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Fragments of a Transepistemic Discourse: Political Economy of Scientific Knowledge and Sociology of Economic Knowledge

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> *it is always with a joyful and positive emotion that we hear those bold statements made by men of science who, for a mere question of professional honour, come to tell the truth - a truth which only interests them because it is true, and which they have to cherish in their art without hesitating to displease those who see it in a very different light and who regard it as part of a mess of considerations which interests them very little.*
>
> Marcel Proust - *Jean Santeuil* (1952).

I. Introduction

For those still endowed with an essentially idealist conception of science, scientific practices should be conceived as being guided exclusively by their very imanent logic. Even though it is recognized that scientists are first, and by definition, social beings, their professional activities are not thought to be affected by elements other than those supposed as being naturally pertaining to scientific realm. Put another way, and somewhat crudely, both the content and *modus operandi* of scientific activity are seen to be invariant with respect to, and independent of, any allien influences eventually exerted by a variety of supposedly non-scientific factors; even though a few would deny their existence, the latter are usually assigned a very minor, if any, role in influencing scientific process: Ocam has been always ready to put his razor at service of avoiding any undesirable trundle into the midst of scientific purity.

As it seems to be always the case with any radical approach, the idealist vision of the scientific *rationale* has been increasingly challenged within sociology of science since its very early beginning; disagreeing voices, though then largely isolated and of little ressonance, did not fail

to point out the very reductionist nature of idealist picture even in Comtian times, e.g., Marx and Engels. Notwithstanding Durkheim, Mannheim and Merton made important contributions to a sociological approach to science in the first half of this century, one could say that a fundamental step in the consolidation of a consistent alternative to idealist vision was taken with the emergence of the so-called Sociology of Scientific Knowledge (hereafter SSK) roughly in the early 1970's. Following Mannheim and Merton, SSK literature conceives scientific knowledge as being created by human beings and developed, nurtured, and shared among social groups. As an essentially social activity, science is clearly a product of processes envolving human actors who have lives not only in science, but also in the broader societies of which they are members. Adopting a fundamentally social, non-idealist, approach to scientific activity as a fundamental starting point, SSK practioners build on early contributions to sociology of science and to sociology of knowledge and insist upon the largely social nature of scientific knowledge: scientific activity is argued to be practiced in an essentially social context, the products of scientific practice are said to be manufactured in a very social process, and scientific knowledge is conceived as being socially constructed.

The primary purpose of this paper is to evaluate to what extent, if any, SSK tradition and political economy tradition can benefit of each other in the consolidation of their particular discourse, specificities notwithstanding. The general question to be dealt with here is to what extent a broader, and somewhat more unfamiliar, agenda for exchange of ideas between these epistemic discourses can contribute to a mutual strengthening of their heuristic value. In short, it evaluates the extent to which Bloor's notion that it is very intrinsic to the scientific knowledge that any detailed account of it should despoil academic boundaries, for these boundaries contrive to keep some things well

hidden (1976, p. ix), justify the incorporation of political economy into SSK literature.

As the usual caveat regarding the impossibility of discussing all the myriad of aspects suggested by this somewhat excessively broad question really applies *in toto*, a better defined statement of purpose is required. First, it is discussed, from an epistemological perspective, to what extent SSK approach can benefit from the incorporation of political economy in its building of a social account of scientific practices; in other words, do SSK practioners' shared conception of scientific knowledge as being socially manufactured necessarily create the very imperative of incorporating political economy into that social account? Given that several branches of sociology of science have already been using either Neoclassical or Marxian elements in some of their economic account of scientific knowledge, the second question to be dealt with regards to what extent a more consistent explanation of the economic dimension of scientific practices can be built by using some different approach to economic theory. Last but not least, it is discussed to what extent political economy itself can benefit from an incorporation of some of its content by SSK practioners; put another way, to what extent can political economy benefit from a sociology of economic knowledge engendered by an active joint-venture between itself and SSK?

The paper is organised in the following way. Section II briefly outlines, though not from a *whig* perspective, some intellectual roots of SSK tradition, in particular the love/hate-type inspiration drawn from the earlier Mertonian sociology of science. Section III presents some main differences among the several branches within the SSK. The forth section critically surveys some contributions to an economics of scientific knowledge, as well as some attempts within sociology of knowledge itself to take the economic metaphor a step further. After evaluating to what extent can

SSK really benefit from the incorporation of economics in its building of a social account of scientific practices, it is suggested that a more consistent explanation of economic dimension of scientific activity can be built using elements from an alternative approach to economic theory. By viewing the philosophical issue of reflexivity as one capable of engendering important epistemological breaks, this section also evaluates to what extent political economy itself can benefit from an incorporation of some of its content by SSK practioners.

