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G. Nigel Gilbert and Michael Mulkay
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Scientists' discourse as a topic

What's in Pandora's Box?

In this book we offer a sociological analysis of material obtained from practitioners in an area of biochemical research. If this were a typical sociological study, we would be using that material in the following chapters to tell the inside story about this area of social life. We would proceed by extracting from our data what we took to be the most coherent and comprehensive version of 'what really happened', and we would present this story to our readers along with persuasive argument and supporting empirical evidence.

Given that we have available a wide range of evidence about developments within this research area, including the transcripts of interviews, letters and other informal material, as well as access to the formal research literature, it is likely that, as sociologists of science, we would try to use our data to show that the area did not develop solely through the reasoned appraisal of objective biochemical evidence; and that a full explanation of its cognitive evolution must make reference to the kind of social, political and personal factors documented in the less formal sources. Having used participants' own informal talk and writings to substantiate these claims, we would probably conclude by showing how this case study is consistent with and contributes to a recent but steadily growing body of sociological and historical literature on the social production of scientific knowledge.[1]

It will be evident, however, that we do not intend to furnish that kind of sociological analysis here. We will not be opening Pandora's Box in order to reveal how various supposedly disreputable, non-cognitive influences are actually at work in the field we have studied. Our reference to Pandora's Box is not a way of referring to a supposed gap between an orthodox view of science and the social realities revealed by sociological research. It is, rather, a way of drawing attention to some methodological and analytical weaknesses in previous sociological work on science. Pandora's Box and its discordant contents are intended as a metaphor for the remarkably diverse accounts of action and belief which appear in our

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material and which are present, we suspect, in most sociologists' data files, but which are normally suppressed as a result of analysts' unreflective commitments to the production of a unitary 'best account' of the areas of social life they have chosen to study.[2] One of our central claims in this book is that sociologists' attempts to tell *the* story of a particular social setting or to formulate *the* way in which social life operates are fundamentally unsatisfactory. Such 'definitive versions' are unsatisfactory because they imply unjustifiably that the analyst can reconcile his version of events with all the multiple and divergent versions generated by the actors themselves.[3]

Most sociological analyses are dominated by the authorial voice of the sociologist. Participants are allowed to speak through the author's text only when they appear to endorse his story. Most sociological research reports are, in this sense, univocal. We believe that this form of presentation grossly misrepresents the participants' discourse. This is not only because different actors often tell radically different stories; but also because each actor has many different voices. In this book, we will begin to lift the lid of Pandora's Box in order to give some of these voices the opportunity of being heard.

In the rest of this chapter, we will develop this argument with respect to science, to show that the goal of constructing definitive analysts' accounts of scientists' actions and beliefs is possibly unattainable in principle, and certainly unattainable in practice as long as we have no systematic understanding of the social production of scientists' discourse. Sociologists, historians and philosophers have been able to document and make plausible so many divergent analyses of science (and continually undermine each other's claims) because scientists, the active creators of analysts' evidence, themselves engage in so many kinds of discourse. Thus we recommend that analysts should no longer seek to force scientists' diverse discourse into one 'authoritative' account of their own. Instead of assuming that there is only one truly accurate version of participants' action and belief which can, sooner or later, be pieced together, analysts need to become more sensitive to interpretative variability among participants and to seek to understand why so many different versions of events can be produced.

We will try to show that, analytically, there is much to be gained by opening Pandora's Box in the sense of setting free the multitude of divergent and conflicting voices with which scientists speak. Of course, the interpretative variability found in scientists' discourse undoubtedly occurs in other areas of social life.[4] Consequently, our attempt in this book to reorient the sociological analysis of science in order to cope with the variability of participants' discourse has obvious parallels with and

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implications for other fields of sociological inquiry. Although it would be distracting if we were continually to draw attention to similarities between the sociological analysis of science and that of other areas of social life, it is important to stress that in this book we begin with analytical and methodological concerns that are in no way peculiar to the sociology of science and that our conclusions about the importance of discourse analysis should apply to any realm of sociological study.

