CHAPTER 2

SOCIAL CONDITIONING OF ECONOMICS

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SOCIAL CONDITIONING OF SCIENCE

Contemporary sociology of science usually takes it for granted that science is, all down the line, social in character, and that fatal consequences follow from this in regard to traditional philosophical views of the nature of scientific knowledge and practice. There are a number of different accounts of science being social. All such accounts appear to be more or less unclear as to what the social character of science precisely amounts to. In what follows, some features of a few recent accounts will be surveyed, and some of their critical implications will be pointed out in regard to other conceptions of science, Popperian methodology in particular. Preliminary attempts will also be made to point out some of the ways that science is being viewed as social or socially conditioned (or socially constructed, socially shaped, socially constituted), as implied in those accounts. Towards the end of the chapter, the intriguing issue of relativism will be briefly addressed. The major part of the current sociology of science focuses on the natural sciences; the social sciences have received much less attention. In the course of the following survey, economics and economic methodology will be kept in mind.

I propose to use the formulation "science is socially conditioned" as an umbrella expression that should be taken as intentionally neutral in regard to the precise character of the relation between science and "the social." As such, the formulation is multiply ambiguous. Three clarifying questions have to be answered. First, what is there in science that is so conditioned? Second, what is it that does the conditioning? In other words, what is the character and range of the social? Third, what is the relation or process of conditioning like? Each of these three elements in the key statement can be specified in a number of different ways.

Without attempting to be exhaustive, I will show that, among others, the following three alleged kinds of social conditioning are implied in some recent sociologies of science. Each of them involves implicit specifications of the above three elements.

1. The content of accepted theory or belief (or its metaphysical and epistemological presuppositions) is caused (in an unspecified way) by social factors (such as cultures or interests internal or external to science). Here, a social fact causally generates an aspect of science (namely, scientific knowledge).

2. The goals of scientists' actions are social states or processes (such as credibility or power and their growth). Here a social fact constitutes an aspect of science (namely, scientists' goals).

3. The process of the justification of scientific claims is a social process of negotiation and rhetorical persuasion. Again, a social fact constitutes an aspect of science (namely, the process of justification).

Of these, (1.) has been endorsed by the so-called strong program of the Edinburgh School, while (2.) and (3.) have been more emphatically studied by the so-called ethnographic and constructivist approaches, and elsewhere. While there are incompatibilities between various approaches and research techniques relating to theses (1.)-(3.), the theses as such seem to be mutually compatible, suitably interpreted. On the other hand, it would be much more implausible to argue that theses (1.)-(3.) are compatible with the Popperian norms of science, for example.

THE STRONG PROGRAM

Much of the earlier sociology of science was preoccupied largely with the institutional organization of sciences, its changes and its relationship to the growth and direction of research. Unlike these streams within the field, the primary aim of the "strong program" is to attempt to provide sociological explanations of the propositional contents of beliefs or theories held by scientists. This pursuit is something that the strong program shares with such classics in the original field as Marx, Durkhem, Mannheim, and Sohn-Rethel. Besides the Edinburgh core, consisting of Barry Barnes, David Bloor, and Steven Shapin, there are other adherents such as Harry Collins, Donald MacKenzie, Andrew Pickering, and Trevor Pinch. They do not constitute a homogeneous group. For example, Barnes, inspired by Habermas, tends to cite social interests as explanantia, while Bloor, more in a Durkheimian fashion, puts stress on the culture of science. While most, if not all, would characterize themselves as relativists, Collins, for instance, seems to be a more radical relativist than Barnes or Bloor. (See, Barnes 1974, 1977, 1982; Bloor, 1976, 1983; Collins, 1983; for a survey of empirical studies, see Shapin, 1982.)

If, within the socially conditioned entity, an analytic distinction is drawn between scientific knowledge and scientists' actions, it can be seen that the strong program is primarily—although not exclusively—interested in explaining the former. Thus, the sociology of science endorsed by the Edinburgh School is a sociology of scientific knowledge, or belief in a strict sense.

From the point of view of the standard methodology of economics, it is noteworthy that the strong program, at least in its most representative formulations (such as in Bloor, 1976), is directed against a philosophical understanding of science. In this opinion, philosophers, both in general epistemology and in the philosophy of science, have monopolized the study of rational production of knowledge, while leaving the irrational residuum in scientists' behavior to sociologists and psychologists. It is claimed to be characteristic of these philosophical approaches that they are hopelessly unempirical, that is, unscientific. Unlike the sociological approach, they do not aim at empirical accounts for scientific beliefs. The sociological approach proceeds from the premise that knowledge, scientific knowledge included, is a social phenomenon and should be studied (described, explained) just as other social phenomena are studied by sociologists.

Bloor's (1976, 4-5) four tenets for the sociology of scientific knowledge define a version of the Edinburgh approach or the strong program:

1. The principle of causality: "It would be causal, that is, concerned with the conditions which bring about beliefs or states of knowledge."

2. The principle of impartiality: "It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation."

3. The principle of symmetry: "It would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs."

4. The principle of reflexivity: "It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself."

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 program is strongly anti-hermeneutic.

Bloor (ibid., 5-10) is critical of the idea that there can only be a sociology of error (falsity, irrationality) and that rational pursuit of truth is self-explanatory, in need of no further explanation—that only errors would be caused, while true beliefs would have no causes. This exemplifies his distinction between the causal and "teleological" approaches to knowledge. What Bloor regards as the dubious teleological approach takes true knowledge as an end product of a natural course of the rational process of human reasoning, as an embodiment of goal-directed activity with no causal history, while irrational and false beliefs are deviations that can be causally explained. This is the sort of asymmetry that is rejected by the strong program.[1] As Bloor puts it, the strong program, following the example of all scientific approach, is claimed to be "morally" neutral in regard to the epistemic qualities of human beliefs, whereas the teleological view is unscientific in giving rational and true beliefs a morally privileged position. These ideas are expressed in tenets (2.) and (3.), which are most widely accepted among sociologists of science. (For critical discussions of Bloor's tenets, see Laudan, 1981, Bloor, 1981, Collins, 1981a, Hesse, 1980, Newton-Smith, 1981.)

As pointed out above, the Edinburgh School is concerned with the sociology of scientific knowledge rather than the sociology of scientists' actions. The concept of knowledge needs to be roughly specified in this context. According to the standard philosophical definition, knowledge is rationally justified *true belief*. Bloor's specification of what he means by "knowledge" differs from the standard notion. Hence, it is worthwhile to cite him at length: "Instead of defining it as true belief, knowledge for the sociologist is whatever men take to be knowledge. It consists of those beliefs which men confidently hold and live by. In particular the sociologist will be concerned with beliefs which are taken for granted or institutionalized, or invested with authority by groups of men. Of course knowledge must be distinguished from mere belief. This can be done by reserving the word "knowledge" for what is collectively endorsed, leaving the individual and idiosyncratic to count as mere belief." (Bloor, 1976, 2-3; cf. Barnes, 1977, 1) Thus, beliefs become knowledge as individually held ideas receive collective support. Note that no reference to truth is presupposed by this notion and that the idea of rational justification is replaced by unspecified collective endorsement. Such a consensus theory of knowledge makes the very phenomenon of knowledge socially conditioned in the sense of being constituted by attitudes held by a collectivity of individuals.

There is a further sense in which scientific knowledge is claimed to be socially conditioned. Bloor (ibid., 44-47) puts forth the Durkheimian idea that the way we typically conceive of the nature of scientific knowledge is a reflection of the way we see the structure of society. The latter perception gets an expression in various social ideologies, religion included. Theories of knowledge typically "depend on social images and metaphors" (47). In particular, our experience of the structures of social authority and power provide the familiar framework on which conceptions of knowledge can be modelled. This, Bloor thinks, helps to account for the sacred character that is attributed to branches of scientific knowledge. Here, it is not the content of scientific knowledge-claims or beliefs "directly" that are allegedly socially conditioned, but rather the very general forms of belief and principles of justification that somehow "reflect" social structures external to science.

