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THE SOCIOLOGY OF SCIENTIFIC KNOWLEDGE: STUDIES OF CONTEMPORARY SCIENCE

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INTRODUCTION

When, in 1975, Joseph Ben-David and Teresa Sullivan reviewed the "Sociology of Science" for this series, they did not need to mention the sociology of scientific knowledge. Just six years later, Ben-David (1981) published a review article with "Sociology of Scientific Knowledge" as the title. These two articles indicate the phenomenal growth of the subdiscipline in recent years. As Ben-David (1981:54) writes, "No paper on recent developments in the sociology of science can ignore the 'revolutionary' circumstances which prevailed in the field during the seventies." As a participant in the development of the subject I can bear witness to both the excitement and disappointment of the period. It was exciting because the sociology of scientific knowledge appeared to promise a kind of sociological perfection. It was a field in which detailed empirical research could have the most profound theoretical consequences. Accessible and self-contained institutions contained the fundamental secrets of certainty and we had only to split these social "atoms" to create the light of understanding. It was immediately disappointing because hardly anybody in the major sociological marketplace, the United States, saw the light; all they felt, as it were, was the draught. And it is recently disappointing because, though the field has only begun to fulfil its potential, disagreements are now taking up more space than substantive contributions, the standard for an

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acceptable theoretical discussion is not uniformly high, and field study design often owes more to local circumstance that to research strategy. Routinization is having worrisome but, I hope, not inevitable consequences.

To allow space for analysis, the content of this review is severely restricted in three ways. First, I look only at studies of contemporary science even though historical studies are a continuous part of the intellectual web of the subject. Second, I restrict myself almost entirely to the period between 1969 and 1981. Thus I do not discuss some recent work, where it is too soon to separate out articles that will be of lasting interest. (The latest debates may be followed in Knorr-Cetina & Mulkay 1982.) Third, I have not attempted an exhaustive review even within these narrow boundaries. Many authors who deserve mention are excluded from this schematic analysis. These restrictions are not too damaging since, as I explain at the beginning of the second section, a number of full reviews covering the excluded areas are already available.

I devote the first part of the review to an analysis of developments in Britain up to the mid 1970s. I want to dispel a serious misconception of the area that may hinder the rapidly increasing transatlantic dialog. In the second section I discuss research themes and strategies in the sociology of contemporary scientific knowledge. In the third section I summarize some interesting findings and hypotheses and point out the wider implications of the field.

SOCIOLOGY OF SCIENTIFIC KNOWLEDGE AND SOCIOLOGY OF SCIENCE

The relationship between the largely American specialty "the sociology of science" (most often associated with Robert Merton) and the largely British specialty "the sociology of scientific knowledge" seems to have been perceived by nearly all participants as one of competition or perhaps opposition. We can understand this in terms of "resistance to innovation" (Barber 1961). This resistance is no doubt aggravated by the (mis)perceived political connotations of the two approaches: Merton's (1942) and Barber's (1952) thinking about the norms of science must be seen in the context of the rise of European totalitarianism, whereas relativism would seem to allow that "anything goes" (Feyerabend 1975). In this discussion, however, I look only at the cognitive relationship between the two areas.

The main themes of the sociology of science are well set out in Ben-David (1975). All the work coming under this heading could be said to turn on the elucidation of the set of normative and other institutional arrangements that enable science—the asking and answering of questions about Nature—to exist and function efficiently. A crucial feature of this program of inquiry is the assumption that the ultimate answers to the questions are Nature's, mankind being only a mediator. Thus the proper institutional prerequisites must obviate the effect of mundane disagreements and biases. There must also be a reward system to encourage the vigorous pursuit of the answers.

This program does not require sociological attention to the content of scientific answers. It might be possible to say something about the direction of scientific inquiry, but the answers become interesting to the sociologist only if they are wholly men's answers rather than Nature's—that is to say, if they are not "properly" a part of scientific knowledge. In the main, the content of scientific knowledge remains a closed book within this enterprise. [See Merton (1945) for a programmatic discussion.] The sociology of scientific knowledge, on the other hand, is concerned precisely with what comes to count as scientific knowledge and how it comes so to count. The crucial phrase here is "comes to count" since no knowledge of what lies hidden beyond human scientific activity is claimed. The strongest variant of this view is often called "relativism" since it assumes neither fixed points in the physical world nor a fixed realm of logic that would compel agreements between unbiased observers or thinkers from radically different cultures. Neither Nature nor Rationality is taken to be a self-evident universal of human culture. Inquiry based in this program concerns how certain views about the physical and mathematical world come to count as correct within a society, rather than how a society can be arranged so that truth will emerge. Writers in the relativist genre often talk of the "social construction" of scientific knowledge.

The differences between the traditional and more recent research programs lie in underlying epistemological assumptions, in the questions asked of society, and in the target of those questions. It is possible to read the development of the sociology of scientific knowledge as a reaction to the traditional sociology of science. During the period under consideration, the American sociological literature either ignored or criticized the sociology of scientific knowledge, and a number of articles critical of the standard normative schema may be found among British and other European writings. However, in explaining the origins of some early articles I will show that the sense of a necessary opposition is false (see also Gieryn 1982; Collins 1982a).

