate the literature on the SSK to other recent developments in the philosophy of science.

9 At this point the only self-consciously constructivist study in economics is Weintraub (1991). In addition to this major work on the (constructivist) history of stability theory in general equilibrium, Weintraub has also discussed the constructivist viewpoint in a number of shorter papers (1992, for example).

10 For the remainder of this paper 'economic' will mean 'microeconomic'; given our desire to explain the behaviour of individual scientist agents (or the intended and/or unintended consequences of their individual actions) macroeconomics does not seem to be particularly useful. The 'economics' that will be considered as a possible candidate for studying the economics of science is the standard neoclassical theory of rational choice. As the discussion proceeds it will become obvious that certain strains of neoclassical economics, particularly public choice theory and the microeconomics of the older Chicago school, are better suited to this particular task than other strains such as mathematical general equilibrium theory, but only neoclassical microeconomics (in some form) will be considered.

It is also important to note that 'economics' will not include Marxian economics. While this exclusion will probably not surprise (nor bother) most economists, it is important with respect to the SSK. As stated above (see note 1) there is a Marxist tradition in the SSK (or at least the sociology of science) and for Marxist social theory there is little difference between a 'sociological' explanation and an 'economic' explanation; the economic mode of production determines the social relations. Thus, in this general sense, all of the literature on the Marxist sociology of science is actually on the economics of science broadly defined. Economic explanations are so much associated with Marxian explanations in the SSK that authors often reject 'economic' explanations altogether on the basis of the failure of Marxian explanations. For example, the sociologist of science Karin Knorr-Cetina uses the following argument to reject 'economic theory' as a source of inspiration for the study of science.

> In economic theory, the notion of capital is linked to the idea of exploitation defined in terms of the appropriation of surplus value, and to the corresponding concepts of class structure and alienation. Without adequate conception of exploitation and class structure, the capitalist model loses its most distinctive characteristics. But how are we to conceive of exploitation and class structure in scientific fields said to be ruled by capitalist market mechanisms?
>
> (Knorr-Cetina 1982: 108)

Needless to say, this critique of the economic approach to science has nothing to say (one way or another) about the application of neoclassical microeconomics to the study of science.

11 This distinction, the distinction between sociologists that employ economic-type arguments in the study of science and those who explicitly apply economics to science, is of course arbitrary. The distinction requires the assignment of an unobservable methodological intent to the various authors involved. Nonetheless, while it is not without problems, the distinction can be, and will be, usefully maintained in what follows.

12 It should be noted that while Latour and Woolgar's market for credibility sounds very much like the standard neoclassical characterization of a competitive market, their story is, in many respects, quite anti-neoclassical. In the standard neoclassical story of the market the actions of individuals are primary and explanatory – the behaviour of the market prices and quantities is 'explained by' and 'determined by' the actions of (ontologically primary) individual agents – and not the other way around. Latour and Woolgar are not this individualist, nor are they as linear in their explanatory thinking. For Latour and Woolgar there is a market, and there are credibility-seeking actions by individual scientists, but the latter does not necessarily cause the former. This process involves the market, the scientific culture, the individual scientists, the instruments and a myriad of other factors: all wrapped up in an amalgamated and mutually co-determining ensemble of interdependent influences. Latour and Woolgar certainly have a market in their story, but it is not the standard neoclassical market. As they say:

> This consideration is important because we certainly do not wish to propose a model of behaviour in which individuals make calculations in order to maximize their profits. This would be Benthamian economics. The question of the calculation of resources, of maximization, and of the presence of the individual are so constantly moving that we cannot take them as our points of departure.
>
> (1986: 232, n. 10)

13 Two other sociological studies of science that draw on economic analogies are Bourdieu (1975) and Hagstrom (1965). Both of these studies are discussed in Latour and Woolgar (1986) and Knorr-Cetina (1982). Bourdieu is also discussed in Mäki (1992).

14 The warning in note 11 is still applicable of course.

15 The *CQ* in this quotation refers to *Constructing Quarks* (1984).

16 Pickering clarifies the role of 'reality' in his response to Roth and Barrett: Pickering (1990).

17 This paper on SSK is not the place to attempt a general discussion of the relationship between economics and folk psychology. See Rosenberg (1988: ch. 2; 1992) or Hands (1993: ch. 11) for a more general discussion.

18 One of Pickering's comments is particularly telling in this regard. In his response to Roth and Barrett (R&B) he emphasizes the multifaceted and interdisciplinary nature of his work by saying: 'My model of practice – as expressed in CQ and as elaborated here – seems to me to touch upon the legitimate interests of a variety of disciplines: sociology, certainly, but also psychology, cognitive science, anthropology and even, to return to R&B, philosophy' (1990: 709).

19 I am indebted to James Wible for drawing my attention to this important early contribution of C. S. Peirce. Wible (1992b) discusses Peirce's paper in detail along with the work of Nicholas Rescher, a contemporary philosopher of science influenced by Peirce (see Rescher 1976). Peirce's work has recently appeared in the philosophical discussion surrounding the SSK (see Delaney 1992) and it is also examined in Kevin Hoover's contribution to the current volume.

