First publ. in: Science observed. Perspectives on the Social Studies of Science / Karin D. Knorr-Cetina and Michael Mulkay (eds.). London: Sage, 1983, pp. 1-17

Introduction: Emerging Principles in Social Studies of Science

Karin D. Knorr-Cetina
Wesleyan University, USA
Michael Mulkay
University of York, UK

Several times in recent years we have witnessed the appearance of new analytical perspectives in social studies of science. Inspired by wider developments in philosophy and sociology, these perspectives have redefined traditional concerns in the study of science and reformulated problems of current interest and relevance. All of these perspectives are sustained by a programme of detailed empirical research which substantiates and elaborates their theoretical concerns. Those included in this volume are as follows: the ethnomethodological study of scientific practice (chapters 8 and 9); the discourse analysis of scientists' talk, writing and pictures (chapter 7); the ethnography of scientific work (chapters 5 and 6); the relativist programme in science studies (chapter 4); and the sociology of knowledge perspective in 'strong' and 'weak' versions (chapters 2 and 3). It is impossible to identify a single set of characteristics shared by all of these analytical positions. Nevertheless, there are certain broad themes which link them together through a series of family resemblances. In these introductory remarks, we will focus on four themes which are common to most of the following chapters. These themes are, first, a concern to include the technical content of science within the scope of sociological analysis; second, whether to adopt an internalist or an externalist methodology in the study of science; third, the 'linguistic turn' underlying several of these

perspectives; and fourth, their rejection of traditional distinctions such as that between the social and the scientific. In sketching these characteristics, we pass over some of the major divides which separate the above perspectives from each other. Without fail, the attention- of the reader will be drawn to these differences in subsequent chapters.

The rise of the sociology of scientific knowledge

Perhaps the most significant event in the social study of science since the emergence of the sociology of science pioneered by Robert Merton in the 1930s is the rise of a genuine sociology of scientific knowledge in the 1970s. For the first time in the history of sociological thought there was a vigorous and systematic effort to subject natural and technological scientific knowledge to the same scrutiny which has long been brought to bear on other systems of beliefs, such as religious and philosophical knowledge or political thought. The sociology of knowledge has been broadly defined as a theory of the social or existential conditioning of thought (Mannheim, 1954). Its roots are found in the writings of Marx and Durkheim, its development as an autonomous discipline is commonly credited to Max Scheler (1926) and Karl Mannheim (1954). Several authors have recently traced the long and varied history of the sociology of knowledge, and commented upon its virtual disappearance from view after the second world war (e.g., Barnes, 1974, 1977; Barber, 1975:Bloor, 1976;Mulkay, 1979a;Stehr, 1981; Stehr and Meja, 1982: Gieryn, 1982).

However, it appears that far from dying a premature death, the issues raised by the classical sociology of knowledge gained much potential and appeal during their slumber. As a result of subsequent developments in the philosophy and historiography of science, these problems were vigorously revived in a number of fresh approaches to the sociology of knowledge, including those spelled out in the works mentioned above (see in addition Collins, 1974, 1975; Restivo, 1975, 1978, 1982; Mulkay, 1979b; Latour and Woolgar, 1979;Krohn, 1980; Knorr-Cetina, 1981; the studies collected in Barnes and Shapin, 1979; Knorr, et al., 1980: 88-193; CoUins, 1981; as well as virtually all contributions to this volume). These studies suggest that not only are we in a position to ask sociology of knowledge questions on the basis of a new empirical foundation, but that we are now able to address these questions with some hope of success to the most authoritative and esoteric system of knowledge in modern societies, that of the

natural sciences. There are several lines of argument which are commonly cited in support of a new sociology of scientific knowledge (e.g., Bloor. 1976; Mulkay, 1979a; Hesse, 1980). These are best summarized as the thesis of the underdetermination of scientific theories by the evidence; and the thesis of the theory-ladenness of observation.

The notion that scientific theories are underdetermined by the data derives from a line of argument promoted by Duhem, Poincare', Einstein and Quine.[1] In a nutshell, the thesis says that any theory can be maintained in face of any evidence, provided that we make sufficiently radical adjustments elsewhere in our beliefs. This follows from the fact that no one single theory or theoretical hypothesis can ever be extricated from 'the ever present web of collateral assumptions' so as to be open to conclusive refutation.[2] Rather, if certain observational consequences are entailed by a Theory in connection with a set of auxiliary hypotheses and these consequences do not materialize, we can only draw the weaker conclusion that the Theory and the auxiliary assumptions cannot both be true. It follows that a theory whose predictions do not materialize can always in principle be retained by making appropriate adjustments in the auxiliary hypotheses, if so desired. Conversely, it follows that there are in principle always alternative theories wliich are equally consistent with the evidence and which might reasonably be adopted by scientists.