Typically, the factors cited in the applications of the Edinburgh approach as causes of scientific knowledge are "macrosocial" factors external to science. The program is largely macrosociological or even holistic: explanations are framed in terms of the impact on scientists' theories of systems of social relations, of cultural values, of social power and communal interest, of political ideologies.[2] It is implied in the agenda of a typical application of the Edinburgh program that, for example, when scientists cite compelling evidence as the reason for their theoretical beliefs, this is, to say the least, superficial or perhaps indicative of ideological concealment because, in fact, their beliefs have been shaped by cognitive interests that are causally dependent on social interests that, in turn, are determined by social structures.

Interests ascribed to communities or institutions are viewed as theoretical entities in the same way as are forces, fields, and molecular structures, and they are cited as causal conditions of beliefs. No modally deterministic connection is claimed to prevail here by a major representative of this line of thought, just a "temporal co-variation of beliefs, interests and social structure" with the specification that "it is claimed that interests inspire the construction of knowledge out of available cultural resources in ways which are specific to particular times and situations and their overall social and cultural contexts. As for the relationship of interests and social structure, it is accepted that some interests are indeed structurally generated." (Barnes, 1977, 58; see also Barnes and MacKenzie, 1979)

According to Barnes, interests fall into two categories, "an overt interest in prediction, manipulation and control, and a covert interest in rationalization and persuasion." (ibid., 38) When the latter is in operation, the resulting idea or belief is "ideologically determined." In Barnes's definition, "[k]nowledge or culture is ideologically determined in so far as it is created, accepted or sustained by concealed, unacknowledged, illegitimate interests." (ibid., 33)

There are a number of conceptual and methodological issues involved in the interest approach. They relate, among other things, to the precise meaning(s) of the very notion of interest; to whether some interests (such as the instrumental interest in prediction and control) have a transcendental status or whether all interests are socially contingent and open to rival interpretations; to whether interests are nothing but artifacts of the sociologist of science; to the role of interests in the discourse of the scientists studied by sociologists; to the kind of link allegedly connecting interests and beliefs; etc. (For discussions of some of the ambiguities and other problems inherent in the interest approach to scientific knowledge, see Yearley, 1982, Woolgar, 1981, Barnes, 1981, MacKenzie, 1981.)

Marx's attempt to give an account of what he called the "vulgarization" of economics after the classical period might be viewed as an early and crude application of something like the strong program. Its main ideas can be reconstructed briefly as follows. Commodity fetishism as a reflection of a powerful structural feature of the capitalist economy generates a tendency to the origination and establishment of "vulgar" economic theories (which depict market exchange as fundamental to the economy), while this tendency or potency is actualized by changed relations in class struggle external to science (because "vulgar" theories serve the apologetic and threatened interests of the bourgeois class). It is part of that story that the vulgar and apologetic character of those theories was concealed, thus making them ideologically conditioned. Despite some affinities, the Edinburgh School is not straightforwardly Marxist, however. While a Marxist would pursue explanations of at least some important scientific ideas in terms of reference to the fundamental or "base" structure of society, the social facts that applications of the Edinburgh program cite as potential causes of scientific beliefs are typically such matters as rank and status in an educational system or religious affiliation of those involved in the scientific enterprise.

A famous (and controversial) example that shares the general approach of the strong program is Paul Forman's attempt to show that the reason why the German physicists of the Weimar Republic embraced a noncausal, indeterministic view of physical processes had little to do with the experimental and theoretical demands of their scientific inquiries, but that it was rather an expression of the *Zeitgeist*. The social environment was impregnated with romanticism, the general atmosphere was mystical and antirational. The public sentiments were hostile to science and technology, the essential ingredient of which was believed to be the principle of causality. To regain the public approbation and prestige they used to have, the physicists as members of the German academic community dispensed with the principle of causality. All this happened, Forman argues, before the advent of a fundamentally acausal quantum mechanics (Forman, 1971).

I do not know of any such explicit applications of the strong program to economics, but it is clear that an Edinburghian explanation of the reign of the neoclassical orthodoxy would not refer to its having survived severe attempts at falsification, nor to its coming closest of all alternative schools of economic thought to formulating theories in a falsifiable form, nor to its constituting a progressive research program in the Lakatosian sense. Instead, such an explanation might argue that "in some way, and for some as yet unexplained reason, the neoclassical ideology is a part of—or, indeed, the best representative of—Anglo-American ideology." (Burkhardt & Canterbery 1986, 245)

For instance, the individualist commitment in neoclassical theory would be accounted for as a reflection of general ideological individualism in Anglo-American society. One possible way of specifying this is to use the notion of legitimation and to try to show that in order to legitimate themselves as a profession, orthodox economists have to legitimate the prevailing social ideology by giving it a theoretical formulation that is put beyond doubt (ibid., 231-237).

Typically, the precise nature of the allegedly causal connection between social facts and scientific knowledge remains unspecified in the formulations and applications of the strong program. For instance, Bloor claims that "the impact of practical developments in water and steam technology on the content of theories in thermodynamics has been studied in great detail. The causal link is beyond dispute." (Bloor, 1976, 3) However, the question of the exact nature of the causal link remains unsettled here and elsewhere. The problem is also present in those suggestions that connect the popularity or wide acceptance of the standard economic assumptions about the features of individual actors with the established Western ideas of the nature of individuals. As Barnes (1982, 103) admits, "we lack a precise and detailed account of the relationship between goals and interests on the one hand, and concepts and beliefs on the other." At a general level, one radical position is ruled out, though: "no laws or necessary connections are proposed to link knowledge and the social order" (Barnes, 1977, 85).

It may well be that there is some sort of loose *correlation* or *analogy* between some social facts and some scientific theories that the protagonists of the Edinburgh program see as being related to one another. It is, however, much more ambitious and demanding to argue that social interests and structures can causally generate the contents of theories and the involved metaphysical presuppositions held by scientists. Correlation and analogy do not imply causation, nor are they obvious instances of what I have called conditioning. A successful argument for the existence of a genuinely causal relation would have to indicate the existence and functioning of a mediating mechanism that would make it possible for the cause to produce the effect. Bloor, Barnes, and others have been unable to theorize such a causal mechanism. To say the least, their suggestion remains incomplete until a satisfactory account of the causal intermediaries is provided.

The so-called ethnographic approach, and some other recently practiced microsociological approaches, might be taken as complementing the Edinburgh program precisely where the latter encounters some of its worst problems.

ETHNOGRAPHIC AND RELATED APPROACHES

What are often called the ethnographic (or, from a different perspective, constructivist) approaches to studying science provide specific meanings to the statement "science is socially conditioned." Among some of the major representatives are Karin Knorr-Cetina, Bruno Latour, Steve Woolgar, Michael Mulkay, Nigel Gilbert, and Michael Lynch. Four differences between the Edinburgh program and the ethnographic approaches may be sorted out as an introduction to the latter. Note that neither approach is internally homogenous and that the dividing lines between them are not always very sharp.

First, if we draw a (nonabsolute) distinction between internal and external in regard to science, we may say that the scope of the Edinburgh approach encompasses social factors external to science, while the ethnographic analyses put stress on internal social factors. This is accompanied by a difference between more macrosociologically oriented and more microsociologically oriented perspectives.

Second, the Edinburgh program pursues causally explanatory accounts of science, whereas the ethnographic analyses and related (such as ethnomethodological) approaches are more intent on descriptive interpretations of the life processes of scientists and their communities.

Third, applications of the Edinburgh program have typically consisted of sociological reconstructions of historical materials related to past episodes in the development of science, while the ethnographic approaches support concentrated attempts at detailed observation of the situated day-to-day practices of contemporary scientific communities. Not the literary residues, but rather the material and social process of their production as actually observed, is the source of data and primary locus of analysis.

Fourth, we say that the strong program of the Edinburgh school views scientists primarily as holders of beliefs or knowledge-claims. We shall next see that the ethnographic approaches switch the focus of emphasis more to scientists' actions, their everyday practices in their ordinary settings. The title of Latour and Woolgar's *Laboratory Life* is revealing in this respect. This is not to say that concern with belief and knowledge has been excluded, but that it is now given a secondary, derived role; hence, I suppose it would be right to put the stress on "manufacture" in the title of Knorr-Cetina's book, *The Manufacture of Knowledge*.