Analysis of social action in science

In this section, we demonstrate why there is a need for a form of sociological analysis which focuses on the organisation of scientists' discourse. Let us start by looking at a study of scientists undertaken about ten years ago by Marlan Blissett.[5] We have chosen to comment on Blissett's analysis because it clearly shows how sociological interpretation of social action typically depends heavily on unexplicated interpretative work carried out by participants and embodied in their discourse. Another reason for choosing this study is that the research network examined by Blissett overlaps considerably with the one with which we are concerned in this book. Differences between Blissett's study and our own are therefore unlikely to be due to major differences in kinds of respondent or kinds of data. They are more probably signs of genuine differences in analytical approach. A brief description of his work will therefore help us to clarify the distinctive features of our approach to analysis.

Blissett focuses on the role of politics in science. His main thesis is that it is a myth that scientists are neutral and disinterested actors when they engage in research. He aims to show that the professional actions of scientists are essentially political in character and that scientists regularly engage in such political manoeuvres as 'marketing, salesmanship and manipulation'. He suggests that these activities are not regrettable and infrequent lapses by otherwise disinterested scientists, but are vital aspects of the process of scientific enquiry.

Such a thesis is not only sociologically interesting, but it is also typical of much sociological analysis in formulating definitive categorisations of participants' actions. Thus Blissett proposes that some actions were political, as distinct from any other type of action. Blissett further claims that such political actions direct scientific perception and influence the acceptance or rejection of specific theories and ideas. Here Blissett also typifies much sociological analysis in suggesting that his categorisations identify stable entities which cause other social phenomena. Furthermore, Blissett asserts that the material from his interviews with biochemists clearly substantiates these claims.

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Blissett's conclusions describe scientists' actions and the social consequences of these actions. However, Blissett's data consist entirely of statements obtained from interviews with scientists or from their written descriptions of the field. In other words, his data are *accounts* of action. We need to examine how Blissett manages to derive conclusions about actions and their consequences from participants' accounts. We can best do this by quoting some examples of his data and analysis.

Blissett begins by noting that research in the area he is studying is pervaded by controversy and that 'the importance of controversies of this nature is that they are unlikely to be resolved by appeal to evidence alone'. This observation is supported by a quotation from an article by one of the contenders in the controversy, who writes that:

> Until a few years ago the conceptual framework in the field [under study] played a relatively minor role in determining the direction and in shaping the design of experimentation. In the phase of describing phenomena, the conceptual framework is not crucial. It is only when experimentation reaches the interpretative and exploratory stages that *permissiveness* or *indifference* with respect to the conceptual framework has consequences which inhibit progress. [Emphasis added][6]

Thus Blissett proposes that the resolution of this controversy depends on more than the scientific evidence and he justifies this claim by quoting a scientist who, in talking of the role of permissiveness and indifference, can be seen to be saying much the same thing.

Blissett organises his analysis to show that a crucial additional factor which helps to determine the outcome of controversy is the effect of political strategies. It was this concern with political action, he states, which led him to select this particular field as one in which interviews might yield valuable data. He writes, 'The prospect of a hard-bitten scientific controversy led to interviews concerning the political nature of the matter with biologists in the field.'[7] During these interviews, Blissett was offered many statements which provide prima-facie evidence in support of his notion that political strategies are important. For example, one of the contenders in the dispute, he says, admitted that the present level of theoretical conflict in biology was unequivocally immersed in the political strategies of personal salesmanship and scientific advertisement. The same scientist is quoted as stating that: 'To make changes you have to be highly articulate, persuasive, and devastating. You have to go to the heart of the matter. But in doing this you lay yourself open to attack. I've been called fanatical, paranoid, obsessed . . . but I'm going to win. Time is on my side.'[8]

Further evidence of a similar nature is provided. For instance, Blissett

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presents another lengthy quotation in which the speaker says, in part:

> However, aside from the technical difficulties that attend his theory, [he] himself must be held responsible for some of his 'selling' problems. Labelled by a colleague of mine as 'insulting', [he] indeed hardly possesses the patience necessary for the presentation of his theories . . . He suffers not so much from a repressive oligarchy bent on his destruction, as from a plurality of opponents, some of whom like the idea of a revised membrane model, but who detest the man who is responsible for its initial formulation.[9]

On the basis of a series of such quotations, in which participants characterise their own and/or others' actions in 'political' terms, Blissett claims to have shown that political action occurs frequently in this area and that it has significant consequences.