As Laudan (1982) has recently reminded us, from the logical thesis of underdetermination it does not follow that social factors will have to be invoked to explain why scientists adopt a particular theory.[3] After all, there could be all kinds of factors which influence scientific choice in face of inconclusive evidence. Furthermore, the evidence need not *appear* inconclusive in practice even though it might be from a strictly logical point of view. Nevertheless, if the thesis that scientific theories are logically underdetennined by the evidence is correct, it removes one important constraint on theory acceptance which opens the way for social science investigation. Consider the opposite case, namely that only one out of many theoretical interpretations would ever be fully consistent with the evidence. If the proponents of this hypothesis could demonstrate that a set of data uniquely and conclusively supported their interpretation (which would have to be possible if logical determination existed), they ought to be able to gain acceptance of their interpretation even against the will of their opponents. To deny a demonstrably conclusive relationship of logical entailment would be like denying, in any non-contrived context, some of the most entrenched conventions of our language game. For example.

it would be like denying that a bachelor is an unmarried man.

Thus, while the Duhem-Quine thesis of underdetermination does not prove that social factors structure scientists' theory choices, it does make it more likely that some kinds of non-logical factors play a role. Hence, it can be held *against* any attempt to continue to exempt natural scientific knowledge from social science investigation. If it is true that earlier sociologists of knowledge were prevented from subjecting natural and technological science to sociological analysis by the conviction that nature alone decides (entails) scientific theory choices, then sociology today should no longer be so prevented.

The thesis of the 'theory-ladenness' of observation supports the case for a sociology of scientific knowledge from a slightly different angle. The version of the thesis attributed to Kuhn and Feyerabend and supported by the arguments of Bohm, Hanson, and Toulmin appears to make two major points:[4] (1) observations are theory-impregnated in the sense that they involve auxiliary assumptions in the form of measurement theories, theories of the psychology of observation, theories of linguistic classification, etc.; and (2) observations are theory-impregnated in the sense that what counts as relevant and proper evidence is partly determined by the theoretical paradigm which the evidence is supposed to test. The first point comes down to the thesis of underdetermination, if the auxiliary assumptions on which observation depends are included among those mentioned above in connection with scientific theories. The implications we think are clear. If scientific observations are ridden with theoretical assumptions, then scientists can in principle always doubt a particular observation by challenging the auxiliary assumptions upon which it is based. As some historians and sociologists of science have documented, what counts as a proper and competent observation is indeed at stake in many scientific arguments (e.g., Collins, 1975; see the summary in Shapin, 1982). What bearing the evidence is thought to have on a theory depends on its being accepted as valid information, which in turn depends on the unproblematic acceptance of the background assumptions which are constitutive of the observations.

The second point made by the thesis of the theory-ladenness of observation has implications for the role of observations in theory choice. Observations cannot serve as independent arbiters in questions of theory choice if their relevance, their descriptive identification and their proper measurement depend on the theories involved. This part of the thesis denies that 'observations' remain stable and relevant under a specific description across competing or successive theories, thus

leading to the notion of incommensurable paradigms (Kuhn, 1970). This idea has been challenged for portraying scientific change as a succession of all-encompassing and mutually incomprehensible world-views which leave no room for the diversity, multiple coherence and flexibility of paradigm components observed in scientific practice (cf., Mulkay, 1979a: 48). Yet even if the thesis of incommensurability is relaxed to allow for the fact that proponents of different theories can, in practice, often agree on a 'crucial' observation, it is plausible to assume that scientists' theoretical preferences will inform their appraisal of the experiment and the resources they mobilize for or against the observation. The claims made in connection with the thesis of the theory-ladenness of observation direct our attention to the possible existence of such differential preferences. Sociologists have concluded that it is open to them to study their form and their consequences in actual scientific practice.