The ethnographic and constructivist approaches do not postulate anything akin to a set of autonomous social structures or interests external to the scientific community to explain scientists' beliefs and actions. The acting subject with his or her intentions, beliefs, and actions in social settings is the starting point of analysis. It is the scientists with routinized recurrent actions and interactions based on tacit principles who produce, reproduce, and alter the social order of the life-worlds of science. The basic activity is that of the construction of scientific facts and theories in such local and artificial contexts as the laboratory. (For examples of the so-called laboratory studies, see Latour & Woolgar, 1986 (1979), Knorr-Cetina, 1981, Lynch, 1985.) Facts and theories in science are viewed as conventional "fabrications" based on selections, interpretations, and negotiations within research groups. The process of negotiation involves factors like rhetorical persuasion and use of power. Science may be depicted as a game with players using different strategies and tactics for the purpose of maximizing a specific social attribute of individual scientists (or groups thereof), for instance what Latour and Woolgar (ibid.) call "credibility." Both the laboratory and the scientific community at large are viewed in quasi-economic terms as markets in which actors attempt to sell their products, "papers," in order to maximize their own credibility. (For a useful brief account of an ethnographic-constructivist approach, see Knorr-Cetina, 1983.)

It should come as no surprise that the picture provided by the ethnographic studies of laboratory life is radically nonfalsificationist. The laboratory is not an arena of bold conjectures and attempted falsifications organized on the basis of compelling arguments designed in terms of formal logic with the aim of seeking and substantiating the truth or eliminating falsehoods. Rather, the aim of laboratory operations is to "make things work," that is, to achieve a pragmatically satisfactory balance between chemicals, instruments, statistical and other procedures, human participants, and other elements in the complex institution of the laboratory. The thrust of research work is not in the least to test theories. The suggestion that economics is not much different in this regard should not be found startling.

Participant observation is the approach that the ethnographic studies have adopted. Woolgar (1981, 482) complains that most of the social studies of science, even those that are concerned with the contents of scientific claims, rely on such secondary sources as interviews with scientists and published scientific papers. For the most part, this also applies to some recent attempts to find out what actually goes on in economics; they are based on interviews or questionnaires (Klamer, 1983; Colander and Klamer, 1987) or on the published or unpublished work of economists (McCloskey, 1985). Woolgar recommends instead the *in situ* observation of scientific activity, in which the sociological analyst adopts the role of a participant observer. This is what has been practiced in the ethnographic laboratory studies. They are fashioned after the image of anthropological studies of alien tribes. It is claimed that such *in situ* observation provides analysts with more direct access to the actual processes of scientific practice than do interview responses or written documents. As Woolgar (ibid., 483-484) puts it, "more is to be gained from being on the spot than from attempting interpretation from a secondary perspective," such as from actors removed from the scene.

There is no doubt that Axel Leijonhufvud's entertaining piece, "Life among the Econ" (1981 (1973)) is based on observations stemming from being on the spot. This amusing narrative also emulates a kind of ethnographic or anthropological approach with references to the Econ "tribe's" network of "caste and status," "totems and myths," "taboos against association with the Polscis, Sociogs, and other tribes." The status pursued by the members of castes ("fields") is dependent on the manufacture of certain kinds of sacred implements, namely "modls" (ibid., 349). "Each caste has a basic modl of simple pattern and the modls made by individual members will be variations on the theme set by the basic modl of the caste." The basic modl is the "totem" of the caste; for instance, the totem of the "Micro-Econ" consists of intersecting S- and D-lines, while the totem of the "Macro-Econ" consists of intersecting LM- and IS-lines, where both resemble "two carved sticks joined together in the middle somewhat in the form of a pair of scissors" (ibid., 351-353). What is important for our purposes is the claim that modls are in present times produced "more for ceremonial than for practical purposes." This trend among the Econ towards "more ornate, ceremonial modls" is related to the rise of the "Math-Econ" to the "priestly caste" that make "exquisite modls finely carved from bones of walras" (ibid., 349-350, 355). When the ceremony fails to produce concrete results in "prospecting," the Econ adopt strategies through the use of which "the role of the totem in the belief-system of the caste remains unassailed" (ibid., 354), a very non-Popperian feature of the tribe, to be sure.

Most of those who have written on the "culture" of economics (Leijonhufvud, 1981 (1973); Ward, 1972; Earl, 1983) are themselves economists and, hence, in a genuine sense participant observers. There are, however, at least three differences between these attempts and those of the ethnographic sociologies of science. First, these economists do not enter the culture they study as aliens, as do those sociologists who have studied, for instance, the day-to-day practices of biochemists. Second, these economists have not themselves engaged in highly systematic studies based on data collection and empirically controlled theory formation. They have come up with something akin to theories, but these accounts are often more intuitive than in the case of the ethnographic studies. Third, unlike the studies written by economists about their own discipline, the ethnographic approach has produced accounts that reveal the technical or craft character of science by portraying the scientist located at the laboratory bench. Similar studies of economics should provide analyses of what takes place, for example, at economists' desks. Concentration on classrooms, conference sessions, and journal publications is insufficient from this perspective.

SCIENCE AS A RHETORICAL GAME

Much of the recent sociology of science puts considerable emphasis on the linguistic behavior of scientists and its social character. This is well exemplified by Latour and Woolgar's study (1986 (1979)). To them, scientific work is a form of writing, it is production of what they call "literary inscriptions," such as computer printout data sheets, tables of figures, curves and diagrams, and, as the final product, written reports. Science is based on the rhetorical use of language in social contexts, it is "the organization of persuasion through literary inscription" (ibid., 88).

One type of epistemological foundationalism amounts to the doctrine that takes the structure of justification of beliefs as being built upon a set of basic beliefs that are somehow given or warranted but not socially produced—for instance, pure observation reports. The social theories of knowledge production, such as Latour and

Woolgar's and Knorr-Cetina's, reject such foundationalism. They argue that the basic factual representations, or "data," in laboratory research are socially constructed through a process of selection, interpretation, and negotiation. Instead of being given, the data belong to the set of negotiable literary inscriptions.

The winning of the status of a generally accepted representation is a peculiar social process of persuasion whereby the traces of the process itself are hidden. "The function of literary inscription is the successful persuasion of readers, but the readers are only fully convinced when all sources of persuasion seem to have disappeared. In other words, the various operations of writing and reading which sustain an argument are seen by participants to be largely irrelevant to 'facts,' which emerge solely by virtue of these same operations." (ibid., 76)

Latour and Woolgar (ibid., 76-86) claim to have observed a process of gradual transformation of statements from hotly contested conjectures to self-evident facts. They give a fivefold classification of statement types. Type 5 statements correspond to taken-for-granted facts that do not figure in discussions among established members of the laboratory. Type 4 statements are also uncontroversial, but the relation stated is made explicit here. They are typical of teaching texts. Type 3 statements are like type 4 statements with modalities, that is, statements of other statements, such as including a reference to the scientists who discovered the relation stated and the date when the relation was reported to have been found. Type 2 statements are claims rather than uncontroversially established assertions. They contain modalities which "draw attention to the generality of available evidence (or lack of it)." Finally, type 1 statements are conjectures or pure speculations "which appear most commonly at the end of papers, or in private discussion."

From this perspective, science appears as a rhetorical game or struggle with the aim of the participants being to persuade their colleagues to drop all modalities involved in their favorite statements. The aim is to transform as many statements as possible to the status of type 4 statements. In the process of increasing "facticity," or of the establishment of statements as self-evident, all traces of authorship and the rhetorical background disappear. This is why the outcome is dependent on "hiding" the process of its origination.

This insight easily leads to what may be called social coherence theories of justification. Scientific claims become justified when related to other statements that enjoy strong support in a scientific community as a result of socially conditioned processes of selection, persuasion, and negotiation. That is, if there are any "foundational" or "basic" beliefs in science, they are not given, incorrigible, or in any absolute sense privileged; what makes them more basic than other beliefs is the social fact of having won plenty of communal support.