Confining myself to Britain, and including myself and Alex Dolby, who is perhaps more historian than sociologist, there are six contributors to the sociology of scientific knowledge who developed their ideas independently.[1] The other main contributors were institutionally connected to one or more of these six before they produced their first work in the area.

The six divide neatly into two groups of three. Barnes Bloor and I maintained an unambiguous concern with the sociology of knowledge examined from a

[1] I leave Marxist writings out of this brief account, but see MacKenzie (1981a) for an interesting discussion. I should also mention David Edge, who facilitated the growth of the new subject through his founding of the Edinburgh Science Studies Unit and his coeditorship of *Social Studies of Science* (see also Edge 1979).

relativist perspective throughout the 1970s whereas the concerns of Mulkay, Whitley, and Dolby have been less clear-cut. The first part of my thesis is that though the work of the former trio has attracted the most vociferous opposition this work did not grow out of a reaction to the traditional sociology of science but rather out of entirely separate philosophical and anthropological roots. The work of the latter trio does seem to have started in a mood of opposition—perhaps because all three (none of the former) were introduced to the sociology of science in North America during their graduate training: Mulkay at Simon Fraser, Whitley through Belver Griffith at the Annenberg School of Communication, and Dolby at Columbia. The second part of my thesis is that the work that grew out of reaction to the established strictures on the analysis of the content of scientific knowledge, though it may have been effective in clearing ground, did not lead to a sustained program of empirical work in the same way as the philosophically/anthropologically inspired relativist work. Short interviews with the principal actors have made it possible to disentangle these themes.[2]

Six Independent Contributors to the Sociology of Scientific Knowledge

Bloor and Mulkay represent the extremes in what is often mistaken for a unified approach to the subject. Mulkay's work began as a response to Merton's. [Though his book (1979a) contributes to the relativist/symmetrical feature of the program, it largely synthesizes already existing work; the article cited (1979b) was originally intended to be a chapter of the book.] Mulkay's 1969 paper represents the first explicit published move against the "norms of science thesis" and the first call to open up what Whitley later referred to as the "black box" of scientific knowledge. Reading backwards it would seem that this paper was the first to demand a sociology of scientific knowledge. This is not really true. Close examination reveals that this paper is less a beginning to the sociology of scientific knowledge than it is, as Mulkay puts it, a response to Merton in the light of Kuhn's work.

In the paper Mulkay claims that *technical and cognitive norms* have a greater salience and greater explanatory power than Merton's (1942) Communism, Universalism, Disinterestedness, and Organized Scepticism. The paper opens up scientific knowledge to analysis by discussing constraints on scientific innovation and the circumstances under which innovation occurs in spite of them. The constraints and pressures toward conformity in normal science are exemplified in a discussion of the treatment of Velikovsky's heterodox claims

[2]I do not consider this part of the paper to be a contribution to the sociology of knowledge. Here I intend merely to clarify certain themes and explain their immediate origins in the work of selected scholars.

about the history of the solar system. Mulkay suggests that innovations result either from expansion of inquiry into areas of ignorance—an idea attributed to Holton—or from cross-fertilization of ideas between fields.

Scientific ideas themselves are then given complete autonomy in Mulkay's analysis. Innovation is accelerated or constrained as a result of the salience of anomalies, the exhaustion of a line of research, the opening out of previously unexplored territory, and the interplay of one set of ideas upon another as facilitated by social organization. Mulkay neither analyzes the substantive content of scientific ideas nor indicates a mode of analysis. In reacting to orthodox sociology of science Mulkay addressed precisely the set of problems presented by the existing discipline, though he offered an alternative solution in terms of "technical norms."

The themes of all Mulkay's work on science, apart from his book (1979a) and his recent work on "discourse analysis" (Knorr-Cetina & Mulkay 1982) are to be found in this first paper.[3]

Bloor's (1973) paper, on the other hand, represents an unambiguous beginning to the relativist approach. This paper grew out of a training in philosophy and mathematics and Bloor's acquaintance with Lakatos's work. Bloor (1973, 1976) gave the relativist view operational equivalents in two tenets of his "strong program." He argued that the sociologist should analyze theories *symmetrically* and *impartially* irrespective of their perceived truth or rationality. The strong program is undoubtedly the most widely quoted symbol of the new area. Neither in its genesis nor in its substantive concerns is Bloor's work a reaction to the sociology of science; it is an extension and application of the ideas of Lakatos and Wittgenstein.

These two papers exemplify the thesis. Brief discussion of the remaining four independent contributors to the sociology of knowledge supports it further.

Whitley's most important early (1972) paper was on "Black Boxism." Like Mulkay, Whitley demanded that scientific knowledge be opened up to examination. In the current sociology of science, he argued, production of scientific knowledge is treated as a "black-box," of which only the inputs and outputs can be studied. He suggested that a sociology of scientific knowledge will have to open up the box, at least to some extent. This will require the development of some epistemological theory, since different epistemologies will give rise to different sociologies of knowledge.