In sum, the thesis of underdetermination of scientific theories by the evidence and the thesis of the theory-ladenness of observation support the case of those studies of science which do not exempt knowledge production in the natural sciences from social science investigation. These theses have helped to get the sociology of scientific knowledge off the ground. Yet they have also reopened the debate on relativism in a form that is rife with polemics and misunderstandings. Since the earliest days of the sociology of knowledge, the charge against relativism continues to be that it is an 'ideology of helpless surrender' (Lucacs, 1974: 89) which cannot discriminate among different forms of knowledge (such as those of science and fascism). Furthermore, it is claimed that any insistence on the universal social conditioning of knowledge is self-refuting, since there is no reason to accept the claims of a sociology of knowledge which must themselves be seen as socially conditioned. To assess these charges, let us briefly consider a distinction between epistemic relativism and judgmental relativism.[5]

Epistemic relativism asserts that knowledge is rooted in a particular time and culture. It holds that knowledge does not just mimic nature, and insofar as scientific realism wishes to make such a claim, epistemic relativism is anti-realist. On the other hand, judgmental relativism appears to make the additional claims that all forms of knowledge are 'equally valid', and that we cannot compare different forms of knowledge and discriminate among them. If this were to mean that all forms of knowledge are equally good for a particular agreed-upon purpose it would lead to the consequences spelled out above and

to **the** traditional criticism levelled against relativism.

It should be clear, however, that judgmental relativism manifestly does not follow from epistemic relativism. The belief that scientific knowledge does not merely replicate nature *in no way* commits the epistemic relativist to the view that therefore all forms of knowledge will be equally successful in solving a practical problem, equally adequate in explaining a puzzling phenomenon or, in general, equally acceptable to all participants. Nor does it follow that we cannot discriminate between different forms of knowledge with a view to their relevance or adequacy in regard to a specific goal. Of course, some analysts may wish to argue for the assumptions of judgmental relativism.[6] But the study of the relationship between scientific knowledge and social life in no way entails these assumptions. To claim that judgmental relativism logically follows from epistemic relativism presupposes among other things that we know exactly what we mean by the assertion that knowledge is 'socially or existentially conditioned'. Yet it is precisely the project of the sociology of scientific knowledge to work out *in what sense* and *to what degree* we can speak coherently of knowledge as being rooted in social life. Conceivably, the results of this project may turn out to be perfectly consistent with an instrumentalist interpretation of knowledge or with other epistemological positions which contrast with judgmental relativism.

Methodological internalism

To talk about the emergence of a new sociology of scientific knowledge is not to suggest that there exists a single and clearly defined research programme which circumscribes the area. Rather, we are talking about an upsurge of studies relevant to sociology of knowledge questions.[7] Their common characteristic is that social scientists are for the first time engaged in a systematic investigation of the technical activities, judgments and interpretations of natural and technological scientists from a broadly sociological perspective. Several of the perspectives in which this goal is paramount adopt what can be described as a form of *methodological internalism:* the 'internal' practices of the scientific enterprise constitute the focus of inquiry. Methodological internalism should not be confused with the attempt to explain scientific belief exclusively in terms of technical-rational considerations imputed to scientists, a tendency evident in much traditional historiography and philosophy of science. Explanatory internalism, as the latter could be called, is unsympathetic to the idea that scientific knowledge may have other than 'scientific' explanations. In contrast, methodological internalists tend actively to entertain the possibility of social explanations, whatever these may be. As we shall see, some perspectives ignore questions of explanation, but still maintain their focus on the internal practices of science.

The methodological internalism found in recent social studies of science can be further characterized in terms of the following tendencies: (1) a preference for the microscopic study of scientific practice; (2) a tendency to give priority to the question HOW scientists go about talking and doing science over the question WHY they act as they do; and (3) a tendency to adopt what can loosely be described as a constructivist perspective in addition to the received perspective of the sociology of knowledge. Note that we are talking about tendencies; not all research programmes in the area necessarily subscribe to all three characterizations.