II. Some early contributions to sociology of science

It is almost consensual that the modern macrosociology of science begins with the path-breaking sociological work of Robert Merton in the late 1930s [(1)] . Moreover, one can sustain that sociology of science as a truly distinct specialty emerged in the United States in the 1950s as a result of the contributions of Merton and his followers [(2)]. Even though others before him, including several sociologists, had already investigated several social aspects of science, one can argue that only Merton and his co-workers and followers made a conscious effort to establish a definition of the area, a conceptual framework, and a research program, and they were the first to make a conscious and articulated effort at gaining recognition for the field as a really distinctive branch of sociology [(3)]. Even though the literature on the SSK tradition draws its intellectual inspiration from a wider range of sources, one can argue that the earlier sociology of science of the Mertonian tradition - though reinterpreted from a Durkheim-Mannheim perspective - together with the historicist approach to philosophy of science developed by authors such as Thomas Kuhn (1962), constituted a major source of intellectual influence [(4)]. Merton's first work, his Ph.D. dissertation in 1935, was a comprehensive analysis of the

process of institutionalization of natural science in seventeeth-century England. In this work, Merton's main conclusion was that the development of natural science was largely promoted by the Puritan ethic of seventeeth century England [5]. Like Mannheim's 1929 work, *Ideology and Utopia*, Merton's was an externalist history of science, that is, it argued for the interdependence of science and a variety of extrascientific factors, both ideal and material: Puritan values and economic and military imperatives, in Merton's work; effects of class membership on the ideas and thought patterns of scientists, and so its very implications for the nature of scientific knowledge, in Mannheim's work [6]. As an exercise in externalist sociology of science, their work begin with the observation that scientists are also members of society, membership meaning in this context that they locate themselves somewhere, in some social class, status, and roles. As a consequence, their experience in this location, direct or empathetically shared, inevitably affects their thinking (Diesing 1991, p. 164).

Merton's framework is also an essentially functionalist theory of the social characteristics of what Kuhn called as normal science. It is a theory both of the structural requirements for a scientific community to maintain itself and of what is likely to happen when these requirements are not adequately fulfilled. For a Parsonian functionalist these are naturally among the fundamental facts to be known about any social institution. Society is conceived as a complex interaction among a small set of institutions, each with its own proper goal: the goal of the economy is to mediate between society and natural resources to produce goods and services, the goal of families is to socialize new members, the goal of the polity is to maintain the system, and the goal of science is truth. Each institution has its own structure of relations that hopefully operates to produce the needed results: a structural deficiency, or interference by other institutions, weakens the productive

process and lead to deficient results (Diesing 1991, p. 150). Despite the fact that Mertonian functionalism differs from that of Parsons on a few points, one can argue that they share the same common *structure* (7).

Put differently, Merton's framework hinges on the tenet that science is a distinctive kind of social activity, conforming to specific values and norms. Merton called the complex of these values and norms as *scientific ethos*, and argued that in fact it is precisely the scientific ethos that defines the scientific role (8). But as Hands (1992, p. 5) correctly asserted, it should be emphasised that Merton's sociology of science is in fact a sociology *of science*, rather than a sociology *of scientific knowledge*: neither Merton nor his followers really questioned the objective validity of our scientific knowledge. Since this objective independence of scientific knowledge is not endorsed in most SSK literature, the latter has been devoted to a sociology of scientific knowledge, rather than to a mere sociology of science.

For the Mertonian tradition, science is to conceived as employing a particular scientific method that provides reliable knowledge about the objective world, perhaps even the truth; sociology, in turn, is conceived as merely providing intellectual background for attempts at explaining the particular social context that allows such objective knowledge to be discovered. Put another way, for the Mertonian tradition knowledge is not ultimately social: though there are several external factors that, by virtue of being likely to affect the development of knowledge, can be studies by the sociology of science, the knowledge itself is not ultimately determined by these social factors. Unlike SSK practioners, Mertonian tradition clearly distinguishes the conceptual content of science from its social context: even though science is claimed to be influenced by its social context, neither all knowledge is social nor it has a social content. Not surprisingly, I would adduce, the recent

SSK literature not only maintain a love-hate relationship with the Mertonian tradition, but, and by this very reason, draws much of their intellectual inspiration from the early sociology of knowledge of Durkheim and Mannhein, rather than from Parsons and Merton [9].