Though Whitley's interest in epistemology is clear, this paper is again primarily a reaction against the prevailing strictures on the examination of the content of scientific knowledge. Whitley remarks that since Mertonian ideas had dominated sociological discussion of science up to that time, new work had to react to it. In hindsight the reactive theme in Whitley's paper clearly

[3]It is worth noting in this context that in a review published as late as 1977, Mulkay did not treat the sociology of scientific knowledge as a significant area for discussion.

outweighs the prescriptive. Apart from the demand for self-conscious (nonrelativist) epistemology, no outline or empirically supportable program for a sociology of scientific knowledge is given.

Whitley's later papers (e.g. 1976, 1978), though still lacking satisfactory empirical exemplification, do point in a valuable new direction—toward comparative interdisciplinary analysis emphasizing the relationship between the organizational structure and the cognitive structure of the sciences. These ideas, however, emerged from his experiences during his empirical studies of communication. The various sciences appeared to be radically different social systems. He describes this aspect of his work as "anti-Kuhnian" since he interprets Kuhn as presenting a monolithic picture of science.[4]

As for myself, my first paper (1974) grew out of an attempt to apply Kuhnian ideas to the study of scientific communication. I had read Kuhn (mistakenly) as an application of Wittgenstein's (1953) notion of "form-of-life" to science. Interpretivist sociology was also an influence. In subsequent work (e.g. 1975) I applied the resulting view of communication to an analysis of the process of replication of scientific results (see below). This work owed nothing to the sociology of science and used the relativist/symmetrical viewpoint throughout.

Dolby's work on the interplay of controversy and consensus in the survival of knowledge-claims seems to adopt a broadly symmetrical perspective. However, he does maintain a boundary criterion for identification of nonrational belief systems—namely, immunity to change (Dolby 1974). His first publication in the general area was the anti-Mertonian paper (1970) coauthored with Barnes and aptly titled "The Scientific Ethos: A Deviant Viewpoint." This collaboration came about because Barnes and Dolby found they had already written broadly similar papers independently and decided to combine their ideas. Dolby claims he wrote his contribution as a reaction against the teaching he encountered at Columbia. The rest of Dolby's work seems to have been influenced not by the sociology of science but by (a misreading of?) Kuhn.

Barnes is the most complicated case. Educated outside North America, he is a mainstream contributor to the sociology of scientific knowledge, yet he published the unambiguously anti-Mertonian paper with Dolby.

Barnes's ideas developed mainly from anthropology and from the relativistic treatment of ideas encouraged by Bernstein and MacIntyre (on ethics). His concern with the relativity of categorization runs from his 1969 paper in *Man* to his recent (1981) paper in *Philosophy of the Social Sciences*. His (1974) book is an important statement of the goals of the new program. Why then did Barnes (with Dolby) write the 1970 anti-Mertonian paper in the *European Journal of Sociology?*

[4]I have not been able to go into Whitley's relationship with Weingart and Mendelsohn and their joint founding of the *Sociology of the Sciences Yearbook*, nor into the other early papers of Whitley and Weingart on the analysis of the notion of paradigm (e.g. Whitley 1975; Weingart 1974).

The paper was an application of his critique of functionalism, developed during his training as a sociologist at Essex, to a paradigm case of functionalist explanation. Conveniently, since Barnes was then employed at the Science Studies Unit, the Merton paradigm had general relevance for science. This 1970 paper did not further influence Barnes's work. As he put it, he needed never have written it had he had a slightly different professional career. Thus the early antinormative theme and the later anthropological antirationalist theme in Barnes's work have no cognitive relationship.

Conclusion

In the late 1960s and early 1970s in Europe a sequence of papers attacked the standard sociology of science. If not primarily opposed to the normative approach, many of the papers written at about that time contained at least a few critical introductory sentences. This may have given the impression that the predominant motivation of European work in the sociology of science was a reaction to the traditional form of the subject. However, the most distinctive and, on current evidence, most sustainable theme in European work—the broadly relativist/symmetrical sociology of scientific knowledge did not derive from a reaction against existing analyses. When the relativists did criticize tradition they did so to distinguish their emerging field from an orthodoxy with which they felt it in danger of being confused, rather than as a springboard for new ideas.

I have argued that mainstream sociology of science played no positive part in generating the new sociology of scientific knowledge. The relationship between the two fields is not evolutionary; it is rather one of cognitive tangentiality with (for the unfortunate reasons I have described) an admixture of academic antagonism. If the antagonism can be removed, the two subdisciplines may profit from their points of contact.

But before I leave the matter, and in case the reader believes I have overstated the tenuousness of the cognitive contact between the schools, let me report the results of a small survey of the *Social Science Citation Index*. Between 1971 and 1981, review and discussion articles aside, not a single mention of a finding of the relativist/symmetrical British sociology of scientific knowledge is to be found in any journal article published in America by a recognized American sociologist of science. In terms of real *use*, neither the ideas nor the findings of the sociology of scientific knowledge have made so much as a ripple in the published mainstream of American sociological work.