(1)The tendency to analyze scientists' practices in microscopic detail is not new. Historians of science have often preferred the detailed study of a particular period of scientific work or of a particular scientist's life to the study of collective social action. However, both sociology in general and social studies of science have until recently shown the opposite preference. For example, Mannheim's sociology of knowledge was in its classical period marked 'by a tendency to set up grandiose hypothetical schemes' (Coser. 1968: 433) which it sought to verify only on a very general level. Similarly, Durkheim analyzed the social origin of 'collective representations' such as systems of values or systems of classification and advocated a macro- rather than a microscopic methodology. Functionalist sociology of science and scientometrics also prefer the institutional level of analysis and the use of aggregate data. Some current internalist studies of science employ microscopic methods in conjunction with generalizations that refer to social collectivities (e.g., Young, 1977; MacKenzic, 1977; Barnes and Shapin, 1979; Restivo, 1982; see also Barnes, and Chubin and Restivo, this volume). The microscopism of recent science studies is a methodological principle rather than a principle of social explanation and is closely associated with sociologists' growing interest in the production of scientific knowledge.[8]

(2) Like the preference for a microscopic methodology, the tendency to give priority to the question HOW rather than to the question WHY is not limited to recent science studies. Microsociology in general in the last two decades has shown a marked inclination to refrain from

traditional forms of sociological theorizing. Some proponents of microsociology profess to be uninterested in furnishing sociological explanation and concentrate on providing a systematic description of *participants'* interpretative (explanatory) practices (Garfinkel, 1967). Others reject the notion that theories ought to be stated in terms of formal propositions sustained by unambiguous definitions and specified initial conditions. They substitute instead a version of theorizing which seeks to elaborate analytical frames of meaning capable of opening up new research questions and yielding a fresh stream of defensible interpretations in response to these questions (Geertz, 1973). The latter form of theorizing is imbued with what Gilbert Ryle called 'thick description' (1971). In both forms of inquiry, the question HOW rather than WHY receives most attention from the analyst.

Several perspectives in social studies of science have moved in this direction. Ethnographic studies of scientific work tend to advance theoretical frameworks in conjunction with thick description (cf., Latour and Woolgar, 1979; Knorr-Cetina, 1981; Traweek, 1982; see also Latour, and Knorr-Cetina, this volume). Ethnomethodological studies of scientific practice display no interest in theoretical explanation (e.g., Woolgar, 1981; Lynch, 1982; Lynch et al., and Woolgar, this volume). This commitment is shared by the programme of discourse analysis in science studies which rejects traditional forms of explanation on methodological grounds (e.g., Mulkay and Gilbert, 1982a; Mulkay, Potter and Yearley, this volume). Within these research traditions, the question HOW receives primary consideration. On the other hand, the relativist programme and research in the Edinburgh tradition continue to promote the development of explanatory hypotheses (e.g., Collins, 1981: 3ff.; and Collins, this volume; Bloor, 1976; Barnes, 1977; and Barnes, this volume). With the 'weak' programme and the Marxist tradition, the distinctive characteristic is an interest in critique (e.g., Restivo and Zenzen, 1978; Chubin and Restivo, this volume). Thus, these two latter perspectives treat sociological explanation as important only insofar as it leads to and makes possible effective practical action. Their concern with HOW questions, therefore, is dominant in the sense that the ultimate justification for their analysis lies in the practical realm. Analysis is valid when it tells us *how to change the world.*

(3) The inclination to adopt what can loosely be described as a constructivist perspective is characterized by a concern for the processes by which outcomes are brought about through the mundane transactions of participants. It entails the assumption that outcomes

are the result of participants' interactive and interpretative work. Within this perspective, the sociology of knowledge question of the 'social or existential conditioning of thought' is analyzed with a view to the (social) processes which are constitutive of the production and acceptance of knowledge claims. This constructivist approach to the production of scientific culture and action is closely allied to, and dependent on, the detailed microsociological study of scientists' routine practices and discourse. However, although this kind of microsociological approach dominates much recent academic analysis, there are clear signs of an opposing trend in which the interpenetration of science and wider social processes is emphasized (see Chubin and Restivo, this volume).

The 'linguistic turn' in social studies of science

The study of communication among scientists has a history that can be traced back almost to the very beginning of the sociology of science (cf., Edge, 1979). Until very recently, communication in science was conceived primarily in terms of the exchange of scientific products for some form of scientific credit and measured by means of citation and publication counts. This conception of communication treats language as a neutral medium for the transmission of information or as a mere channel through which social transactions take place. In contrast, some recent social studies of science have adopted the view that linguistic utterances are basically speech *actions* (Searle, 1969). Since human interaction to a significant extent consists of such speech actions, they must in themselves become a focus, and for some analysts *the* focus, of investigation.