It is then a most natural thing to suggest that these social selection mechanisms should be sociologically investigated. What has been found in such studies is a plethora of various factors, mechanisms, and processes that work towards support-formation, such as persuasive skills, authority and power, tenacity of tradition, use of culturally rooted metaphors, and other such socially loaded facts. They contribute to "closing down" controversies that otherwise would continue without limits due to the unlimited interpretive flexibility of data (Collins, 1981b). What emerges is a picture of scientific justification as a social process that is irreducible to the falsificationist process of deductive reasoning. On this view, the mechanisms of closure are social, not logical.[3]

Still, scientists themselves often appeal to metatheoretical notions such as Popperian falsificationism in giving accounts of their research practices. Having studied the way a group of biochemists construe accounts of actual procedures of theory choice, Michael Mulkay and Nigel Gilbert report that there is a systematic symmetry between how these scientists account for what they regard as correct beliefs and incorrect beliefs. Correct beliefs (usually those of the interpreter and of those with whom the interpreter agrees) are interpreted as being objective and guided by scientifically reliable experimental evidence, while incorrect ones (usually held by those with whom the interpreter disagrees) are viewed as having been influenced by the intrusion of distorting social factors into the scientific domain. (Mulkay and Gilbert, 1982a, 1982b; Gilbert and Mulkay, 1984.)

Mulkay and Gilbert (1981) show that Popperian rules of science are often appealed to in such accounts since conclusions with which the accountant agrees are portrayed as results of actions that obey those rules, while those with which the accountant disagrees are claimed to contravene them. Because the Popperian norms are extremely abstract, they can be, and in fact are, flexibly interpreted to support scientists' varying objectives in different social situations. Mulkay and Gilbert argue that almost any action, belief, or judgment can be made compatible with loose Popperian ideas and that this opportunity is in fact used by scientists. Of course, such discourse by scientists does not yet suffice to establish the idea that good scientific research follows Popperian canons. Not surprisingly, Mulkay and Gilbert argue that scientists' accounts of their own practice should not be accepted at face value. Instead, they appear to be extremely unreliable in that they are incoherent, diverse, variable, and contingent upon contexts. They should be viewed as part of the metatheoretical rhetoric or discourse that scientists practice in persuading others to accept their views and to reject those of their opponents. It is regarded as a task of a sociologist of science to analyze such discourse for what it is.

SCIENTISTS' ACTION AS A PURSUIT OF SOCIAL ENDS IN A MARKET

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.

The ethnographic studies view the laboratory as a local site of a production process that yields published research reports as final outputs. Formal publications are products of complicated social processes with informal interactions and flows of information involved within a community of research workers. Both the laboratory and the scientific community at large are considered in quasi-economic terms as markets in which participants do their best to market their products, that is, "papers," and in that way to maximize an asset, namely what Pierre Bourdieu (1975) calls "credit" and "symbolic capital," and what Latour and Woolgar (1986 (1979)) call "credibility."

Scientific credit in Bourdieu's sense is symbolic capital that consists of both scientific competence and social authority and that can be converted into various kinds of resources needed for carrying on scientific production. Credit is pursued by scientists in an exclusively rivalrous manner in the market of science by using an investment strategy that would bring them a monopoly of authority in a given field of research, "defined inseparably as technical capacity and social power" (Bourdieu 1975, 19).

The notion of credibility as developed by Latour and Woolgar is indebted to Bourdieu's suggestions, although they are critical of the latter's idea of credit for failing to provide an account of "the way in which technical capacity is linked to social power" (Latour and Woolgar 1986 (1979), 206). Credibility is more than just reward for past achievements. It also refers to future capabilities of scientists to practice science with success. Credibility is a resource that can be cashed in. Scientists are maximizers who invest their energies in the research fields and topics that are anticipated to yield the best return, that is, in those for which there is demand in the "market." The credibility they acquire by doing so leads to new rewards such as research grants, appointments, accepted publications, and so on. These, in turn, generate more credibility. As Latour and Woolgar (ibid., 197) put it, "scientists' behavior is remarkably similar to that of an investor of capital. An accumulation of credibility is prerequisite to investment. The greater this stockpile, the more able the investor is to reap substantial returns and thus to add further to his growing capital." The ensuing process of what they call the "cycle of credibility" constitutes the ultimate dynamics of science.[4]

The point of emphasis in the credibility model is on scientists' action (although, to be sure, there are hints of some kind of systemic teleology in the way the "cycle of credibility" is being characterized). There are two important senses in which scientists' action is understood here as being social in character or socially conditioned. First, the goal scientists pursue by their action—namely, credibility—is a social property. One's credibility is dependent for its existence and utility-yielding properties on other persons in a social context. Second, action oriented towards achieving this goal is in fact a process of social interaction. Scientists choose their strategies and tactics constrained by the actual and anticipated reactions of other scientists.

What kind of a thing, ultimately, is credibility as the property to be maximized, and why is it that scientists would be interested in maximizing it? Latour and Woolgar do not have much to say about this. They say that "there is no ultimate objective to scientific investment other than the continual redeployment of accumulated resources" (ibid., 198) and that "[t]he objective of market activity is to extend and *speed up the credibility cycle as a whole*" (ibid., 207). Presumably, they do not think that credibility is maximized for its own sake. Even though they say that there is no objective beyond "the continual redeployment of accumulated resources," they might be prepared to think that credibility is an instrumental entity that is maximized because it can be used to acquire other things that yield utility. That they do not discuss these questions may be unfortunate, as the picture they give about scientists' action has now been painted in very monotonous colors. Indeed, it would seem that if scientists were viewed as utility maximizers, Latour and Woolgar's scientists would have only one argument in their utility function, namely, credibility. It is, of course, true that this is compatible with a variety of factors that may motivate scientists, but it is also clear that it does not encompass everything that might motivate scientists to act.

It has been suggested by Williams and Law (1980, 313) that "[t]o view science as the disinterested search for credibility is, in its own way, as misleading as to view it as the disinterested search for truth." They argue that science is more broadly social in character than suggested by the credibility model; calculations about credibility usually are moderated by the social context, loaded with non-credibility issues, in which they occur. There is a broader interactional order with contingent entanglements and commitments that shapes considerations of credibility. "Actors come to value their colleagues as friends, confidants or opponents. Time and effort are invested in these other involvements, public positions are adopted, and the network of side-bets grows and becomes constraining." (ibid., 313) While the market analogy of the credibility model depicts science as social action, these remarks propose to give it more concrete sociopsychological content.

Williams and Law do not develop their suggestion into a well-formulated notion. In this respect, Peter Earl's (1983) "behavioral model of economists' behavior" goes further. It specifies the goal component of scientists' actions more richly than do Latour and Woolgar, in terms of subcomponents of psychic and monetary cost and return. Furthermore, unlike the latter, Earl is interested in the motivations underlying scientists' actions. Earl considers the academic economist's position as analogous to that of managers in business firms as conceptualized in a behavioral framework.

Earl's model depicts scientists as having lexicographic preferences so that their choices are based on certain priorities rather than trade-offs among their goals. The set of goals that "an academic economist will rank highly" includes predictive power, fame and prestige, high income and certain lifestyle, nice (social, natural, academic) environment, minimum effort and avoidance of anxiety (Earl, 1983, 94). Most of these goals have social content or are dependent on social matters. This is the case, for instance, with Earl's notion of fame, which is close to that of credibility in Latour and Woolgar. Thus, we have here another example of a social theory of science built upon the idea that (at least some of) the ends of scientists' action have a social character.

Earl's point is that "ideas find academic acceptance not necessarily because of their intrinsic scientific worth . . . but rather because they are salable as tools which enable their users more easily to reach their goals" (Earl, 1983, 90).[5] Earl argues that his model can be utilized to explain facts such as the reign of neoclassical equilibrium theorizing by referring to the alleged fact that economists "will tend to be attracted by the leisure or promotion advantages that come from practicing as a technically competent equilibrium theorist rather than attempting to swim against the tide as, say, a behavioral economist" (ibid., 101).