Much of the inspiration for the SSK contributors's claim regarding the constitutively social nature of knowledge itself was drawn from the work of the historical philosopher of science Kuhn. Some SSK authors regard Kuhn as an exemplar externalist sociologist of knowledge and even the father of the SSK tradition (e.g., Barnes 1982). Indeed, Ravetz, who is usually cited as a very predecessor, asserted that Thomas Kuhn "has, as it were, created the new paradigm which we all follow" (1971, p. 73). What SSK authors found particularly appealing about Kuhn's work is its call into question of the very Mertonian idea that underlying all scientific activity is a single scientific ethos [10]. By and large, Kuhn argues, scientists form a closed community that investigates a well-defined range of problems with methods and tools adapted for that task. The definition of the problems and the methodology of investigation derive from a professional tradition of theories, methods and skills the acquisition of which requires prolonged training and a great deal of indoctrination. The rules of the scientific method, as explicated by logicians of science, do not, according to Kuhn, adequately describe what scientists really do. In other words, Kuhn's underlying principle is that science should be treated as people, communities, not sentences (Diesing 1991, p. 62). In their daily work, scientists are guided by a paradigm that determines which questions can be asked and which are to be excluded, defines normas of conduct and criteria of evaluation. The younger scientists are socialized into it, just as the mature scientists uphold and transmit it to next generation; by adopting a paradigm one enters a community which, like any

other community, sensitizes its members to each other and desensitizes them to outsiders (11).

For Kuhn, the key to understanding scientific change is not the logical form of propositions but rather the shift in the broader 'paradigm' with which theories are invariably associated (Proctor 1991, p. 210). During a scientific revolution, which represents a disruption in what would otherwise be a situation of normal science, the scientific community embark in the search for a new paradigm in response to the exhaustion of the prevailing one. From a sociological point of view, the very assertion that revolutions regularly follow the exhaustion of paradigms and occur neither before nor after that point and that, moreover, revolutions are completely discontinuous and different from other types of change convert science in an essentially social process. The scientific community, like any other human community, is therefore just a particular kind of society, sociology being a very basic framework for understanding its dynamics (12).

III. Emergence of the sociology of scientific knowledge

SSK contributors conceives Kuhn's main insight as being that since the scientific community is simply a community - a society, with its own beliefs, customs, cerimonies, and rituals - one should use the same analytical framework to investigate the practices carried out in science as one would use to investigate the practices in any other social community (13). Scientific community, as any other society, is characterized by a system of collective beliefs; in science, this system of very collectivelly held beliefs is called *scientific knowledge* (Hands 1993, p. 157) (14). To distinguish between the sociology of science and the SSK, Collins (1983) argues that while the former focuses on "the elucidation of the set of normative and other institutional arrangements that enable science (...) to exist and function

efficiently" (p. 266), the latter is basically concerned with "what comes to count as scientific knowledge and how it comes so to count" (p. 267).

The outline that follows is by no means intended to be a complete one; it intends simply to point out some characteristics that are shared by most of the several branches of the SSK tradition and some that are rather particular to each of them. Most of the SSK authors can be divided into the following quasi-homogeneous groups, which, in turn, differ slightly on method, theoretical background, and theoretical focus of interest: ethnomethodologists (Lynch, Woolgar, et al); constructivists (Knorr, Latour, et al.); Edinburgh School (Barnes, Bloor, Shapin, Edge, et al.); empirical relativist or Bath School (Collins, Pinch, Pickering, et al.); and many others not easily classifiable such as Gilbert, Mulkay, Restivo, Chubin, Whitley, among others (15). Leaving aside their differences, the main point of agreement among these various groups seems to be their denial that scientific practices occur in a way that is truly consistent with traditional epistemological virtue, scientists rather adhering to their theories for reasons that are almost never the very reasons that traditional philosophy of science would call the cognitive status of the theories in question.