As a consequence, a series of new questions has been raised about scientific communication. What are the persuasive functions of scientific speech acts and how do speech acts further participants' goals? How do speech acts become organized into orderly sequences of discourse? How are they turned into patterns of argument which appear 'rational' and 'coherent' to participants? Prompted to some extent by developments in literary criticism and by the emergence of a semiotic method of text analysis (Greimas), similar questions have been asked about scientific texts. Several distinctive, yet overlapping, approaches to the discourse of scientists can at present be differentiated: (1) the model of literary inscription' formulated by Latour and Woolgar (1979); (2) the analysis of the practical reasoning of scientists outlined below;

and (3) the 'discourse analysis' of Mulkay and Gilbert (1982b; see also Mulkay, Potter and Yearley, this volume).

(1) Latour and Woolgar's conception of scientific work as essentially a form of writing comes closest to the semiotic approach to discourse. These authors portray scientific activities as 'the organization of persuasion through literary inscription' (1979: 88). The process is depicted as a struggle for the transformation of conjectural statements which are linguistically qualified into statements of 'fact' formulated without linguistic qualification. Within this approach, the notion of 'writing' or of 'literary inscription' is a central part of the theoretical framework which guides the analysis. It is used to explicate scientists' preoccupations and to account for the scientific laboratory as an instrument of persuasion.

(2) The second approach distinguished above shares with the former an interest in the analysis of scientific writings. However, it equally emphasizes that scientists' informal practical' reasoning is a process within which writing is embedded and through which meaning and significance is attributed to scientists' literary inscriptions. The organizational properties of scientists' talk and texts, the negotiation of meaning in scientific conversations, and the strategies of persuasion employed in scientific discourse are topics of investigation. Most analysts draw upon the practical reasoning of scientists both as a topic of analysis and as a resource in describing and accounting for scientific practice (e.g., Lynch, 1982; Woolgar, 1980; Knorr-Cetina, 1981; Law and Williams, 1982).

(3) The third approach mentioned above is perhaps the most radical in that it attributes absolute priority to the study of the organization of meaning in scientific discourse (e.g., Mulkay and Gilbert, 1982a and 1982b; Mulkay, Potter and Yearley, this volume). It treats the discourse of scientists as a topic of analysis, but objects to its use by the analyst as a resource to describe and explain action and belief. This concern with discourse rather than action is said to follow from the interpretative flexibility of scientists' accounts, from the variation of accounts between social contexts, and from the difficulties facing the analyst in his attempts to extract a coherent version of participants' actions and beliefs from the diverse interpretations generated by these same participants. Clearly, generalizations about scientific practice derived from scientists' accounts are only as dependable, as precise and as valid as the accounting practices on which they are based. The authors conclude that instead of relying on scientists' accounts as indicators of scientific practice, we first have to improve our understanding of these accounting practices. The claim is made that, not only is discourse analysis a methodological priority, but also that it makes most traditional forms of analysis redundant.

Systematic inquiries into the properties of scientists' talk and writing partly overlap with ethnographic studies of scientific work (cf., Latour and Woolgar, 1979, Knorr-Cetina, 1981; Law and Williams 1982; Lynch, 1982). Though a wealth of material has already been accumulated (see Mulkay, Potter and Yearley, this volume), both perspectives are among the most recent developments in social studies of science. The linguistic turn in science studies reflects an awakening to the role of language which sociology in general has only recently experienced. Since the production of discourse which purports to have a systematic relationship to scientific work is such a crucial part of the scientific enterprise, its study promises to be particularly critical to the study of science as a social phenomenon.

The breakdown of received distinctions

Traditional sociology of knowledge and the received sociology of science have tended to consider the social side of science as something that exists apart from and in addition to the technical core of science. In keeping with everyday usage of the terms 'social' and 'scientific', some perspectives have implicitly *defined* the social element in science by contrasting it with the technical component. In general, it appears that the more recent studies of science have looked closely at the 'technical' work of science, the more thoroughly social an accomplishment it has turned out to be. The social has come increasingly to appear to be integral to the cognitive and technical, and the latter has come to seem to display those characteristics which have traditionally been attributed to social phenomena.

Studies of scientific 'reasoning' in the laboratory, during controversies, and generally on occasions when scientists communicate with each other, tend to document the negotiated or socially accomplished character of technical outcomes. Whether it is the nature of the things one 'sees' in scientific observation, the proper conduct of an experiment, or the adequacy of a theoretical interpretation, scientific agreement appears to be open to contestation and modification, a process often referred to as 'negotiation'. Through contestation and modification, the meaning of scientific observations as well as of theoretical interpretations tends to get selectively constructed and reconstructed in scientific practice.