20 Wible compares Peirce's paper to a 1965 paper by Frederic Scherer on the utility approach to research project evaluation, and finds the resemblance to be 'striking and uncanny' (1992b: 14).

21 The last paragraph of Pierce's paper is very interesting in this respect.

> It is to be remarked that the theory here given rests on the supposition that the object of the investigation is the ascertainment of truth. When an investigation is made for the purpose of attaining personal distinction, the economics of the problem are entirely different. But that seems to be well enough understood by those engaged in that sort of investigation.
>
> (Peirce 1967: 648)

22 Also see Delaney (1992) and Wible (1992b).

23 Radnitzky defines 'cost–benefit analysis' (CBA) quite broadly. He mentions neoclassical economics, Gary Becker in particular, but he also considers the approach to be general enough that it includes Austrian economics as well as Popper's 'situational analysis' approach to social science (see Hands 1985, for a discussion of the latter). On the question of the generality of CBA, Radnitzky says:

> Man is a chooser. *All* rational choices involve the weighing up of benefits and costs. Hence, CBA is the core of the economic approach/rational-problem-solving approach.
>
> (1986: 127)

24 This question is also considered in Ghiselin (1987).

25 See Bartley (1984, 1990) and Radnitzky and Bartley (1987).

26 In addition to the Boland paper in this volume and the works cited therein, the argument is presented in detail in Hands (1993: ch. 11) and also in Wible (1992b).

27 Tullock's comments on the sociology of science are worth quoting:

> I have, for example, read *The Structure of Scientific Revolutions* with profit and pleasure, but it will not be further mentioned in this book. This is not because I regard it as unimportant but because it deals with different problems. In this it is typical. Most of the recent work has

> been done by people whose basic orientation is sociological, while mine is economic. There is no necessary conflict between sociologists and economists, but they do ask rather different questions.
>
> (1966: v-vi)

28 Reprinted as chapter 5 of Boland (1989).

29 The examples I have discussed do not exhaust those in the literature. Mäki (1992) discusses papers by Earl (1983) and Loasby (1986), and Diamond (1992) provides a survey of much of this literature as well as the somewhat related literature on natural science's contribution to economic growth.

30 One of the reasons the strong programme has borne the brunt of the criticism is simply that it has been around longer than most of the constructivist approaches. A second reason for the strong programme being targeted is its rather narrow and carefully delineated methodological stance; critics generally prefer targets that are clearly defined and stable over those that are ill-defined and constantly moving.

31 I am intentionally giving a rather relativist reading of the reflexivity problem here. It is certainly the case that many authors in the SSK, particularly those in the strong programme, do not see reflexivity as having such relativist implications. For these authors, science proceeds by induction; the fact that most natural scientists act consistently with their social interests, has nothing to say about the legitimacy of the sociology of science that correctly applies the inductive method to the study of the behaviour of those in the scientific community. The strong programme would admit that the sociologist's tools could be turned on the sociology of science itself; that is just not what they are interested in doing; they are interested in studying natural science.

32 The exchange between Collins and Yearley (1992a, 1992b), Callon and Latour (1992) and Woolgar (1992) offers various interpretations of the reflexivity problem. Collins and Yearley (1992a) discuss three approaches to the problem: 'reflexivity' (associated with Woolgar and others), the 'French School' (associated with Latour and others), and 'discourse analysis' (associated with Mulkay and others). Of these three approaches, the only one that is (thus far) directly relevant to economics is discourse analysis; McCloskey's much discussed work on the 'rhetoric of economics' (1985) is basically discourse analysis applied to economics. The Woolgar edited volume (1988) contains a number of different papers that wrestle with the reflexivity problem in a variety of ways.

33 Perhaps the most radical reading of reflexivity is given by Steve Woolgar. For Woolgar the primary reason for doing the SSK is its potential for radical reflexivity: the 'potential for reevaluating fundamental assumptions of modern thought' (Woolgar 1991: 25). According to Woolgar, the SSK has failed to fully exploit the radical implications of its reflexivity – or perhaps failed because it has not fully exploited the radical implications of reflexivity. For him:

> the radical potential of SSK has been compromised because it has failed to interrogate its concept of hardness and thereby failed to exploit the analytic ambivalence at the heart of its practice. With a few encouraging recent exceptions, SSK has not addressed its own dependence upon conventions of realist discourse. Consequently, SSK fails to address the issue of representation at a fundamental level; it seems set to become another exemplification of the relativist–constructivist formula rather than an occasion for questioning the idea of applying formulae altogether.
>
> (Woolgar 1991: 43)

34 A similar reflexivity occurs in Grubel and Boland (1986) where mathematical economists are characterized as an interest group that attempts to capture economic rents for its members, and also in chapter 2 of Mayer (1993) where the economics profession's emphasis on formalism is attributed to market failure within the discipline.

35 This latter approach seems to be suggested in that last line of the above quotation from Wible, and it is also one of the main themes in Philip Mirowski's contribution to this volume.

36 See Collins and Yearley (1992a) and Callon and Latour (1992).

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