THE NEW PROGRAM

So far I have discussed the relationship between the sociology of scientific knowledge and the traditional sociology of science. Two other disciplines are closely related: the philosophy of science and the history of science. Apart from philosophers such as Lakatos (Lakatos & Musgrave 1970) and Feyerabend (1975), whose work has tended to weaken the bastions of rationalist philosophy of science, most philosophers have been moved to defend the concept of rational progress in science against the relativist "threat." The resulting tension has resulted in some lively debates but little progress. [See for example Laudan's (1977) *Progress and Its Problems,* the (1979) review by Barnes, and the continuing debate between Bloor and Laudan—e.g. *Philosophy of the Social Sciences,* volume 11 (1981) and onwards.]

In historians of science, however, the sociology of scientific knowledge has found its most appreciative audience—though to think of historians of science as an "audience" is to miss the character of the relationship. Most sociologists have used historical materials as a resource, and some early historical work has been an inspiration (see Dolby 1971 for a discussion). As I have said, there is no distinguishable "seam" between the work of relativist historians and sociologists (even though differences of style and emphasis can be found). Thus a proper description of the development of the sociology of scientific knowledge would treat the history of science as integral. Fortunately, the large body of relevant historical writings has recently been reviewed most adequately by Shapin (1982).

In addition to Shapin, a number of other recent reviews allow me to concentrate here on schematic analysis. Mulkay's (1979a) text, though it has a few infelicities of emphasis,[5] captures well the flavor of the main developments. His (1981) article, a fairly full review of recent literature, includes a 340-item annotated bibliography. Finally, Barnes & Edge include a 400-item bibliography in their (1982) collection of readings.

Above I introduced *en passant* some of the central ideas of, and influences on, the new program. The program's endorsement of relativism means that it must seek to explain the content of scientific knowledge as far as possible in social terms. *Rationality* (whatever that means) must play little part in explaining how the world comes to appear as it does. Thus beliefs that seem less rational should be explained in the same way as those that seem more rational. Relativism is thus translated into symmetry and impartiality, to use Bloor's terms (1973, 1976). Bloor's work made explicit Wittgenstein's relevance for the program. Barnes (1974) wrote at length on the irrelevance of the notion of rationality, bringing the debates within anthropology into the study of science. In a later paper, Barnes & Law (1976) showed that Lakatos's analyses of the theory of polyhedra can be seen as revealing the "indexical qualities" (Garfinkel 1967) of even mathematical expressions. Thus if mathematics could be

[5]For example he mistakes the role of Mitroff's (1974) book. It is a brilliantly conceived and executed field study whose individualistic analytic emphasis makes it less pioneering than it might have been.

analyzed ethnomethodologically, ethnomethodology had a bearing on the sociology of scientific knowledge, and mathematics was a proper part of its subject matter. Law had also demanded an interpretative sociology of science in a paper coauthored with French (1974). Wittgensteinian ideas and phenomenological/ethnomethodological ideas were used by me in my analyses of fieldwork on laser scientists (Collins 1974) and the detection of gravitational radiation (Collins 1975). A broadly interpretative approach was taken by Woolgar in his discussion (1976) of the notion of discovery. Thus all the main ingredients had been stirred into the cake by the mid-1970s.[6]

Below, I review the alternative strategies, explicit and implicit, guiding the substantial program of empirical research that coincided with or followed this period of vigorous theoretical development.

ALTERNATIVE STRATEGIES

Core-Set Studies

A number of differences of emphasis are to be found in the field. First, there are two types of studies that elucidate the general mechanisms involved in the production of scientific knowledge.

The first concentrates on demonstrating that the formal "algorithms" of science, such as the methods for proper control and performance of experiments and their replication, do not fully explain the outcomes of passages of research. These formal methods do not have the potential to resolve differences of opinion over what is a proper addition to scientific knowledge and cannot "close down" scientific controversies. Studies with this first aim in view can be seen as the sociological, empirical, counterpart of what has become known as the "Duhem-Quine-Hesse" thesis in the philosophy of science (Hesse 1974).

ANALYSES OF SCIENTIFIC METHOD. A study with this sort of aim in mind is my own comparative study of experimental physics. In an analysis of scientists' attempts to build copies of a "TEA laser" (1974) I discussed the import-

[6]Alternative accounts might emphasize the influence of Kuhn (1962). This would be especially appropriate in a review of social history, since the work of social historians, even where it is very like or perhaps anticipates some of the work described here (e.g. see Farley & Geison 1974), seems not to have been influenced by the philosophical and anthropological writings discussed above. Though Kuhn certainly provided the intellectual mood for some European developments (perhaps unwillingly—see Pinch 1982), his ideas were not developed in sufficient detail to give rise to an empirical research program. A more appropriate forerunner would be Fleck (1935), but his work was largely unknown to British and American scholars until its recent translation. In addition, both Polanyi (e.g. 1958) and Ravetz (1971) discussed the significance of "tacit knowledge" in the "craft" of science, and their influence should not be underestimated.

ance of skills in experimental science. Since skills are invisible in their transmission and possession it is only possible to discover if a scientist has the skills required to do an experiment properly by trial and error. This can only work where the nature of error and success in an experiment are clearly defined. In "normal" uncontroversial science trial and error is adequate, but when the range of correct outcomes of an experiment is not known in advance—as in controversial science—there is no straightforward way of determining if an experiment has been carried out competently. For example, in the case of experiments concerned with the detection of high fluxes of gravitational radiation (Collins 1975), some scientists believed that only experiments that registered such fluxes were competently performed, whereas others believed that only experiments that failed to detect the radiation were competent. Further replication of the experiment cannot in itself settle the issue. Each new experiment is open to interpretation as either competently or incompetently performed, according to the prior view of the observer. Thus the method of replication, in itself, did not and could not close down debate over the existence of high fluxes of gravitational radiation.