The negotiability of scientific outcomes is seen as arising from the underdetermination of theoretical interpretations by the evidence which lias been discussed above. Recent studies of scientific practice have therefore considered the negotiation of technical outcomes as a social process. This assumes that the negotiation of scientific outcomes cannot be fully understood as merely a necessary step in the process of dispelling initial uncertainties about the validity of results. It entails that scientific outcomes may depend on the argumentative skills, the prestige or other symbolic and material resources which participants mobilize to convince each other. If there exists an unavoidable indeterminacy in principle in relation to scientific decisions, then it may be that the rhetorical brilliance of those advocating a particular outcome, the political saliency of the findings, or the support proponents can draw on, etc., may tip the balance in favour of a specific choice. The negotiation of scientific reality documented in internalist studies of science is presented as a process of persuasion which involves, among other things, subtle linguistic strategies of argumentation. Persuasion can be considered to be a basically social phenomenon. If agreement in science is founded upon processes of persuasion and dissuasion, as several of the perspectives outlined below maintain, both the attainment and the dissolution of technical consensus in science can be taken to be social accomplishments.

A related sense in wliich the core of technical scientific inquiry is held to be 'social' refers to the contextual contingencies manifest in scientific work. Internalist studies of science describe scientific inquiry as a process in which decisions are occasioned in part by the local circumstances of the work. They have pointed out the logic of circumstances according to which scientists' decision criteria vary with the situation, and they have documented the idiosyncrasies of the local interpretations on wliich scientists draw to give meaning to seemingly universal conclusions. Contextual conditions nearly always involve 'social[1] factors and may have social explanations.

Reasoned decisions, then, are treated as decisions in which the variables selected for consideration and the values attributed to these variables depend on the specific situation. They are rooted in definitions of the situation which are partly constituted by the perceptions and beliefs of other participants in the situation, and which take into account the crystallized outcome of previous social action. Situational variables, of course, often originate in the larger social context or have other social explanations. A scientific decision itself may at the same time be a social strategy. There are a multitude of ways in which what

has traditionally been called the 'social' appears to be part of the contextually contingent logic of technical inquiry.

At the same time, there is a sense in which the very distinction between 'social' and 'technical' is produced through scientists' own interpretative practices. It has been argued that practitioners (as well as social scientists) make reference to social factors in connection with scientific results which have not yet been fully established, but tend to exempt these results from social explanation when they have become generally accepted (Bloor, 1976; Latour and Woolgar, 1979). The transition from a scientist's knowledge *claim* to a taken-for-granted *object* in the real world is accompanied by a break in participants' use of social explanations (Mulkay and Gilbert, 1982b). For participants, established scientific results become part of an independent, technical realm, in relation to which 'social' factors have no explanatory relevance. As a result, as scientists continually revise their conceptions of the natural world, so they reinterpret the nature of their own and their colleagues' past and present actions (Gilbert and Mulkay, forthcoming). In the past, sociologists have tended to be dependent for their own conclusions on interpretative work of this kind, carried out by scientists. Only recently have sociologists begun to examine empirically practitioners' use of the distinction between the social and the scientific. However, one of the central tendencies in recent work has been to try to avoid adopting participants' folk sociology and to try to concentrate analytically instead on describing how participants construct and deconstruct the technical substance of the natural world along with their own social world of science.

Conclusion

Social studies of science today reflect tendencies in sociology in general, such as the increasing awareness of the relevance of language to sociological investigation, the movė towards a sensitive methodology which can cope with the concrete course of human conduct, and the rejection of theorizing wliich is detached from close empirical study of the complexities of social action. In the most general sense, we can perhaps speak of an influx of the concerns of microsocial theory and methodology into science studies. This is accompanied in some cases by a pronounced concern to link the detailed study of scientists' actions and culture to an understanding of the overall structure of modern societies, and by a growing concern with the practical implications of our own knowledge.

These concerns have far-reaching consequences for the study of science. They have helped to revive and revitalize the daring questions posed by the classical sociology of knowledge, for which they promise to provide a new empirical foundation, as well as to generate questions and findings which have no precedents in prior sociological study of science. In the view of some authors, they have opened up the prospect of an epistemologically relevant sociology of science (Campbell, 1977). On the preceding pages we have drawn attention to some of the pertinent characteristics of these new developments in the sociology of science. But we have hardly done them justice. Readers are now invited to sample them for themselves.