That economists are not critical Popperians is characterized by Earl in the following way: "If an anomaly is discovered, information overloads are avoided by not asking difficult questions. A limited rule-guided search will usually provide a way of coping with a difficulty without challenging fundamental questions . . . . As long as (the rules) seem to be working and the scientist is able to meet her aspirations she will have no obvious reason to question them" (ibid., 101). The way Earl depicts economists' behavior is closer to Lakatosian ideas, but there are important differences here, such as the latter's neglect of "the role played by scientists' personal motivations" (ibid., 102). Economists are too conservative and too much guided by their personal aspirations to be real Lakatosians. "If a switch to a new (scientific research program) would have no positive career payoff, yet would involve an admission that she believes she has hitherto been foolishly wasting her time (thus hurting her self-image), the economist may carry on as before" (ibid., 103). In general, Earl's thesis is that "choice between theories ultimately rests on personal preferences and perceptions, shaped as they are by predispositions, by upbringing in a social/academic/economic context, and by the selectivity of cognitive processes" (ibid., 118). Note that this statement contains not only the idea that some of the ends of economists' action are social states and processes, but also the idea that the ends and means of economists' action are shaped by social factors. This makes such action doubly socially conditioned.

From our point of view, one important idea involved in these models of scientists' behavior is the implied dependence of acceptance and rejection on socially loaded factors. Scientists tend to be committed to particular theories and approaches to the extent that they have made prior investments of time, effort, and money in the acquisition of the mastering of those theories and techniques. In the early stages of those prior investments it is relatively easier to change one's beliefs and orientation, but once they become established in the form of institutions and traditions, a change will be more difficult; however, it should not be impossible. The credibility of scientists and the research groups they form is dependent on their continued ability to produce new marketable results. This ability depends on both the investments made and the demand in the market. This demand is not, of course, pregiven, that is, independent of the marketing efforts of the producers of those results, but it is not completely determined by them either. In any case, if the demand or marketability of a particular kind of result decreases considerably, the only option available to a scientist willing to stay in the business may be to change his beliefs. In general, beliefs or methods are not rejected if the cost of such rejection is too high to the standing of the scientist or his research grou Obviously, in this picture, if there are rejections, they are not based on falsifications.

It is noteworthy that, to a large extent, Earl's model is intended as an account of the behavior of economists making "conservative" choices, i.e., sticking to already established ("mainstream") frameworks and techniques and thereby contributing to their further entrenchment. This means that the contents of those frameworks and their origination and winning of a ruling position remain unaccounted for. The model attempts to chart the social and psychological mechanisms of maintenance while leaving the mechanisms of genesis uncharted.

While Earl's sketch model fills in some gaps in Latour and Woolgar's view of scientific action, there are some shared assumptions. Both models are agent-centered and internalist; their focus is on scientists acting in the intellectual market within the boundaries of their discipline. Furthermore, both assume an integrated agent with a well-formulated decision problem in the single scientific game in which he participates. These assumptions have been challenged by Knorr-Cetina, among others (see also, Latour, 1987).

TRANSSCIENTIFIC ARENAS

Knorr-Cetina (1981, 1982) argues that it is not only the laboratory that constitutes the field or market in that scientists play their games. There are other fields or arenas of scientists' action which she calls "transscientific fields" (1981) and "transepistemic arenas" (1982).[6] "It is crucial to realize that the moves which are made in the various arenas of action need not add up to one particular game played according to a coherent set of rules in pursuit of a definite goal. The picture we get is more that of a field on which different games are played at the same time by a variety of people." (Knorr-Cetina, 1982, 118.) Scientists do not play merely with their disciplinary colleagues, but also with other people such as those who have power over the resources of research, such as funding, careers, etc.

This implies that the notion of a scientific community as a specialty network, as something that is restricted to the relationships between specialists in a field, becomes obsolete (see ibid., 114-116).

Knorr-Cetina's suggestion implies, of course, an extension of sociological considerations to encompass social realms external to science. It is, however, different from some of the applications of the Edinburgh program in that it does not imply an agenda for searching for the causal imprints of wider social relations on the contents of scientific theories. Knorr-Cetina's point is, so to speak, to enlarge the market of scientists' action beyond the boundaries of specific scientific fields. To argue that scientists act in several arenas, including extrascientific ones, is to suggest that, if viewed as rhetoric, scientists' action has what may be called a *multiple-auditorial* character in that acts of persuasion may and have to be directed to different audiences using different strategies adapted to the qualities of the respective audiences.

There is no doubt that economics is multiple-auditorial in such a sense. Some of the typical audiences confronted by economists are: the like-minded within academic economics; their critics within the discipline; their students, both undergraduate and graduate; scholars other than economists within academia; their sponsors, both in asking for support and in reporting their successes; the sophisticated members of the public such as journalists, politicians, business managers, and bureaucrats; and the larger public unfamiliar with their message and language (see Coats, 1988, 70; Goodwin, 1988, 209-210). This list could, of course, be extended and its items divided further into smaller and more specific audiences.

While it may be that sciences typically are multiple-auditorial, it is probable that not all sciences are so to the same extent and in the same way. For instance, it would seem obvious that there are differences between physics, economics, and management research in this respect. Richard Whitley's framework, to be discussed next, draws attention to these and other important differences.

ECONOMICS AS A PARTITIONED BUREAUCRACY

Whitley has developed a framework, based on the so-called contingency approach to studying organizations, which gathers together, renames, and modifies many of the ingredients that we have found in the sociologies of science considered so far. While many of the theories considered above build upon generalizations based on findings from one or more disciplines, what makes Whitley's work particularly interesting from our point of view is his attempt at a classification or taxonomy of different kinds of disciplines, in which economics also finds a place (Whitley, 1984a, 1986; see also Coats, 1984, Loasby, 1986).

Whitley considers scientific disciplines as "reputational work organizations" oriented towards knowledge production. They are organized on the basis of structures of coordination and uncertainty inherent to scientific communities and conditioned by their external social contexts. Disciplines vary as to the ways in which and degrees to which they are so organized and conditioned. This major insight allows Whitley to make attempts to find out what is peculiar about each individual discipline, such as post-war physics, management studies, and post-1870 economics.

The first notion in Whitley's framework that makes any science essentially a social enterprise is the idea that scientists pursue positive reputations from particular groups within (or, in some cases, without) their disciplines. This is a matter of the goals of scientists' actions seen as social action: positive reputation as one of the goals has a social character.

The acquisition of reputations is controlled by other features related to each discipline. These features are conceptualized by Whitley to form a framework for concrete analysis. Mutual dependence and task uncertainty are the two key concepts in the framework. These help organize the idea of the social character of the structure of disciplinary action.

The concept of *mutual dependence* refers to "the extent to which scientists have to coordinate and specifically interrelate their research with that of a well-defined and bounded group of fellow specialists"

(Whitley, 1984a, 86). Mutual dependence is divided into two subcategories. The degree of *functional dependence* is concerned with "the extent to which researchers have to use the specific results, ideas, and procedures of fellow specialists in order to construct knowledge claims which are regarded as competent and useful contributions" (ibid., 88). This is a matter of coordinating the outcomes and competence standards of research. The degree of *strategic dependence* is "the extent to which researchers have to persuade colleagues of the significance and importance of their problem and approach to obtain a high reputation from them" (ibid., 88). This is a matter of coordinating goals and strategies of research.

The degree of these two aspects of dependence vary from field to field (from "low" to "high"). To connect these notions to our concerns, in a sense, we may say that the higher the degree of mutual dependence, the stronger the social conditioning of research.

The second of the two key categories in Whitley's framework is that of *task uncertainty*. It is also divided into two subcategories. The degree of *technical task uncertainty* varies in accordance with the extent to which the use of research techniques is either well established and standardized or open to personal, fluid choices and the extent to which the interpretation of results is either straightforward and uniform or ambiguous and open to conflict (ibid., 121). The degree of *strategic uncertainty* "encompasses uncertainty about intellectual priorities, the significance of research topics and preferred ways of tackling them, the likely reputational pay-off of different research strategies, and the relevance of task outcomes for collective intellectual goals" (ibid., 123).