Other aspects of scientific method that have been reanalyzed in this way are mathematical (Pinch 1977) and experimental "disproofs" (Wynne 1976; Collins 1976) and the meaning of experimental "control" (Travis 1981; Collins & Pinch 1982). Analyses of the use of calibration as a way of trying to limit the interpretability of experimental results are in progress.

CLOSURE OF DEBATES The second type of study concerned with general mechanisms looks at the way scientific debates—potentially limitless, as we have seen—are actually closed in practice. Scientific arguments take place within a context of scientific culture that prevents controversy from becoming anarchy. In a study of arguments and experiments concerned with the supposed discovery of a magnetic monopole, Pickering (1981) suggested that scientists act to preserve the maximum number of already existing agreements on the proper interpretation of experimental results. In that case, preservation of such agreements required that the experimental evidence should not be interpreted as demonstrating the existence of the monopole. Prior agreements about monopole experiments were not decisive, of course, since the monopole experiments were new, without a history of interpretation. Pickering suggested that existing theories link aspects of the monopole experiments to other experiments that are already well-embedded in scientific culture and that do have standard interpretations. Pickering explains the outcome of the monopole debate as the working through of scientists' interests in overturning as few standard interpretations as possible.

In another paper on the theoretical dispute between "charm" and "color" interpretations of the J-Psi subatomic particle, Pickering (1980) explains the eventual triumph of the charm interpretation as the result of alliances forged between charm theorists and a group of mathematicians whose technique could be used for charm but not for the color interpretation.

Pickering's analyses show the details of how interests in the preservation of the scientific culture work indirectly—through a complex and far from obvious theoretical nexus—to effect the outcome of apparently autonomous passages of scientific research. Without doubt the constraining culture of science is also felt more directly. In the case of the gravity wave controversy (Collins 1975) there were heterodox explanations of the different behavior of gravity wave "antennae" that scientists would only discuss in private!

It must not be forgotten, however, that scientific traditions are not entirely inflexible. We now recognize that there have been occasions when standard interpretations have been overturned on a large scale. On a much smaller scale, the meaning of current practice is continually open to reinterpretation (Woolgar 1980)—at least to some extent. In his study of the interpretation of "nonlocality" experiments in quantum physics, Harvey (1981) shows that one scientist's mere statement of intention to test an "implausible" hypothesis made the hypothesis seem considerably more reasonable.

Other constraining mechanisms on scientific debate are far more familiar to the general sociologist. They might usefully be collected together under the catch-all notion of the operation of "power". Favored experimental interpretations have been supported through selective reporting in the professional journals (Travis 1981 on memory transfer; Collins & Pinch 1979 on parapsychology), management both of professional meetings and of the publicizing of scientists' small errors (Collins 1981b on gravitational radiation), magnification of the importance of trivial experiments supporting a popular view (Collins 1976 on parapsychology) and concealment of results that might prove embarrassing (Wynne 1976 on Barkla's "J-Rays"), and many other tactics (Collins & Pinch 1979). All in all, the generation, location, and maintenance of scientific certainty is beginning to be understood.

Studies of contemporary pure science have rarely looked outside the scientific community itself for explanations of the outcomes of controversies. Only in historical studies have the specific outcomes of debates been explained by reference to wider social and political factors. An exemplary collection of papers in this vein is Barnes & Shapin's (1979) *Natural Order*. It includes discussion of the race/I.Q. debate, phrenology, and the history of statistics, among other scientific areas. This difference between contemporaneous and restrospective studies in the use of explanatory resources does not resurrect the old "internalist/externalist" distinction in the study of science. In that context "internalist" carried the implication of internal to the logic of science, so that an explanation in terms of the social organization, or distribution of power, *within* science would still be counted an *externalist* explanation. It may be that some studies in the new sociology of scientific knowledge are internalist, but this is not demonstrated by their lack of attention to events outside of the scientific community. Most of the studies (as I have argued in the introduction to Collins 1981a) are certainly compatible with the sort of explanation of knowledge offered by the historians—i.e. in terms of political and social interests. That a similar exercise for contemporary science is still awaited probably has to do either with an absolute increase in the autonomy of science over time or, as I believe is more likely, with the difficulty of seeing certain sorts of social and political "wood" because the "trees" of modern science surround us so closely.

All the contemporaneous studies so far described used a similar methodology. They are all studies of controversies; the fieldwork comprised primarily depth interviews with those members of the scientific community, often in conflict, who made significant experimental or theoretical contributions to the debate in question. Such groups are usually small—from about 3 upwards. They have been called "core-sets" (Collins 1981c).