Another important feature of many of these authors's work is their familiarity not only with the organizational aspects of science but with its substantive theories and methods. Indeed, many of them had been trained as scientists and then became researchers in the sociology of knowledge. In some cases they not only knew and even participated with the working scientists in their field, but also did not hesitate, as invited as guest-researchers, to spend long periods of close observation of day-to-day work in scientific laboratories. Besides, it is worth of mention that another virtue of this tradition was the way in which it brought the philosophy, history, and sociology of

knowledge into the closest and most friutful interaction, sometimes in the work of single individuals, sometimes in the work of effective groups of colleagues.

Out of these various branches, the Edinburgh group or the so-called *strong program* can be considered the most cohesive one, its central claim being that all kind of knowledge is shaped by society and moreover is somehow about society, i.e., it has a social content. The very content of science should therefore be amenable to social study; to the extent that the scientific community is embedded in a larger social context, the latter determines the content of scientific knowledge. For the proponents of the so-called strong program the sociology of knowledge must therefore take over many of the traditional functions of epistemology: sociology is supposed to explain not only the context of scientific discoveries, but also their very content and nature. Moreover, the strong version of the sociology of knowledge is inherently anthropocentric, for as Bloor (1976) argued "men are not governed by their ideas or concepts", to which he added that "even in mathematics, that most cerebral of all şubjects, it is men who govern ideas, not ideas which control men" (p. 139). As Hands (1992, p. S) correctly observed, the strong program's view of scientific knowledge departs very radically from the traditional philosophical characterization of scientific knowledge. While for the traditional view it is the world out there that determines our scientific knowledge, for the strong program it is the particular interests present in a given social context that determine the beliefs that scientists hold, how scientific knowledge comes to be what it is being determined in turn by these beliefs. This view of science thus not only elevates the role played by 'the social' far beyond that which it has been allowed to play in traditional philosophy of science, but also well beyond the role it played in the earlier Mertonian sociology of science as well.

Bloor (1976, p. 5) holds that the strong program of the sociology of knowledge should adhere to four tenets, namely, causality, impartiality, symmetry and reflexivity. The first refers to the fact that it should be concerned with the conditions which bring about belief or states of knowledge, this involving, in turn, a commitment to a somewhat determinist conception of sociological explanation as a form of causal explanation. Bloor's second tenet demands that sociology of knowledge be impartial with respect to truth and falsity, rationality or irrationality, success or failure, and his third that it should be symmetrical in its style of explanation. Put shortly, the same types of cause would explain both true and false beliefs. Finally, Bloor's fourth tenet of the sociology of knowledge requires that sociology's patterns of explanation should be applicable to itself. Otherwise, as he pointed out, "sociology would be a standing refutation of its own theories" (p. 5).

Constructivist sociology of knowledge, in turn, focuses more upon microsocial elements, even though it is recognized that the wider social context in which sicentific activity takes place also matters. For constructivists, it is the more localized microsocial context of the laboratory and professional life within the discipline of science that is influential, rather than the wider social, political, and economic that are usually emphasized by the strong program; since reality is a human construct, all constructs have a social content. Put directly, the 'out-there-ness', i.e., the external world, is the very consequence of scientific activity rather its cause (Latour and Woolgar 1979, p. 182). To the extent that scientific knowledge is socially 'constructed' in the laboratory or other scientific context, it is this very socially conditioned character of scientific knowledge that should be focused on. That is because the phrase 'social construction of scientific facts' has become commonplace in the constructivist literature, particularly

since it was adopted as the subtitle of Latour and Woolgar's *Laboratory Life* (1979). Moreover, by the very reason that scientific activity is supposed to be influenced by a large variety of microsocial factors, contructivists studies tend to be carefully detailed, densely textured, and quite particularized narratives based upon the influence of these factors [16].

Epistemologically speaking, constructivist sociology of knowledge is more hermeneutic in its attempts to understand scientific knowledge and the social context that determines it, its method being more anthropological, more participant-observer. The study of the 'inscriptions' produced in the laboratory is thus not accidental, but rather the product of a very deliberate choice, namely, that of studying the tribe of scientists as if it were an ordinary social system, such as a tribe of hunters and gatherers or a fishing village; Latour and Woolgar (1979, p. 182), for instance, argues that scientific activity is just one social arena, and a laboratory just a system of 'literary inscriptions'.

These literary inscriptions are tables of figures, computer printout data sheets, curves drawn on graph paper, diagrams and - as the end result - written reports in which these inscriptions are employed in support of one or another set of claims and inferences. These inscriptions, in turn, are generated by 'inscriptions devices' or laboratory apparatus, whose main purpose is basically to transform pieces of matter into written documents. "More exactly", it is concluded, "an inscription device is any item of apparatus or particular configuration of such items which can transform a material substance into a figure or diagram" (Latour and Woolgar 1979, p. 51) [17].