We have presented sufficient material in this brief résumé of Blissett's study to be able to draw out the basic elements of the interpretative method he uses. His procedure is to make a claim about scientific action, such as that it is political, and then to confirm this claim by presenting material in which scientists themselves can be seen to be making the same claim. Thus to justify his thesis that political action is involved in the creation of scientific knowledge, Blissett offers passages in which scientists describe their own and others' actions as political. In practice, such passages are not difficult to find; they occur regularly in our own interview transcripts, for example. Then, having shown that descriptions of political activity do often appear in statements made by scientists, Blissett concludes that political action is a fundamental feature of science.

We can set out this form of analysis in a more systematic way as a series of steps. This is worth doing because not only Blissett's but most qualitative studies seem to follow these steps.

(1) Obtain statements by interview or by listening to or observing participants in a natural setting.
(2) Look for broad similarities between the statements.
(3) If there are similarities which occur frequently, take these statements at face value, that is, as accurate accounts of what is really going on.
(4) Construct a generalised version of these participants' accounts of what is going on, and present this as one's own analytical conclusion.

This is not an unreasonable characterisation of Blissett's procedure. He interviewed scientists (1), found numerous statements which dealt with salesmanship, manipulation and the like (2), took it that these statements were accurate reports of the way in which scientists acted (3) and concluded that science is political (4).

Blissett's use of participants' accounts is far from unique. As has been

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shown elsewhere, procedures very similar to that adopted by Blissett are recommended in influential pronouncements on sociological methods and employed in qualitative studies of quite different types of social actors;[10] and a wide range of empirical studies specifically within the sociology of science, quantitative as well as qualitative, have been shown to be as heavily dependent as that of Blissett on interpretative work carried out by participants.[11]

This does not mean, of course, that the analyst does *nothing but* reproduce participants' discourse. Analysts do typically make contributions of at least three kinds. They subsume participants' specific pronouncements under more general concepts. Blissett does this, for example, when he collects together a variety of particular statements referring to manipulation, influence, manoeuvring, and so on, as all about one kind of action, namely political action. At the same time, analysts tend to generalise participants' statements about particular actors or actions to whole classes of social action and to whole groups of actors. Thirdly, analysts identify those segments of participants' discourse which are to be regarded as accurately representing important social processes occurring within the area of social life under study. Other parts of participants' discourse are ignored or treated as inaccurate. Although these three facets of sociological research practice are closely related, we will concentrate on the third component. In the sections which follow, we will show that there are good theoretical as well as practical reasons for doubting whether some sections of participants' discourse can be selected as providing sociologically more satisfactory descriptions of members' action or belief than others.

The context-dependence of participants' discourse

The difficulty with taking any collection of similar statements produced by participants as literally descriptive of social action is the potential variability of participants' statements about any given action. The reasons why we would expect participants' statements to be potentially variable are clearly expressed by Halliday in his discussion of the basic characteristics of language use.