The depth interview requires that researchers become familiar with the technical details of the area of science under investigation. The first training of several researchers has been in natural science. Though this in not essential, a level of technical proficiency goes along with the intent to investigate the content of scientific knowledge as opposed to its institutions alone. There is a marked contrast in method here with previous approaches to the sociology of science. Researchers from the ethnomethodological school associated with Harold Garfinkkel (see Lynch 1982) have spent periods of years in formal training, learning the science in question. In other cases (e.g. Collins & Pinch's 1982 study of parapsychology), sociologists have acted as participants in scientific activity under analysis.

Laboratory Studies

The style of the "laboratory studies" is somewhat different. The first of these was carried out by Latour, who spent two years working as a technician in the Salk Institute at La Jolla. This work is reported and analyzed in Latour & Woolgar (1979). In common with more recent laboratory studies (reviewed in Knorr-Cetina 1982), a single location was given intense scrutiny while the network of scientists and other laboratory institutions forming the evaluating audience was not researched in detail. This network only features when the scientists in the laboratory under study try to anticipate outside reactions.

The laboratory studies do not stress technical familiarity with or participation in scientists' work. The style is anthropological. Proximity to the research site is maintained, but so is a degree of "strangeness". It is claimed that only in this way can scientific work be properly observed. Certainly the somewhat "behavioristic" descriptions of the actions of scientists generated by this method are always amusingly countercommonsensical and sometimes revealing.

Among the major contributions of Latour & Woolgar (1979) are their descriptions of how a scientific "fact" is first generated from the day-to-day contingent acts of laboratory life. A series of apparently disconnected acts of measurement are given unity as they are all seen to point to the existence of the same fact—e.g. the existence of a new drug. Eventually the drug takes on a life of its own. It is "split" off from the instrument readouts assembled to constitute it. While the estranged observer would continue to see only instrument readouts, the scientists cease to see them as separate data points and perceive them as manifestations of an external object—the drug. At the same time, it is hinted, the language used in talking about the drug/fact is transformed. The "modalities" change. Forms such as "Johnson suggests that 'x' exists" are translated into "it has been confirmed a number of times that 'x' exists," and finally, when full facticity is reached, into "x can be used as . . ." Researchers may even stop referring to the fact, since it comes to seem a part of common sense.

Another consequence of the behaviorist approach (less desirable it seems to me) is emphasis on odd aspects of scientific life. An early characteristic of the sociology of scientific knowledge was a shift of attention away from the formal face of science—the scientific paper—to the underlying processes of knowledge construction and transmission [what Edge (1979) called the "soft underbelly of science"]. The behaviorist/anthropological approach seems to have rediscovered science as a producer of documents of one sort or another. Latour & Woolgar see the laboratory as a collection of "inscription devices," producing graphs, tables, or whatever. The job of the scientist is to transform the output of these devices into other documents and then into still other documents, culminating in the scientific paper.

It may be that the differences in emphasis between these studies and the studies of core-sets involves the fact that nearly all laboratory studies have been carried out on biological science, whereas nearly all the others have been carried out on physics. Perhaps biology laboratories, unlike physics laboratories, are full of inscription devices. My own view is that the difference is a consequence of the deliberately behaviorist method adopted on the one hand and the interpretivist method (emphasizing not strangeness but a mastery of participants' skills) on the other. It seems to me that the most valuable insights in the laboratory studies have emerged from the analysts' (accidental?) deep understanding of the scientist's world, not from their calculated estrangement. Nevertheless, some interesting work on the published output of scientific life is being done.

Phase and Discipline

Many of the core-set studies have taken episodes of scientific controversy—what we might call "extraordinary science"—as their subject [but cf Collins (1974) and Pickering (1981)]. The laboratory studies have all looked at episodes of relatively "normal" science. The difference is almost certainly not coincidental, since laboratory studies are unlikely to be able to shed much light on the resolution of controversy—something that takes place outside of the single institutional location. One study of contemporary events has examined what is claimed to be a period of potentially "revolutionary" science (Collins & Pinch 1982). If we think of the terrain of the sciences as being split up into "phases" of scientific activity—extraordinary/revolutionary/normal—we can see that the ground is being covered in the various studies, albeit in a patchy and undirected way. A comparative/integrative exercise is urgently needed. The same applies if the terrain of the sciences is split up according to subject matter.

The subject of nearly all the early work was either physics or mathematics, the aim being to counter the criticism that the sociology of scientific knowledge could only be done on areas of soft science, such as fringe science or obviously political topics. It was argued that development of the hard sciences was fully determined by "internal" criteria of practice and rationality. Thus physics and mathematics are the "hard case" for the relativist approach.

Bloor's early paper (1973) and his book (1976) both use mathematics as an anvil on which to hammer out the argument for a relativistic sociology of knowledge. Bloor draws extensively on Wittgenstein (1956) and Lakatos (1963). Barnes & Law (1976) also use a mathematical study to demonstrate the applicability of ethnomethodological ideas. Pinch (1977) examines the role of mathematics in closing physical debates by looking at the way a (faulty) proof by von Neumann was used to suppress certain interpretations of quantum theory for 30 years. MacKenzie (1981b) looks at developments in statistics. [See also Bloor (1978) for an anthropological treatment of Lakatos (1963).] The work on physics has been discussed above.