Again, the degree of these two aspects of uncertainty varies across disciplines (from "low" to "high"). It is easy to see that both of them have been defined by Whitley in a way that makes them irreducibly socially loaded notions.

On the basis of these two dimensions of dependence and uncertainty and the respective variables, each with two values, Whitley is able to construe a typology of scientific fields in which economics represents a specific type. To give examples, post-1945 physics belongs to the type of "conceptually integrated bureaucracy" with high functional and strategic dependence and low technical and strategic uncertainty, while management studies and British sociology are "fragmented adhocracies," characterized by low functional and strategic dependence and high technical and strategic uncertainty (see

Whitley, 1984b). Economics occupies a category of its own, called a "*partitioned bureaucracy*" by Whitley, and characterized by low functional and high strategic dependence combined with high technical and low strategic task uncertainty.

Economics is claimed to be a partitioned bureaucracy in the sense that it "seems to combine considerable mutual dependence and task predictability in the analytical core of the subject with rather less coordination and integration of research results in peripheral 'applied' subfields where the meaning and implications of research are often ambiguous and open to conflicting interpretations" (Whitley, 1986, 191). Whitley relies on the testimonies by many witnesses in making the point that "this combination of relatively strong collective control over 'hard-core' . . . assumptions and principles with uncertainty and 'anomalous' peripheral areas would be unstable if reputations in the analytical core were dependent upon success in controlling and coordinating empirical phenomena. However, research involving statistical data and empirical indicators seems to be separated from theoretical model-building activities in economics and to have lower intellectual prestige . . . . Thus, theoreticians can obtain high reputations by producing highly abstract and general models of 'ideal' worlds without considering how they are related to economic phenomena in actual worlds; their work is partitioned from empirical economic studies, and they do not need to demonstrate any systematic connection to them" (ibid., 191-192).

A possibility of misunderstanding may lurk behind Whitley's more categorical pronouncements and I suppose he would agree on the need to eliminate it. This is the reading of Whitley's framework implying that there are strictly and qualitatively separate types of sciences, each with an unshakable identity of their own. However, as should be clear from the way the typology is constructed, the differences between kinds of disciplines are not very strict and clear. The crucial point is that the degree of mutual dependence and task uncertainty are said to vary from "low" to "high," and this, of course, leaves plenty of room for differences of degree, intermediate cases, overlap, etc. Consequently, physics as a "conceptually integrated bureaucracy" is obviously not completely devoid of the sort of partition that is claimed to characterize modern Anglo-Saxon economics as a "highly rule-governed field which separates the stable and controlled analytical core from the uncertain and ambiguous periphery" (Whitley, 1986, 192). The point has to be that there is a difference of degree between economics and physics as to the separation of theory from empirical work.

Another point to be made is that the features we take to characterize a given discipline or field of research are dependent on how we mark off that field. Whitley's "post-1870 economics" is a case in point. An obvious criticism of his account of this field is that he has an unnecessarily restricted view of the extent to which the theoretical core of post-1870 economics is uniform and coherent and has control over all research, thus underestimating the role of dissenting traditions (Coats, 1984, 225). However, it seems to me that Whitley has defined "post-1870 economics" as denoting only the mainstream orthodoxy so as to save the notion from such charges. Still, one may ask whether, after all, a sensible definition can be given to the notion of mainstream economics such that the alleged degree of unity could be preserved.[7]

The notions of mutual dependence and task uncertainty imply that sciences are socially conditioned on the level of scientists' actions and interactions within their disciplines. There is more to the social conditioning of sciences, namely, the role of the determinants that Whitley calls *contextual factors*, which include wider social facts external to science plus other factors: *reputational autonomy* (which concerns performance standards, significance standards, and problem formulation and descriptive terms); *concentration of control over the means of intellectual production and distribution;* and *audience structure* (consisting of audience variety and audience equivalence) (Whitley, 1984a, 220-238). These contextual factors have an impact on the degrees of mutual dependence and task uncertainty (ibid., 104-112, 139-147). These notions make it possible to view much of scientists' actions being performed in transscientific arenas in Knorr-Cetina's sense, albeit to a varying extent—more so in management studies than in economics, for example. Research in the field of management studies is more strongly multiple-auditorial than that in economics since the degree of audience variety and audience equivalence in acquiring reputations is higher in the former. Reputational autonomy is low in fragmented adhocracies and also in the applied periphery of economics as a partitioned bureaucracy, while it is high in the theoretical core of economics.

In conclusion, if post-1870 mainstream economics were a partitioned bureaucracy in Whitley's sense, then we would have here an account that characterizes theory appraisal in economics as a socially conditioned process that has nothing to do with Popperian or Lakatosian standards of rational science, or, even more radically, that is not systematically constrained by empirical evidence.

SOME LESSONS

As pointed out by many commentators, economists do not behave according to the norms prescribed by falsificationist methodology even though they often preach that same methodology. Mark Blaug (1980), for instance, makes this observation and also insists that economists should try to do their best to obey the falsificationist prescriptions, to which, after all, they themselves subscribe in their own methodological declarations.

What should be said about these ideas in light of the findings and suggestions of the recent sociology of science? We have learned that sociologists depict science as socially conditioned in that, for instance, scientists are viewed as pursuing social ends in an interactive process of negotiation and persuasion, shaped by the social context. What conclusions do these insights suggest?

First, the negative observation that the grounds on which economists "choose" theories are not falsificationist grounds conforms to what seems to be the case in other disciplines, too. Thus, it might give some consolation to economists to find that at least they are not much less "scientific" in this sense than researchers in other fields. Furthermore, falsificationist rhetoric by participants, not adequately reflecting their actual research practices, seems to be a typical characteristic of other disciplines as well; again, there seems to be nothing peculiar about economics in this regard.

Second, while neither Blaug nor many others unhappy with the situation have shown what in fact takes place in economics and why, work designed after the example of the sociological accounts of science might be able to contribute to describing and explaining some of the facts about economics. This concerns not only the negative fact of economists not obeying falsificationist norms but also positive facts related to how economists do behave and believe and why.

As to the negative fact, what we have learned is that there seems to be something in the way sciences are socially organized that gives support to the fact that scientists behave in ways that systematically diverge from falsificationist canons. Action animated by credibility and interaction, amounting to negotiation and persuasion, do not appear to fit the categories of bold conjecture, falsification, corroboration, and progressive problem-shift. In particular, if economics were a "partitioned bureaucracy," conditioned by contextual factors in the way Whitley suggests, this would give us further reasons for describing and explaining phenomena within this discipline in non-falsificationist terms.[8]

One particular doctrine of Popperian epistemology that threatens to be undermined can be separately mentioned: namely, the distinction between the context of discovery and the context of justification. For Popper, only the latter is characterized by systematic rationality, while the discovery process is open to various nonrational influences the workings of which cannot be systematized philosophically but should be studied by sociology, psychology, even political science. This distinction, with all the epistemological burden it is supposed to carry, gets blurred by the findings of sociologists. "Whether a proposed knowledge claim is judged plausible or implausible, interesting, unbelievable or nonsensical, may depend upon *who* proposed the result, *where* the work where done, and how it was accomplished .... Thus, the scientific community itself lends crucial weight to the context of discovery in response to a knowledge claim" (Knorr-Cetina, 1981, 7).

It may be maintained that the rationality of Popperian norms remains intact even though actual practice does not, as a contingent fact, conform to them. The third point to make is that recent sociology of science alerts us to the possibility, or even high likelihood, that (at least some of the) Popperian norms will inevitably prove ineffectual. Again, if economics is a drastically partitioned discipline, due to its social structure, there would seem to be little hope of getting falsificationist criteria applied, even approximately, in the field. This state of affairs would be rooted in the social organization or economics and would be independent of any single economist's possible endeavors to act in a contrary fashion. Therefore, the mere prescribing that falsificationism be adopted by economists in actual practice would fall on deaf ears. Blaug's prescriptive statement to the effect that economists should try harder to satisfy the falsificationist norms of ideal science may prove utterly utopian in the absence of a radical (probably itself utopian) revolution in the social organization of economics. One would then have to ask about the grounds for insisting on the imposition of such norms.