> The ability to control the varieties of one's language that are appropriate to different uses is one of the cornerstones of linguistic success . . . Essentially what this implies is that language comes to life only when functioning in some environment. We do not experience language in isolation . . . but always in relation to a scenario, some background of persons and actions and events from which the things which are said derive their meaning . . . any account of language which fails to build in

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> the situation as an essential ingredient is likely to be artificial and unrewarding . . . *All* language functions in contexts of situation, and is relatable to those contexts. The question is not what peculiarities of vocabulary, or grammar or pronunciation, can be directly accounted for by reference to the situation. It is *which* kinds of situational factor determine *which* kinds of selection in the linguistic system . . .[12]

We do not wish to endorse in detail every aspect of Halliday's treatment of the relationship between linguistic variation and social context. Nevertheless, we take his general claim with respect to the complex interdependence between participants' discourse and its situation of production to be firmly established. If there *is* a strong connection between the form and substance of discourse, on the one hand, and the social situation in which discourse is produced, on the other hand, it follows that discourse can never be taken as simply descriptive of the social action to which it ostensibly refers, no matter how uniform particular segments of that discourse appear to be. For similarities between different statements are just as likely to be the consequence of some similarity in the context of linguistic production as of similarity in the actions described by those statements. For instance, the apparently overwhelming orientation towards political action in Blissett's material may well have been at least partly due to a response by interviewees to unintentional cues provided by the investigator. Without detailed examination of the linguistic exchanges between researcher and participant, and without some kind of informed understanding of the social generation of participants' accounts of action, it is not possible to use these accounts to provide sociologically valuable information about the actions in which analysts like Blissett are interested. It certainly cannot be assumed that marked similarities within such collections of statements indicate the existence of corresponding regularities in social action.

Traditional sociological research, like that exemplified in the previous section, operates according to a methodological principle of linguistic consistency; that is, if a 'sufficient proportion' of participants' accounts appear consistently to tell the same sort of story about a particular aspect of social action, then these accounts are treated as being literally descriptive.[13] Only in those instances where the existence of incompatible accounts is treated as sociologically significant do analysts pay attention to the social generation of accounts; and in such cases, reference to the social or personal context of participants' discourse is usually introduced into the analysis in order to explain away those accounts which weaken the analyst's conclusions, on the grounds that they are exaggerations, biased reports, ideology, lies, and so on.[14] Acceptance of Halliday's argument, however, implies a need to revise such an approach to participants'

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discourse in a fundamental way. For Halliday proposes that there are no literal descriptions available and that all linguistic formulations, indeed all members' symbolic products,[15] have to be understood in relation to their context of production. This proposition clearly implies that the systematic investigation of participants' discourse is methodologically prior to analysts' *use* of such discourse to characterise and explain social action. Even more significantly, it may be that the traditional sociological goal of providing analyses of social life which build upon the interpretations furnished by participants is made unattainable by participants' ability to engage in the creative use of language.

Direct observation and participants' discourse

Proponents of traditional methodologies might respond to the argument so far in one of two ways. In the first place, they might accept that participants' retrospective accounts of action and belief, as obtained for example from interviews, autobiographies, review articles, public lectures, and so on, are highly variable, context-dependent, and therefore unreliable; but they might suggest that it is possible to replace such indirect sources of data with direct observation of social action as it occurs. Some of the recent ethnographies of work in scientific laboratories seem to exemplify this view.[16] The idea is that by observing actions as they take place, the analyst is able to avoid, or at least reduce to an acceptable minimum, any dependence on participants' potentially variable interpretative activities.

Although we have no wish to deny the interest of this kind of observational work, it does not seem in itself to resolve the difficulties identified above. There are several reasons why this is so. First, social action is not 'directly observable'. The observable, physical acts involved in performing an experiment, for example, do not reveal whether the experiment is an attempt to refute an hypothesis, an attempt to find a new way of measuring a known variable, a routine check on the experimental apparatus, and so on. Which of these or other actions is being observed on any particular occasion can only be established by reference to the statements, either written or spoken, of participants. Yet, not only can descriptions of an experiment vary considerably from one scientist to another,[17] but the accounts given of a particular experiment by an individual scientist can, as Hanson and others have shown, vary appreciably.[18] Thus so-called 'direct observation' of social action as it takes place in no way frees the observer from reliance on the potentially variable discourse of participants.