More recently we have seen a number of studies of life-related sciences. As has been pointed out, the findings are intriguingly different, but it is not completely clear whether this is due to different methods or to differences in the subject matter. Initial indications are that it is a mixture of both. Comparative analyses even of existing work promise to be interesting.

Fringe Science

One of the most interesting comparisons is between orthodox and fringe sciences. Work on fringe sciences has uncovered features remarkable similar to those typical of controversy in the hardest areas of science, though in the former the attempts to engineer the credibility of claims to knowledge are less well disguised. The ferocity of argument in fringe science areas ensures that nearly every "negotiating" tactic is available for public inspection.

Studies of fringe science play a dual role in the sociology of scientific knowledge. Because of the transparency of argument and tactic in fringe science it is an easy case for study. But it is also a "soft" case since the relative lack of agreement over theories, facts, and proper experimental practice does not allow for confident generalization about other areas of science. Nevertheless, since the study of fringe science is so easy, a good tactic for the researcher is to shift between a fringe area and a hard area. The former, interesting in itself, provides a valuable source of ideas for harder tests. (Alex Dolby, in conversation, has called this "tacking".)

While fringe and pseudo-sciences may be soft and easy cases for study in some respects they are difficult areas in which to apply the principles of symmetry and impartiality—the operational core of the relativist program. The apparently "crazy" ideas of fringe scientists must be treated on a par with the competing prestigious established ideas. The point of the relativist heuristic is made exceptionally clear to practitioners when they try to analyze fringe science work, for it is in just such areas that ethnocentric analytical lethargy has its greatest attractions (Collins 1982a; Gieryn 1982).

Three recent collections contain publications dealing with fringe science. Probably the most well-known and useful is that edited by Wallis (1979). The others were edited by Nowotny & Rose (1980) and Duerr (1982). The excellent *Zetetic Scholar,* a journal edited and published by Marcello Truzzi of the Sociology Department of Eastern Michigan University, contains data and frequent articles of sociological interest on the fringe science area. A number of researchers have examined the way fringe science is prevented from crossing the boundary into "legitimate" science. Some of the ideas in these studies follow naturally from earlier work on resistance to scientific discovery. For example, the paper by Barber (1961) has formed a jumping-off point for some of this work. Dolby (1980) has "tacked" among controversies over the artifactual nature of electron microscope observations, the case of Kammerer's apparently fraudulent observations of inherited acquired traits in frogs, and the early history of the kinetic theory of gases.

I have already referred to my own (1976) discussion of replication in experiments concerned with emotional responses of plants and with psychic phenomena controlled by quantum random-number generators. With Pinch I have published on "spoon-bending" (1982) and on how parapsychologists have sought recognition as legitimate scientists while their critics have sought to prevent it (Collins & Pinch 1979). We found that the objectification of scientific knowledge is partly a product of conventional practices in journals and the other institutions of science. Special treatment in these places—such as an inappropriately nonuniversalistic discussion within the "constitutive forum" of discussion—presents the subject matter as not worthy of serious consideration.

Before leaving the subject of deviant science I want to draw attention to a series of interesting papers by Westrum. Westrum does not write from an explicitly relativist perspective, but his work is nevertheless exactly compatible with the relativist studies. Westrum has studied the way observational information collected by lay persons—particularly about sea-serpents (1979), meteorites (1978), and Unidentified Flying Objects (1977)—is handled by the scientific community. Westrum shows how such information can be processed to render it "incredible" and of no scientific value, at least until such time as the corresponding science becomes accepted by the scientific establishment. For example, "incredible" reports of stones falling from the sky have become acceptable as observations of meteorites. The "same" reports change from nonsense to truth as a function of the receptivity of the established scientific community.

SUMMARY AND CONCLUSION

Throughout the 1970s the approach described in the previous pages has been the subject of philosophical criticism. It is argued that if claims to knowledge are socially negotiated, then this claim itself is only the outcome of social negotiation and need not be taken seriously; in a word, the whole subject is self-refuting. Though it is possible to counter this view, and several counter attacks have been mounted, the real question is whether the studies themselves have produced enough compelling findings to render such an abstract critique otiose. The stridency of this criticism seems to be decreasing as philosophers begin to find useful ways of using the material.

Recently there have also been some internal methodological critiques. It has been suggested that the account of science emerging from the core-set case studies is an artifact generated by recording only one set of the utterances of scientists. Scientists also have an entirely different vocabulary for describing the same events, it is claimed. This could be a problem were the studies atheoretical and behavioristic, resting entirely on recorded utterances. Since they are not, this critique does not appear sustainable. The debate may be followed in Knorr-Cetina & Mulkay (1982). Again, the decisive argument will be the extent to which findings of the new sociology of science are interesting and useful. In this final section of the paper, I try to summarize and draw out a few results, hypotheses, and suggestions for research without going back over the details of individual case studies.