The minimum point made by the sociological theories is that *the* *fate of a scientific statement is at least partly dependent on the social context* (where by "fate" I mean such things as invention, introduction, persistence, acceptance, rejection, modification, etc.). The minimum point is a descriptive statement of the actual situation in the actual past, present, and future of science. The extent to which various statements in various branches of a discipline, like economics, in various stages of their development are socially conditioned in one or more ways is not, of course, decidable a priori. Still, the minimum point seems to me plausible enough to be taken seriously. It just requires a lot of empirical work to find out where, when, and how in economics the point might hold. Here we encounter the fact that the sociology of economics as a field of inquiry is still in its infancy.

There are two questions of a philosophical sort that can be discussed, if not settled, prior to extensive empirical evidence. First, are there standards of rationality that surpass or are independent of the actualities summarized by the minimum point? Popperian and many other methodologists think there are such standards. I will not tackle this important issue directly here. Second, does admitting that (at least much of) science is socially conditioned necessarily lead one to the idea that the truth value of scientific statements is similarly indexical or context-dependent, or that the pursuit and attainment of objective truth is a meaningless or useless notion? It is this antirealist conclusion suggested by some relativist sociologists that I do not buy. Here I side with the Popperians. The final section is devoted to this issue.

RELATIVISM AND REALISM

Much of the recent sociology of science explicitly declares itself as adhering to "relativism." Since this has caused both confusion and contempt, I will conclude this chapter with a brief discussion of some aspects of this philosophically interesting notion. Relativism appears in a great variety of versions. Building upon the statement "X is relative to Y," relativism takes on different forms depending on how "X" is specified (as the contents of beliefs, aims of research, criteria of acceptance and rejection, truth, reality, etc.) and on how "Y" is specified (as language, professional interests, audience, culture or form of life, etc.). My focus will be on those versions that are related to the realist notion of truth as something that hinges upon the objective structure of the world.

X The question we have to face is this: If we accept the idea that a scientific discipline, such as economics, is bound to be socially conditioned in a number of ways, can we still take it as actually, or at least potentially, constituting or providing true knowledge about the way the preexisting economy is? I am inclined to answer this question positively, while some contemporary sociologists of science have either an indifferent or a negative position. In what follows, no penetrating arguments can be formulated to back up any of these answers. Only some of the issues and outlines of some of the arguments will be laid down in somewhat simplified terms.

One way of giving truth a role in a socially conditioned science is to postulate it as one of the ends that scientists pursue. This might be what Whitley suggests. In the second sentence of his book, he says that modern sciences "attempt to monopolize the production of true knowledge of the world" (Whitley, 1984a, 1). This might be taken to amount to the assumption that individual scientists or the groups they constitute have truth about the world as one of their ends. On this assumption, scientists would pursue strong epistemic goals in addition to being activated by the social goal of high positive reputation. This combination appears as completely possible. It seems to me, however, that if this were Whitley's idea, the notion of knowledge defined in terms of truth would be an external element in his framework. It would not have an intimate connection to the other elements.

Alternatively, it may also be that Whitley is among those sociologists of science who relegate the notions of truth and reality merely to scientists' own ideological discourse by means of which the participants try to justify their beliefs and actions. Rhetorical usage of "true," "false," and "real" does not, on this view, have any other function beyond that involved in the persuasion and negotiation by practicing scientists. As Collins (1981a, 218) puts it, these terms (and others, such as "rational" and "progressive") are used in sociological explanations exclusively as "actor's categories." They would have no role among the explanantia of actors' beliefs.

In general, however, regarding science as a social process of rhetorical persuasion does not in itself threaten in any way the idea of science having a veristic dimension. Beliefs marketed by using whatever rhetorical means are found efficient (or inefficient) for that purpose may be true or may be false, and scientists may or may not have the truth as an end of their rhetorical actions. Thus, the recent suggestion that economics has a rhetorical character does not, without additional premises, undermine the realist intuitions about truth and reality (see Mäki, 1988).

Truth does not have to be postulated as a goal of scientists' action in order for it to emerge as an outcome of the interactions between scientists (as an invisible-hand consequence, as it were). Neither of these roles is reserved for truth in Latour and Woolgar. While Bourdieu (1975) postulated that the pursuit of symbolic capital or credit by scientists in a competitive universe would ultimately promote the attainment of the truth, in Latour and Woolgar's (1986 (1979)) framework there is no connection between investments in credibility on the one hand and truth seeking, or truth finding, or truth approaching, on the other. Investments are made only for the purpose of accumulating one's credibility, which is to be reinvested again with the same purpose. And the "cycle of credibility" does not include an invisible-hand teleology towards the truth.

The legitimacy of talk about truth is sometimes questioned by appealing to the socially conditioned nature of the standards of justification. Some representatives of the current sociology of science seem to think that a meaningful notion of truth presupposes the existence of final, and presumably infallible, criteria or procedures of deciding whether a given knowledge claim is in fact true or false. Because no one has been (nor, most probably, will be) able to provide such unambiguous, compelling, and universally valid criteria, some sociologists of knowledge conclude that truth itself is dependent on those same social factors that have a role in conditioning which claims are accepted and which are rejected in a scientific community. This amounts to confusing the truth and justification components of knowledge with each other. Even if it were the case that the rules of argument and the criteria of justification are to be defined in terms that are internal to a social order, it would not yet follow from this that the very notion of truth should be so defined. It may be argued that whether or not our reasons for accepting a given statement as true are good or bad, socially conditioned or unconditioned, the truth value of that statement remains stable by virtue of its relation to its object.

Truth in the realist sense as something that characterizes the relation between a representation and its object in the preexisting world has been questioned by some constructivist sociologists by forwarding the argument that the objects of scientific representations are socially constructed. Science as a process of manufacture or construction of *knowledge* also often appears to the constructivists as a process of construction of the *world*, or worlds. Science is not an attempt to truly represent the preexisting reality, as science never in fact "touches" reality: "Where in the laboratory . . . do we find . . . nature? Most (!) of the reality with which scientists deal is highly preconstructed, if not wholly artificial" (Knorr-Cetina, 1984, 225). Here, it sounds as if reality itself were relative to the social processes of investigating it.

It seems to me that the notion of the constructedness or artificiality of the domain of phenomena studied in laboratories has been left in a considerably obscure state by the constructivists. There seem to be at least two possible ways to specify the meaning of the notion. First, the idea might be given the radical form in which it is stated that whatever it is that scientists encounter in their laboratories, such as the elements, forces, and fields, is ultimately just human construction in a social setting. These items do not have an independent existence. Radical constructivism of this sort is incompatible with realism. Secondly, it might be suggested that it is not the basic constituents of nature, but instead, their specific constellations and modes of interaction that are artificially constructed in the laboratory. It is the substances-and-forces-as-purified-and-isolated-from-"disturbing"-ingredients-and-forces that constitute the artificial objects of study. It is these purifications and isolations, in this interpretation, that are manufactured and in that sense artificial. This position may be called moderate constructivism and it is compatible with realism. On the basis of a suitable interpretation of the word "most" in the above quotation from Knorr-Cetina, she might, after all, count as a moderate constructivist.

Now, this suggestion also has some relevance with respect to the situation in economics. Economic models are typically constructed analogously to the models of laboratory sciences, the analogy being that both are based on purifications and isolations of a few allegedly crucial relations from everything else. The disanalogy is that while in the laboratory circumstances these purifications and isolations can be carried out materially, in economics they are usually possible only conceptually. This is accomplished by means of the use of assumptions such as *ceteris paribus*, which economists know never quite hold true. Thus, one may say, the "worlds" constructed by economists are also artificial. The question that remains is this: in which of the two senses suggested above are the worlds artificial? I suggest that sometimes, at least, it should be possible to take the idea seriously that it is the isolations, closures, and simplifications involved in economic models that are artificial rather than at least all the economic entities, relations, and forces that are postulated. These two interpretive options may be called the realist and the fictionalist reading of economic models. (See Mäki, 1990 and 1991.)