Notes

1. See Grunbaum (1960) for a summary and critical discussion of the arguments made by these authors.
2. Ibid., p. 76.
3. These claims have been made most forcefully by Bloor (1976: 12ff.) and Hesse (.1980: 32ff.).
4. See Suppe (1974, particularly the introduction) for a summary and critical philosophical discussion of the arguments of these authors.
5. See Bhaskar (1979) for a more detailed elaboration of the distinction between cpistemic and judgmental relativism.
6. Few authors have actually made these assumptions, as becomes clear upon closer reading of Mannheim (1954) or Feyerabend (1975).
7. This development owes much to 'cognitive' sociology of science which has, for some time, been interested in the relation between and interaction of social and cognitive factors in science (e.g., Whitley, 1972; Weingart, 1976). Present perspectives tend to reject the distinction between social and cognitive factors.
8. Some authors have attempted a mixture of the use of aggregate data and the use of a qualitative methodology oriented towards processes internal to a scientific specialty (see Studer and Chubin, 1980).

References

Barber, B. (1975) 'Toward a New View of the Sociology of Knowledge', pp. 102-16 in L. Cosr (ed.), *The Idea of Social Structure,* New York: Harcourt, Brace, Jovanovich.

Barnes, B. (1974) *Scientific Knowledge and Sociological Theory,* London: Routledge & Kegan Paul.

Barnes, B. (1977) *Interests and the Growth of Knowledge,* London: Routledge & Kegan Paul.

Barnes, B. and S. Shapin (eds) (1979) *Natural Order,* London and Beverly Mills: Sage.

Bhaskar., R. (1979) *The Possibility of Naturalism: A Critique of the Contemporary Human Sciences,* Brighton, Sussex: Harvester Press.

Bloor, D. (1976) *Knowledge and Social Imagery,* London: Routledge & Kegan Paul.

Campbell, D. (1977) 'Descriptive Epistemology: Psychological, Sociological and Evolutionary', William James Lectures, Harvard University (Spring).

Collins, H. (1974) 'The TEA Set: Tacit Knowledge and Scientific Networks', *Science Studies, A:* 165-85.

Collins, H. (1975) 'The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics', *Sociology,* 9: 206-24.

Collins, H. (ed.) (1981) 'Knowledge and Controversy: Studies of Modern Natural Science', *Social Studies of Science,* 11 (1), special issue:

Coser, L. (1968) 'Sociology of Knowledge', pp. 428-35 in D. Sills (ed.), *International Encyclopedia of the Social Sciences,* vol. 8, New York: Macmillan/ Free Press.

Edge, D. (1979) 'Quantitative Measures of Communication in Science: A Critical *Review', History of Science,* 17: 102-34.

Feyerabend, P. (1975) *Against Method,* London: New Left Books.

Garfinkel, II. (1967) *Studies in Ethnomethodology,* Englewood Cliffs, NJ: Prentice-Hall.

Geertz, C. (1973) 'Thick Description: Towards an Interpretative Theory of Culture', pp. 3-20 in *The Interpretation of Cultures,* New York: Basic Books.

Gieryn, T. (1982) 'Durkheim's Sociology of Scientific Knowledge', *Journal of the History of the Behavioral Sciences,* 18: 107-29.

Gilbert, G. N. and Mulkay, M. (forthcoming) *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse,* Cambridge and New York: Cambridge University Press.

Grunbaum, A. (1960) 'The Duhemian Argument', *Philosophy of Science,* 27 (1): 75-87.

Hesse, M. (1980) *Revolutions and Reconstructions in the Philosophy of Science,* Brighton, Sussex: Harvester Press.

Knorr-Cetina, K. D. (1981) *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science,* Oxford: Pergamon Press.

Knorr, K., Krohn, R. and Whitley, R. (eds) (1980) *Tfte Social Process of Scientific Investigation, Sociology' of the Sciences Yearbook,* vol. 4, Boston and Dordrecht: D. Reidel.

Kxohn, R. (1980) 'Toward the Empirical Study of Scientific Practice, introduction', pp. vii—xxv in K. Knorr, R. Krohn and R. Whitley (eds), *The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook,* vol. 4, Boston and Dordrecht: D. Reidel.

Kuhn, T. (1970) *The Structure of Scientific Revolutions,* Chicago: University of Chicago Press (second enlarged edition).

Latotir, B. and Woolgar, S. (1979) *Laboratory Life. The Social Construction of Scientific Facts,* London and Beverly Hills: Sage.