One of the more decisive results for earlier sociology of science, but one that confirms the Duhem-Quine-Hesse philosophical view, is that science does not have a set of methodological techniques that can quickly or decisively prove or disprove the existence of natural phenomena. A previously unanticipated part of this result is that replicability of results does not establish a firm link between theory and observation. Likewise, replicability cannot act as a hidden policeman to enforce the normative structure of science. This role for replication does seem to have been assumed by the "Mertonian school". The result emerges, surprisingly, from the working out of the implications of the notion of "tacit knowledge" for experimental work in controversial areas. Other elements of scientific method also turn out to be problematic. Further, technical arguments have been found to be limited by cultural constraints and the distribution of power, rather than "internal" technical knowledge or logical possibility.

Surprising conclusions also arise from the view that knowledge construction depends on the *form* of descriptions of laboratory activity. If the "same" passage of activity when described one way creates facticity but when described another way dissolves facticity (Latour & Woolgar 1979), the astute critic of a claim to knowledge need do no more than honestly redescribe an experiment in all its contingent detail to dissolve the scientific potential of the experimental findings (Collins & Pinch 1982). Oddly enough, what this means is that for scientific work to develop facticity in the face of active criticism, the laboratory needs to be a fairly private place. Only then can distanced "scientific" accounts have any chance of predominating.

A further curious corollary is that where findings are not vigorously contested their facticity will seem inviolable to all except those involved in the period of fact creation (the core-set). Contrary to the popular view of the ultimately compelling nature of experiment—a view fostered in training and where spectacular experiments are used for demonstration purposes—those distant from difficult experiments will find the reported results more compelling than do members of the core-set who are aware of the possible redescriptions of their work, aware of the socially mediated nature of the closure of debate, and aware of the potential for reopening it. What is then *unsurprising* is that carefully mounted opposition can reopen debates. This makes the lawyer's ability to unsettle expert witnesses entirely natural, along with the ability of one expert witness to mount a case against another. All that is necessary is to reopen discussion of the tacit, normally private, taken-for-granted features of laboratory life and inter-laboratory debate.

I have suggested that though most criticisms directed at the program of the sociology of scientific knowledge have been answered, its ultimate destiny depends on its usefulness to other researchers and the extent to which it can provide and solve puzzles. To finish, let me suggest likely applications and future directions for the work.

Though the stress on the "hard case" explains why "politically flavored" controversies have not formed the topic of sociology of knowledge case studies, the work does provide a perfect underpinning for analyses of scientific debate within a political setting. A paper by Robbins & Johnston (1976) concerning the environmental lead controversy brings this out nicely, as does a recent interesting study of the estimation of geological reserves (Bowden 1982). Here the concerns for traditional sociology of science and the sociology of knowledge are complementary.

A second role for the subject is in the traditional preserve of the philosophy of science. For example, it may clarify issues of methodology for those less self-confident disciplines—such as sociology—that take the physical sciences as their exemplar. It also illuminates the nature of "expertise". Whereas traditional philosophy of science attempts mainly to provide the abstract formula that justifies our reliance on expertise, the sociology of scientific knowledge explores the way that different, and sometimes competing, bodies of expert knowledge are put to work (see Wynne 1982).

A third vital direction for future research involves comparative studies of the sciences. A rough-and-ready comparison of the case studies already completed suggests that major differences are to be found between different *phases* of science. All controversial science, whether in hard or fringe areas, looks alike in the major respects, and so does all normal science. However, smaller but still important differences are to be found between different scientific disciplines of similar phase. Inter-phase and inter-disciplinary comparisons are vital for an understanding of the nature of science and for their potential value, in the science policy-making process. A proper understanding of the phases of science offers an alternative to short-term utilitarian considerations in science policy.

These comparisons require empirical data on the nature of scientific institutions and the character of formal publication in different fields. Thus given a degree of methodological astringency, differences in underlying "credo" do not give rise to a corresponding "incommensurability" between those findings of traditional sociology of science and those findings of sociology of scientific knowledge relevant to the comparative exercise. These potential academic bridges can form the basis of future cooperation if the sense of a necessary opposition between the traditional and the new can be dispelled. As I argued in the first section of the paper, this impression of opposition is a historical contingency.[7]

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[7]See Studer & Chubin (1980) for a demonstration that the gulf is not unbridgeable.

The following papers referred to in the text are to be found gathered together in Collins (1981a): Travis (1981a), Collins (1981b), Harvey (1981), Pickering (1981), Pinch (1981). The following are reprinted in Barnes & Edge (1982): Collins (1974), Collins (1975), and part of Bloor (1976). Collins (1982b) reprints the following: Farley & Geison (1974), Bloor (1973), Barnes & Law (1976) Woolgar (1976), Shapin (1979), Collins & Pinch (1979), Westrum (1978), and Robbins & Johnston (1976). Latour (1982) reprints the following in French: Farley & Geison (1974), Shapin (1979), Collins (1975), Collins & Pinch (1979). Mulkay (1979a) discusses a number of items, including Wynne (1976), Collins (1974, 1975), Collins & Pinch (1979), and Mitroff (1974), at useful length.

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