Ian Hacking's comments on Latour and Woolgar's radical constructivism supplement my point. Latour and Woolgar (1986 (1979), 64) state the following: "The central importance of this material arrangement (in the laboratory) is that none of the phenomena 'about which' participants talk could exist without it . . . . It is not simply that phenomena *depend on* certain material instrumentation; rather, the phenomena *are thoroughly constituted by* the material setting of the laboratory. The artificial reality, which participants describe in terms of an objective entity, has in fact been constructed by the use of inscription devices." Hacking (1988, 285) rightly points out that the contrast drawn here between "artificial" and "objective" may be misleading because of the ambiguity of the terms involved. "Artificial" may mean at least two things: first, "produced by man, not occurring naturally"; and second, "made in imitation of a natural product, especially as a substitute—not genuine." Only in the latter sense of the term could we consider the possibility that what is artificial lacks objectivity in some relevant sense. On the other hand, artificial objects in the first sense may exist objectively. As Hacking argues, this concerns shoes as well as the manmade objects (materials, phenomena) of the laboratory. In accordance with what was stated in the preceding paragraph, I would like to suggest that the situation in economics is not much different in this respect, though it may be more complicated.

Some of the popular forms of "relativism" prevalent in the sociological discussions on science are philosophically much less radical than the ontological versions. Barnes and Bloor, for instance, take their own version as being composed of "(i) the observation that beliefs on a certain topic vary, and (ii) the conviction that which of these beliefs is found in a given context depends on, or is relative to, the circumstances of the users," and (iii) the symmetry or equivalent postulate that "all beliefs are on a par with one another with respect to the causes of their credibility" (Barnes and Bloor, 1982, 22-23).

Barnes and Bloor emphasize that the equivalence postulate "is not that all beliefs are equally true or equally false, but that regardless of truth and falsity the fact of their credibility is to be seen as equally problematic" (ibid., 23). Abstracting from other textual evidence, this could be taken to imply that theirs is not relativism about truth. What counts as true is claimed to be relative to social context, that is, reasons for belief are regarded as socially conditioned irrespective of whether the belief is true or false. For the purposes of sociological explanation, the question of truth and falsehood is bracketed; hence, there is no relevant distinction between truth and what counts as true or between what is the case and what is taken to be the case.

The proponents of the Edinburgh approach sometimes sound like straightforward realists about the world and its role in cognition. For instance, Barnes (1977, 25) subscribes to ontological realism and gives it epistemological import: "there is indeed one world, one reality, 'out there,' the source of our perceptions if not their total determinant, the cause of our expectations being fulfilled or disappointed, our endeavours succeeding or being frustrated." He also says that he does not agree with those sociologists (such as Collins and Latour and Woolgar) who "claim that the world has nothing whatsoever to do with what is believed about it" (Barnes, 1984, 122; see also ibid., 124, n12, and his 1974, viii). It is true that, as opposed to Barnes, Collins (1981c, 54) argues that, when designing sociological explanations, it has to be assumed that "the natural world in no way constrains what is believed to be" (see also Collins and Cox 1976, 436-348). Still, Collins (1981a, 218) seems to imply realism about truth when he states that "what is true may be perceived by scientists as being false, and vice versa."

Bloor gives a characterization of the minimum element of his "relativist" program in terms which make it easy for a realist to agree: "Men's ideas about the workings of the world have varied greatly. This has been true within science just as much as in other areas of culture. Such variation forms the starting point for the sociology of knowledge and constitutes its main problem. What are the causes of this variation, and how and why does it change?" (Bloor, 1976, 3) Barnes and Bloor (1982, 34) put it more clearly in realist terms when they say that "reality is, after all, a common factor in all the vastly different cognitive responses that men produce to it. Being a common factor it is not a promising candidate to field as an explanation of that variation." In other words, they subscribe to the realist principle that there is such a thing as the workings of the world about which men have beliefs. No departure from the principle follows from admitting that these beliefs vary and change and often do so because of various sorts of social facts.

It may be granted that at least some variation of beliefs and standards is socially conditioned. This poses no threat to the notion that beliefs may be true or may be false. On the contrary, the idea of social conditioning opens up an important dimension in regard to the role of truth in our conception of science. If beliefs are socially conditioned and if beliefs have the possibility of being true of the world, we may ask about the social conditions of the actualization of that possibility. Indeed, it becomes legitimate to pose this question: What are the favorable social conditions, both internal and external to a particular science such as economics, that help direct a discipline to approach the truth (for instance, by inspiring researchers to seek the truth and helping them be successful in that endeavor)? In other words, what are the social conditions for the implementation of realist rationality? Not all social conditions are supposedly equal in this respect, which implies that successful truth hunting is relative to social matters. Whether or not this insight be regarded as still another form of relativism, methodologists interested in the role of truth in science should incorporate it into their investigations. A realist ("absolutist") methodology, in short, cannot do without a ("relativist") sociology of science.

NOTES

1. For a formulation of the asymmetry thesis by one of its advocates, we may cite Laudan (1977, 188-189): "When a thinker does what it is rational to do, we need inquire no further into the causes of his action; whereas, when he does what is in fact irrational—even if he believes it to be rational—we require some further sociological explanation." On this, Popper and Lakatos agree.

2. For a qualification, see Bloor (1981, 203): "The question of the kind or scope of social factors at work in a system of knowledge is entirely contingent and can only be established by empirical study. The important point, however, is that where broad social factors are not involved, narrow ones take over. The sociology of knowledge is still relevant. As well as *external* sociology of knowledge there is also an *internal* sociology of knowledge."

3. In the context of economics, the rhetorical processes of inquiry

have been insightfully studied by Donald McCloskey (1985) and Arjo Klamer (1983). See also Klamer, McCloskey, and Solow (1988).

4. One might add that the earlier generations of sociologists of science had observed similar features in the social process of science. For instance, Merton (1968) discusses the so-called "Matthew effect" that refers to the phenomenon that scientists with a long list of publications are more likely to get their work published than those with no established reputation, although the submissions of the beginners may be superior. To Merton, this is a deficiency in the scientific process. The difference between the two generations is that, for Latour and Woolgar, it is not at all clear in what sense the Matthew effect could be evaluated as a deficiency or a submission could be assessed as superior to another independently of the actual social process of credibility formation.

5. Note that, unlike many contemporary sociologists of science, Earl holds a notion of the "intrinsic scientific worth" of ideas, unimpregnated by the actual social process whereby they are accepted or rejected.

6. I would prefer the terms "transscientific field" or "transscientific arena" to "transepistemic arena," introduced by Knorr-Cetina (1982) to replace the earlier suggestion. The reason is that the latter suggestion appears to imply, first, that scientific arenas are necessarily epistemic, and second, that nonscientific arenas are necessarily non-epistemic. I find both of these implicit presumptions dubious.

7. Provided there would be enough unity in orthodox mainstream economics to warrant most of Whitley's suggestions, one may question whether the discipline as a whole is characterized by high technical task uncertainty, as he maintains. This may be the case in empirical work, but obviously not so in the theoretical core of orthodox economics (Loasby 1986, 224). This, of course, does not undermine the suggested separation between theoretical and empirical work.

8. We have suggested that economics is not much different from "other fields" or "natural sciences" in some regards while in some other respects it is peculiar. One lesson that suggests itself is that from a perspective such as Whitley's it seems clear that methodologists of economics should pay much more attention to fields like biochemistry

and ecology than to physics when preparing assessments about the scientific status of economics. The traditional reference to "natural sciences" when one in fact has physics in mind is often seriously misleading. It may be much more instructive to take into account differences among the natural (and social) sciences when viewing economics from a comparative perspective. And such a perspective certainly is in itself extremely instructive.

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