It is this complex process of literary inscription that produces a number of claims or statements concerning material events and/or causal processes. In this process, the natural world is supposed to have a small or nonexistent role in the construction of scientific knowledge; since

laboratories are chockfull of artifacts, living as well inanimate, it is claimed that nowhere in them can be found the 'nature' or 'reality' which is so crucial to the descriptivists' interpretation of inquiry (Knorr-Cetina 1983, p. 125). Unlike several realist accounts of science, constructivists argue that true scientific statements do not denote an antecedently existing reality, for the very reason that reality and facts are equally socially constructed: "Argument between scientists transforms some statements into figments of one's imagination, and others into facts of nature (...) reality [is] the consequence of the settlement of a dispute rather than its cause" (Latour and Woolgar 1979, p. 236).

Knorr-Cetina (1981), in turn, also in clear opposition to a realist account of science, equally interprets the scientific activity as a decision-impregnated, contextually bound construction (p. 3); a scientific laboratory is by and large an occasion for the instrumental manufacture of knowledge. Accordingly, the products of scientific activity (knowledge claims, factual statements, verification, and so on) are contextually specific and therefore contingent constructions. Thus, not only what happens in the process of construction is relevant to the products we obtain, but also the products of science themselves have to be seen as highly internally structured through the process of production, independent of the question of their external structuring through some match ot mismatch with reality (Knorr-Cetina 1981, p. 5). This cognitive and social structuring of the products of science by processes and events internal to science implies, in turn, that such products result from selections, that is, decisions and negotiations regarding criteria that even meta-structure the laboratory framework (including the equipment, phenomena selected for analysis, research strategies, etc.). In her study, Knorr-Cetina pays considerable attention to written scientific documents and notes Latour and Woolgar's concept of literary inscription.

Moreover, she equally recognizes that written as well as spoken communication has very direct effects "in the concrete negotiations of the laboratory, in the bargaining which marks the highly selective construction and deconstruction of scientific findings and leads to continuous reconstruction of knowledge" (1981, p. 14).

By virtue of being more anthropological, more based on the sociologist-of-knowledge's coming to understand science by being immersed in its very context-laden practice, the constructivist approach is somewhat related to the ethnomethodological approach of Garfinkel (1967). The so-called ethnomethodologists are the most meticulous and detailed of all laboratory observers, and their stated aim is to rediscover the problem of social order in the details of scientific practice. As one can realize, several themes discussed within the constructivist tradition overlap with recent ethnomethodological studies of science such as Lynch, Livingston, and Garfinkel (1983), in which every object is seen as an 'icon of laboratory temporality', and Garfinkel, Lynch, and Livingston (1981), in which it is argued that even the celestial bodies are 'cultural bodies'. As Woolgar (1986) observed, the discourse analysis which he, Latour, Knorr-Cetina, and others practice is indebted to poststructuralism (in particular Foucault), which "is consistent with the position of the idealist wing of ethnomethodology that there is no reality independent of the words (texts, signs, documents, and so on) used to apprehend it. In other words, reality is constituted in and through discourse" (p. 312).

Finally, the central thesis of the empirical relativist approach is that empirical observations alone cannot conclusively account for a theoretical interpretation [18]. Since it is believed that there is no such thing as an independent reality, and that the entire world is a construction of a certain kind, one is led to conclude that there is no objective truth; and to the extent that there is

no such thing as an objective truth, then scientific activities are not (and in fact could not be) a quest for truth. Put another way, what really counts as truth is likely to change not only from place to place, but also form time to time (19).

IV. Political economy of scientific knowledge or sociology of economic knowledge ?

Hands (1992) carefully examines some quasi-economic arguments that have been put forward in the SSK literature. His conclusion is that these arguments 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. He then surveys some attempts to approach science from an explicitly economic perspective and discusses some of the philosophical difficulties that the so-called problem of the reflexivity raises for the SSK. Knorr-Cetina (1982), on the other hand, not only presents a critique of scientific communities as sociological constructs which appear to be largely irrelevat to scientific work, but also criticizes the prevailing quasi-economic models of such collectives for what appears to be a naive internalism and functionalism. In other words, he expresses some objections, to say the least, to the use of economic theory as a source of inspiration for the study of science. She also argues that the arenas of action within which scientific inquiry (laboratory) proceeds are transepistemics, that is, they in principle include scientists and non-scientists, and encompasses arguments and concerns of a 'technical' as well as a 'non-technical' nature.