Laudan, L. (1982) 'Overestimating Underdctermination', paper presented at the Philosophy Colloquium, Virginia Polytechnic Institute and State University, Blacksburg, Va. (April).

Law, J. and Williams, R. (1982) 'Putting Facts Together: A Study of Scientific Persuasion', *Social Studies of Science,* 12 (4): 535-58.

Lucacs, G. (1974) *Die Zerstörung der Vernunft,* Bd. 3, Ncuwicd: Luchterhand.

Lynch, M. (1982) *Art and Artifacts in Laboratory Science,* London: Routledge & Kegan Paul.

MacKenzie, D. (1977) 'The Development of Statistical Theory in Britain, 1865-1923: A Historical and Sociological Perspective', PhD dissertation, Edinburgh: University of Edinburgh.

Mannheim, K. (1954) *Ideology and Utopia: An Introduction to the Sociology of Knowledge,* New York: Harcourt Brace and World. (First published in German, 1929-31.)

Mulkay, M. (1979a) *Science and the Sociology of Knowledge,* London: George Allen and Unwin.

Mulkay, M. (1979b) 'Knowledge and Utility: Implications for the Sociology of *Knowledge', Social Studies of Science,* 9: 63-80.

Mulkay, M. and Gilbert, G. N. (1982a) 'What is the Ultimate Question? Some Remarks in Defence of the Analysis of Scientific Discourse', *Social Studies of Science,* 12 (1): 309-19.

Mulkay, M. and Gilbert, G. N. (1982b) 'Accounting for Error: How scientists construct their social world when they account for correct and incorrect belief, *Sociology,* 16 (2): 165-83.

Restivo, S. (1975) 'Toward a Sociology of Objectivity', *Sociological Analysis and Theory,* 5 (2): 155-83.

Restivo, S. (1982) 'The Epistemological Relevance of the Sociology of Science', in D. Campbell and A. Rosenberg (eds), *Proceedings of the Conference on Epistemologically Relevant Internalist Sociology of Science.*

Restivo, S. and Zcnzen, M. (1978) 'A Humanistic Perspective on Science and Society', *Humanity and Society,* 2 (4): 211-36.

Ryle, G. (1971) 'Thinking and Reflecting', pp. 465 - 79 in *Collected Papers,* London: Ilutchinson.

Schcler, M. (1926) *Die Wissensfonnen und die Gesellschaft,* Bern: Francke.

Searle, J. (1969) *Speech Acts,* London: Cambridge University Press.

Shapin, S. (1982) 'History of Science and its Sociological Reconstructions', *History of Science,* 20.

Stehr, N. (1981) 'The Magic Triangle: in Defense of a General Sociology of Knowledge', *Philosophy of the Social Sciences,* 11: 225-9.

Stehr, N. and Meja, V. (1982) 'Zur gegenwärtigen Lage wissenssoziologischer Konzeptionen', pp. 893-946 in V. Meja and N. Stehr (eds), *Streit urn die Wissenssoziologie,* Frankfurt am Main: Suhrkamp.

Studer, K. and Chubin, D. (1980) *Tfie Cancer Mission, Social Contexts of Biomedical Research,* London and Beverly Hills: Sage.

Suppe, F. (1974) *Tite Structure of Scientific TJteories,* Urbana, 111: University of Illinois Press.

Tiaweek, S. (1982) 'Uptime, Downtime, -Spacetime and Power: An Ethnography of the Particle Physics Community in Japan and the US', PhD dissertation, Santa Cruz: University of Califomia.

Weingart, P. (1976) *Wissensproduktion und Soziale Struktur,* Frankfurt am Main: Suhrkamp.

Whitley, R. (1972) 'Black Boxism and the Sociology of Science', pp. 61-92 in P. Halmos (ed.), *TJie Sociology of Science,* Sociological Review Monograph 18, Keele: University of Keele.

Woolgai, S. (1980) 'Discovery: Logic and Sequence in a Scientific Text', pp. 239-68 in K. Knorr, R. Krohn and R. Whitley (eds), *The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook,* vol. 4, Boston and Dordrecht: D. Reidel.

Woolgar, S. (1981) 'Interests and Explanation in the Social Study of Science', *Social Studies of Science,* 11: 365-94.

Young, R. (1977) 'Science *is* Social Relations', *Radical Science Journal* 5: 65-129.