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Preface

During the last decade, the sociology of science has undergone a transformation. Not only have traditional perspectives been overthrown, but new forms of analysis have been proposed, new kinds of empirical study have been undertaken, new social groupings have emerged, and increasingly wide-ranging contacts have been established with historians and philosophers. As a result of these intellectual and social changes, the field has often seemed to be in disarray. The old certainties about science, the old belief in its cultural uniqueness and the old landmarks of sociological interpretation have all gone. But it has been far from evident what is to take their place. In my view, the first clear signs of a coherent and viable programme of sociological analysis are now beginning to appear. This book by Gus Brannigan is one of those signs.

The single most important defect of the traditional sociological perspectives on science was that the typical assumptions of scientists themselves were taken for granted by the analysts involved. Thus scientific knowledge was conceived in a straightforward positivist fashion. Scientists' public pronouncements about the values of science were seen as accurately defining the research community's ethical system. Researchers' claim that autonomy was essential for effective knowledge-production was accepted more or less at face value. In other words, scientists' own interpretations of their social world were taken over by sociologists, incorporated into their analytical frameworks and in the process further objectified, that is, made to appear even more 'thinglike' and unquestionable.

In recent years, this objectification of scientists' interpretative achievements by past sociological analysis has begun to be recognized and dismantled in relation to various topics in the sociology of science. Consequently, attempts have been made to devise new and more satisfactory forms of analysis, the aim of which has been increasingly to understand scientists' interpretative practices instead of allowing those practices to dictate the shape of sociological work. Thus, traditionally, sociologists tended to formulate their research topics as fol-

lows: given that science exhibits a certain kind of rationality or a type of moral action or whatever, how can these characteristics of science be explained or their consequences identified? In contrast, those working within the newer perspective tend to ask: given that scientists' actions and beliefs can be construed in various ways, by what interpretative practices is science made to appear to exemplify a certain kind of rationality or a type of moral action or whatever?

Until the publication of the present book, however, this kind of revision of traditional questions has not been attempted in relation to the topic of discovery, with the exception of a single paper by Woolgar (see chapter 5 below). This is surprising because, as Brannigan points out, the notion of discovery is prominent in our conception of science. Thus this book fills an important gap in the current reconceptualization of the sociology of science. Moreover, because there is virtually no earlier work on which to build, the author has had to do it all himself more or less from scratch. Accordingly, he presents a systematic critique of existing theories of discovery. He erects on this critical evaluation an alternative analytical perspective. He illustrates the fruitfulness of this perspective in the interpretation of specific examples of discovery. He offers a highly original account of the origins of existing theories. And he identifies some of the lines of research which are prompted by his 'attributional theory of discovery'.

Brannigan suggests that previous theories of discovery are of two types: those which rely on mentalist notions, such as 'genius' or 'gestalt switch', and those which employ a conception of 'cultural determination'. These two kinds of theory have in common the assumption that discoveries are naturally occurring events which can be identified by the analyst without too much trouble and the assumption that, once identified, discoveries can be explained by linking them to some preceding event. Brannigan challenges both these assumptions. He demonstrates that discoveries as such are not naturally occurring events. They are 'events' whose status as discoveries is variable and dependent on the contingent interpretations of participants. Previous analysts, in identifying discoveries, have either adopted these lay interpretations ready-made or have themselves engaged in the same kind of contingent interpretation as they sort through conflicting versions of events to achieve a 'correct categorization'. Brannigan also shows clearly that the attempts at explanation are uniformly unsuccessful and that the theories are, on the whole, analytically empty. In many cases, they are mere tautologies. For instance, the only evidence of 'genius', as distinct from high intelligence, is usually just that discovery which is to be explained. In addition, these theories are often unable to distin-

guish between discoveries and situations of ordinary learning, that is, situations where an individual comes to understand for the first time some conception which is already widely understood by others.

The author seeks to avoid the inadequacies of traditional theories by focusing on the social context of discovery rather than on the mental event of discovery. He stresses that a theory of discovery must deal specifically with the social category 'discovery'. It must distinguish between discovery and other possible categorizations, such as learning, replication, plagiarism, presentation of the self-evident, fraud, fantasy, and so on. This can only be done by taking into account the social context in which actions and mental experiences occur and as a result of which various categorizations are applied. The theory must also be able to deal with changes in the attribution of categories to particular actions or knowledge-claims; that is, we have to recognize that what are now known as discoveries were not necessarily always so defined and that what were previously said to be discoveries are not always treated as such today. Thus, the theory must come to terms with the fact that the categorization of a scientific contribution as a discovery is an interpretative and variable accomplishment on the part of those actors who create the social contexts in question.