In this final section I deal with some issues that implicitly emerged from these two contributions. In particular, I make some comments about the extent to which SSK tradition really must incorporate some economic concepts in its building of an account of scientific practices. For that end, I make a particular, reflexivity-based use of

Knorr-Cetina's argument regarding the very transepistemic nature of the arenas of action within which scientific investigation proceeds and argue that precisely because of them is that the incorporation of some economic concepts into the social studies of scientific knowledge must occur. In short, I would argue that such incorporation must take place precisely because of the very transepistemic nature of scientific practices; given the overdetermined nature of the relationship between these practices and the wider range of social domains, political economy can provide social studies of science with important contributions. This process of cross-fertilisation, in turn, constitutes itself a particular arena of negotiation.

Knorr-Cetina, however, is correct when she expresses some reservations about the explanatory power of approaches such as 'economics of science' or 'economics of scientific knowledge'; given the manufactured nature of knowledge, any approach intended to apprehend it must necessarily be transepistemic. On the other hand, I would argue that Knorr-Cetina throws the baby out with the dirty water when she uses this transepistemic-based characterisation of the arenas of action of scientists to present a critique of scientific communities as sociological constructs which appear to be largely irrelevant to scientific work. Put directly, I would sustain that the use of the notion of scientific community is not necessarily incompatible with the notion of a transepistemic arena of action within which scientific preactices take place, and this precisely because it is simply an analytical construct. Indeed, I would argue that it is reasonable to think of 'transepistemic scientific communities' in a non-internalist and non-funcionalist way.

Moreover, once the micro-macro relationship is viewed as a duality rather than a dichotomy, scientific communities as well as laboratories can be made complementar to each other. In other words, I would argue that a given social study of science is not either scientific-community-centered

or laboratory-based precisely because of the transepistemic nature of the arenas of action within which scientific practices occur. Perhaps more relativist light should be shed therefore into Knorr-Cetina's claims.

By simmetry (following one of Bloor's tenets for a strong sociological approach to knowledge), I would also argue that the so-called economic knowledge, itself a constructed/manufactured discipline, can benefit from an incorporation of some elements that have been emphasised by the SSK tradition precisely because of the transepistemic nature of the arenas of action within which economists circulate and produce knowledge. In particular, it is probably time for economists themselves try an ethnographic expedition to the domains of tribes of economists to study their beliefs and practices and, similarly, draw relativist conclusions with regard their beliefs, norms and goals. While some considers reflexivity as an honest but suicidal requirement (e.g. Bunge 1991), I would argue that a careful self-examination of the inscriptions produced in an 'economic laboratory' can contribute for an improvement in their heuristic value. Otherwise, economists will continue to produce truths that, as in the case of the men of science of Proust's narrative presented in 'epigraphy', interest only themselves.

Notes

(1) See Ben-David (1970) for a detailed historical account of how incipient sociology of science was in the 1920s and 1930s and, in particular, for the emergence of Merton's work as the only bridge between the pre- and postwar sociology of science. Ben-David (1982), in turn, shows how Merton's seminal work on the question of the ethos of science was a response to the perceived threats to science from fascism in the late 1930s.

(2) Some representatives of the Mertonian, Columbia school are the following: B. Barber, *Science and social order*, New York: Collier, 1962; J. Ben-David, *The scientists role in society*, New York: Prentice-Hall, 1971; J. and S. Cole,

Social signification in science, Chicago: The University of Chicago Press, 1973; J. Cole, *Fair science*, New York: The Free Press, 1979; J. Gaston (ed.), *Sociology of science*, San Francisco: Jossey-Bass, 1978; H. Zuckerman, *Scientific Elite*, New York: The Free Press, 1977.

(3) Ben-David (1978) provides an interesting sociological analysis of the main differences of approaches to the sociology of science in the United States and in Britain. In his view, the classic, Mertonian sociology of science remained limited to the United States because, for institutional reasons, those interested in the subject in Britain were interested in philosophical reflections of science, were generally critical of the structuralist-functionalist approach to sociology, and did not belong institutionally to departments of sociology. I would argue that such differences provide some reasons for the fact that SSK is an essentially European tradition, particularly a Britain one.