The ability of social actors to characterise a given set of activities in

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various different, and sometimes apparently incompatible, ways becomes understandable if we accept that social activities are the repositories of multiple meanings. For instance, does a given set of activities constitute an experiment, an attempt indirectly to raise more research funds, an effort to secure professional credibility, a bid for more students; or can it be any or all of these, depending on the context in which the actor is talking or writing about his actions? If the latter is the case, and we suggest that it is, then 'the meaning' of his action is variable and context-dependent. It will be quite impossible to establish the nature of the action unequivocally by being present at and directly observing the original laboratory experiment. For the social character of the original laboratory work will continually change as participants interact in different settings and thereby generate different kinds of linguistic gloss upon those initial activities.

It seems best, then, to conceive of the meaning of social action, not as a unitary characteristic of acts which can be observed as they occur, but as a diverse potentiality of acts which can be realised in different ways through participants' production of different interpretations in different social contexts. It is important to recognise that this production of social meanings through language is a temporal process. Actors continually reinterpret given actions as their biography unfolds and as changing circumstances lead them to fit these actions into new social configurations. And the meaning of each new situation is defined in part through participants' reinterpretations of what they have done in the past.[19] Consequently, participants' observable accomplishment of actions at a specific point in time cannot be neatly distinguished from, or separated from, the kind of retrospective story-telling which is generated in interviews and other indirect methods of data collection. The technique of direct observation cannot avoid becoming entangled in members' variable and context-dependent reconstructions of their social world, because this kind of reconstruction is a pervasive feature of the creation of social meaning.

These reflections on the nature of direct observation thus serve only to strengthen our previous argument for the methodological priority of analysis of participants' discourse. However, exponents of traditional methodologies might still reject the argument we are developing on the grounds that, even though all participants' statements are socially generated, this does not mean that some statements by participants are not more accurate or more sociologically useful than others. For instance, it has been argued that a scientist's rendering of 'The Bluebells of Scotland' or a page torn at random from a telephone directory are obviously less informative about the nature of social action in a research network than a page of detailed interview transcript or copies of letters exchanged among

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participants.[20]. It is proposed in this line of argument that sociologists can tell good from bad accounts of action and belief; and that they do so by acquiring tacit craft skills which enable them to assess the veracity of different kinds of account.[21]

This view of social research is obviously unsatisfactory if one has reached the conclusion, suggested above, that the social world is not composed of a series of discrete, one-dimensional actions which can be more or less accurately represented. Once we begin to conceive of the social world in terms of an indefinite series of linguistic potentialities which can be realised in a wide variety of different ways and which are continually reformulated in the course of an ongoing interpretative process, the simple procedure of sifting good from bad accounts becomes entirely inappropriate. But even if we remain within the traditional conception of social action, this line of argument still has several weaknesses. For example, the fact that all researchers distinguish fairly easily between relevant and irrelevant data, between participants' letters and the telephone directory, in no way implies that the analysis of relevant data can be accomplished with equal facility. Moreover, it is clearly being conceded that sociological interpretation does depend on the analyst's capacity for understanding and systematically allowing for the social generation of participants' discourse. What is being rejected is the idea that this topic could or should become a critical focus of sociological investigation. There seems to us to be no good reason to insist that such a crucial facet of the sociological craft could not be considerably improved by means of careful, explicit study. In addition, we suggest that linguistic variability is much greater than is implied in the view summarised above; so much so, that no degree of craftsman's expertise can enable the sociologist to sort out the interpretative dross within participants' discourse from what is sociologically valuable.

The variability of participants' discourse

This last claim clearly requires, and is open to, empirical demonstration. If participants' accounts of action and belief are so variable that, when this variability is acknowledged and systematically considered, it prevents the construction of satisfactory sociological interpretations, then it should be possible to demonstrate this by reference to empirical data. However, we can hardly formulate a convincing case for such a general argument in the present introductory chapter, before we have even begun to provide the background information necessary for an understanding of our data. Furthermore, it will not be particularly helpful to examine one or two brief illustrations of actors' interpretative variability at this juncture, for they

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