Brannigan concludes, then, that discoveries do not occur as discrete, causally explicable, events. 'Discoveries' are rather context-dependent categorizations achieved by participants in pursuit of their own practical objectives and 'explicable', for those practical purposes, in terms of participants' own lay theories. Traditional theorists have made the mistake, not only of treating participants' contingent identifications of discoveries as literal descriptions, but they have also taken over scientists' lay theories. Unfortunately, as Brannigan shows, these folk theories, although they are entirely appropriate for and satisfactory within the requirements of everyday informal scientific discourse, are quite unsuitable for the purposes of systematic sociological analysis.

In the central chapters of this book, Brannigan demonstrates how sociologists can begin to take as their topic for analysis scientists' theories about, and procedures for constituting, discoveries. He shows how, instead of taking discoveries for granted and trying to account for them, we can pursue the prior issue of how certain events are made out to be discoveries in the first place. His procedure is to explore the common sense meaning of 'discovery' and to identify the basic criteria through which discoveries are defined, recognized and constituted by participants. He identifies criteria dealing with four aspects of discovery; namely, the feasibility of a knowledge-claim, its validity, the kind of motivation involved, and the degree of originality. It is these ele-

ments of an act, Brannigan suggests, which scientists, and laymen, take into consideration when deciding on the attribution of the term 'discovery'. This interpretative machinery for the constitution of discoveries is then used by the author to illuminate the chequered history of Mendel's claims, the Piltdown Man, and so on, in order to show dynamically how the status of these claims as discoveries depended on members' variable employment of this machinery.

Brannigan himself does not use the phrase 'interpretative machinery'; probably because it might seem to imply, misleadingly, that participants' procedures for construing discoveries are rigidly formalized. However, the mechanical metaphor *is* helpful insofar as it draws attention to the objectification of the interpretations which members of society produce. For once participants construe an act as a discovery, the act comes routinely to be described in terms of the category 'discovery' and to be embedded in lay theories of discovery. To refer to the act at all is to refer to a *discovery* and to draw upon the interpretative vocabulary appropriate to a discovery. The fact that it was a discovery comes to *appear to be* a natural fact of life.

I will not try to convey any more of the content of the book. It is impossible to do it justice here. But it is worth commenting briefly on two of its possible implications. Brannigan shows to my satisfaction that 'discovery' can best be approached, sociologically, as a method or interpretative practice which scientists use in making sense of the events going on around them. His formal criteria can therefore be seen as a set of rules or procedures for construing events as discoveries. However, he also shows clearly that, although members of society appear to employ similar rules, they often reach quite different conclusions about the character of particular knowledge-claims; and he describes in detail how scientists came to divergent conclusions in various specific instances. Furthermore, we know from previous work that the meaning of a set of rules is never self-evident in its application to particular cases and that rules always require further interpretation which cannot be fully specified by the rules themselves. Now it is possible that the varying implementations of the procedures for constituting discoveries are due to purely local, idiosyncratic features. In which case, if we assume that Brannigan's substantive analysis is broadly correct, the sociological study of discovery would have little further to go conceptually. It would consist primarily of various illustrations of how Brannigan's machinery was put into operation on specific occasions. The other possibility, however, is that these 'surface rules' are linked to one or more repertoires of supplementary interpretative procedures, which make possible a range of specific interpretations in any

one case, yet which can be formulated analytically. If this is so, and it seems to me to be the more likely possibility, the author's analysis will have opened up a whole new area of sociological research.

The second issue to which I wish to draw attention is whether it follows from Brannigan's analysis that discovery is a purely linguistic phenomenon. The author himself recognizes the importance of this issue and he states clearly that this conclusion does not follow necessarily from his argument. He writes that his four criteria constitute a set of necessary and sufficient conditions for the definition of an act or a knowledge-claim as a discovery and that 'the criteria and their use explain members' action regarding new theories'. Thus the idea is that, once we have understood how an act comes to be deemed to be a discovery, this will help us to explain why certain kinds of *action* then ensue. The problem, of course, is how can we characterize these ensuing actions without adopting in our treatment of the consequences of discoveries just that form of analysis which has been abandoned in relation to discovery itself? Once the analyst has withdrawn from the analysis of discovery as action, and replaced it with an examination of discovery as a participants' method for construing action, he seems to be obliged to treat all other classes of action in the same way. For there is no realm of social action where it is possible to identify 'what really happened' without treating at least some part of members' interpretations of what happened as analytically unproblematic, literal accounts.

It seems to me, therefore, that the main thrust of Brannigan's study is towards an analysis of 'discourse about action' rather than of 'discourse *and* action'. It also seems to me that this is eminently sensible. For it is scientists' discourse, that is, their documents, recorded utterances and pictorial representations, which is available to us for study. Thus, for me, Brannigan's book is a major step away from the traditional and unattainable objective of using participants' statements as the basis for definitive characterizations and explanations of 'what really happens' in science, to a more realistic concern with the various kinds of accessible text in which are embodied the practices whereby scientists give meaning to their world. Not all readers will want to follow me that far. But even so, few readers will deny that this book significantly revises the sociological approach to the study of discovery and that it leads us in an intriguing new direction.

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