(4) As an indication of such influence, it is worth noting that when the *Society for Social Studies of Science* was founded in 1975, on the initiative of some of the newer members of the specialty, Merton was invited to be its first president (Diesing 1991, p. 149). On the other hand, Merton (1977, p. 105) not only referred to Kuhn's influential book as "merely brilliant", but also devoted thirty-eight pages of his 1977 memoir to Kuhn, compared to three pages for Popper.

(5) Even though he eventually dealt with several influences on seventeeth-century science, Merton was especially interested in showing that, contrary to Marxian and other materialist theories of the time, religion and values had their degree of independent influence in social systems. He showed that a disproportionate number of the new scientists were Puritan believers and that the Puritanism of the time consisted of a set of of values and religious beliefs that were favorable to scientific practice. Its most interesting sociological argument was that the values necessary for the rise of modern science, namely, the belief that understanding the laws of nature is a potential way to god, arose from certain Puritan way of life. Merton hence extended Max Weber's argument about the influence of the Protestant ethic on capitalism to the other modern development, namely, the enormous growth of science and technology (Barber 1990, p. S-9 ; Ben-David 1970, p. 419).

(6) Merton (1957) provides an admiring exposition and critique of Mannheim's work. His admiration, on the one hand, was for the sociological elements in Mannheim's analysis, his attempts to show how certain social or cultural variables, e.g., ideas about history, were related to other social or cultural variables, e.g., the particular

generation to which the holders of these ideas belonged; his critique, on the other hand, was for the philosophical confusion in Mannheim, the inability or unwillingness to establish some firm rational rules for justifying the validity of scientific knowledge. In his words: "Mannheim has sketched the broad contours of the sociology of knowledge with remarkable skill and insight. Shorn of their epistemological impedimenta, with their concepts modified by the lessons of further empirical inquiry and with occasional logical inconsistencies eliminated, Mannheim's procedures and substantive findings clarify relations between knowledge and social structure which have hitherto remained obscure" (p. 508).

(7) Stressing the differences between their framework, Merton (1976, p. 126-29) noted that his functionalism combines Marx and Durkheim, while his teacher Parsons's functionalism is straight Durkheim. See Diesing (1991, p. 151-54) for a detailed account of the major diffences between their functionalist framework. From the perspective of an ex-student of Parsons and Merton, Barber (1990, ch. 2) provides a detailed account of Parson's contributions to the sociology of Knowledge.

(8) Revealing his clearly Weberian heritage on this issue, Merton (1973, p. 254) approvingly quotes Weber's statement that "the belief in the value of science is not derived from nature but is a product of definite cultures". Merton further developed this insight and defined scientific ethos as an "emotionally toned complex of rules, prescriptions, mores, beliefs, values, and pressupositions which are held to be binding" upon a scientific community (1976, p. 258).

(9) Such a tracing of the essential intellectual influences suffered by the SSK tradition back to Durkheim and Mannheim, rather than to the Mertonian tradition, actually a missing chapter in Hands' story (Hands 1992), should be made precise. I argue that, however important the Mertonian tradition might have been for the institutionalization and professionalization of sociology of science, much of the SSK literature is clearly a reaction to the somewhat idealist Mertonian functionalism. Though it goes beyong the scope of this paper to speculate about what would the SSK tradition look like without the Mertonian one, one can conceive the former as somewhat shedding Durkheimian and Mannheimian light into the latter. Bloor (1976, p. 5), for example, explicitly mentioned that the major tenets to which the sociology of scientific knowledge should adhere are by no means new, but represent an amalgam of the more optimistic and scientistic strains to be found in Durkheim, Mannheim and Znaniecki.

(10) Though criticized only implicitly in Kuhn (1962), the Merton's concept of scientific ethos was explicitly

challenged in Kuhn (1972). Put somewhat crudely, Kuhn (1972) used his notion of paradigm to critique Ben-David's notion of scientific role, which was based upon Merton's notion of scientific ethos, by arguing that the notion of scientific role should be conceived as the cognitive and technical contents of what the incumbent of the role knows and does, so that when those contents change, the role also changes. As this implies that the notion of a single scientific role and of a scientific ethos shared by all scientists are meaningless, it is understandable why it is so appealing to SSK literature.

(11) In Kuhn's words: "The study of paradigms (...) is what mainly prepares the student for membership in the particular scientific community with which he will later practice. Because he then joins men who learned the bases of their field from the same concrete models, his subsequent practice will seldom invoke overt disagreement over fundamentals. Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus are prerequisites for normal science, i.e. for the genesis and continuation of a particular research tradition (1970, p. 10-1).

(12) Ben-David (1978, p. 441-2) provides a very highlighting account of the different receptions to Kuhn's formulation by American and British sociologists of science. The former paid more attention to Kuhn's ideas about the developmental phases of scientific knowledge, in particular because these had, for a Mertonian audience, the most obvious sociological contents and promised to be capable of empirical verification. The British sociologists of science, as a multidisciplinary group, instead of viewing the ideas of Kuhn and Merton as an attempt at conceptualizing the complexities of the scientific community, to be used as input in an effort to unravel the structure and function of that community by piecemeal empirical research, analyzed these ideas philosophically, for their internal consistency and their logical compatibility. By approaching these ideas from a philosophical point of view, the British sociologists of science paid particular attention to the difference between Kuhn's qualified relativism and Merton's emphasis on relative stable institutionalized norms of scientific behaviour.

(13) Another development within sociology of science before the 1960s was the conceptualization of the informal social system of science by Michael Polanyi. Polanyi began his professional career as physical chemist in 1917 and continued to work as an exact scientist until 1948, when he retired from scientific endeavors to write about how the scientist actually thinks and works. I intend to avoid any *whiggism*, but it is worth noting that some Mertonian authors argue that it was Polanyi (1951) who first formulated the

term 'scientific community' and used it as a description of the way scientists enforced strict discipline (e.g., Ben-David 1970, p. 41S). Among others things, Polanyi (1962) noted that the community of scientists is organized in a way which resembles certain features of a body politic that works according to economic principles. Put directly, the activities of scientists are in fact coordinated by an 'invisible hand' towards the joint discovery of a hidden system of things, this process being similar to the self-coordination achieved by producers and consumers operating in a market.

(14) The term *sociology of knowledge* was coined in 1921 by Max Scheler. Surprising perhaps to today's eyes is the fact that Scheler saw the sociology of knowledge growing out of positivism. Traditional epistemology in his view had largely ignored the most interesting questions - only the positive philosophy of Comte and Spencer had attempt to combine a 'pure sociology of knowledge' with sociological statics and dynamics (Proctor 1991, p. 214).

(15) Knorr-Cetina and Mulkay (1983), an excelent survey of the field as a whole, distinguishes eight branches including the weak program, the mild program, and discourse analysis in addition to the above. More recently, McMullin (1992) and Pickering (1992) volumes also contain several papers discussing the similarities and differences among these various approches. I should mention that this paper focuses basically on the first four, and among these, particularly on the Edinburgh and constructivist ones.

(16) It is worth of mention that Bloor's strong program can be also conceived as a 'weak constructivism'. In his view, negotiations create meanings, and "the boundaries and content of our concepts are no more discovered than are the boundaries of our countries or the content of our institutions", all of them being created and negotiated (1976, p. 295).

(17) Employing the concept of literary inscription, Latour and Woolgar draw together a number of activities in the laboratory: "It seemed that there might be an essential similarity between the inscription capabilities of apparatus, the manic passion fo marking, coding, and filing, and the literary skills of writing, persuasion, and discussion. Thus, the observer could even make sense of such obscure activities as a technician grinding the brain of rats, by realizing that the eventual end product of such activity might be a highly valued diagram (...) For the observer, then, the laboratory began to take on the appearence of a system of literary inscription" (1979, p. 51-2).

(18) In truth, it is fair to say that the approaches to SSK discussed above also rests on some form (and some degree) of relativism. Bloor (1976), for instance, admitted that "there is no denying that the strong program (...) rests on a form of relativism" (p. 142), and that if we can live with moral relativism, we can live also with cognitive relativism: "There need be no such thing as truth, other than conjectural, relative truth, any more than there need be absolute moral standards rather than locally accepted ones" (p. 143).

(19) Gieryn (1982) denounces what he considers to be three redundancies in relativist/constructivist discourse, namely, the notions that scientific knowledge is only approximate; social and cultural factors are essential in the generation of knowledge; and what is known constraints what can or will be known. In his view, the first two are consistent with the Mertonian approach, and the third is easily arrived at through the Mertonian approach. I leave to the reader to decide, based upon the survey of these approaches presented above, about how correct is Gieryn's denouncement.

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