SPEAKING AND WRITING SCIENCE

Issues in the Analysis of Psychologists' Discourse

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NOTES AND REFERENCES

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ABSTRACT

This thesis is a development of the programme of discourse analysis in the social study of science. The need for an analysis of scientific discourse is demonstrated by taking representative studies from quantitative, ethnographic, interest and relativist programmes and showing how each fails to deal adequately with the texts and utterances which are their ultimate data base.

The analytic chapters of the thesis are based upon verbatim transcripts of the discussion periods of psychology conferences, along with psychologists' published and unpublished writings. The topics of theory application, values in theory choice, categorisations of scientists and scientists' interpretation of discussion are addressed. In each case a distinctive approach is developed which is sensitive to the wide variation in scientists' versions of their own and others' actions and beliefs, and attempts to explicate the interpretative practices through which accounts are produced. These analyses cast further doubt on the adequacy of more traditional approaches.

The analysis also documents scientists' use of two broad explanatory repertoires, corroborating findings from other studies of scientific discourse, and describes some of the detailed interpretative procedures through which versions are produced and modified. Finally certain criticisms of the programme of discourse analysis are discussed.

INTRODUCTION

The work discussed in this thesis is a development of the programme of discourse analysis which is at present under way in the area of social studies of science. This is fundamentally different from most of the major approaches to science taken by other researchers. Although the latter differ from one another in many of their theoretical presuppositions and have adopted widely divergent methodologies, their basic aim is to produce an empirically based account of events, actions and beliefs during particular scientific episodes. In the long run traditional researchers hope to draw together studies of different episodes and specialties to formulate a social theory of scientific knowledge production or to test and improve upon existing conjectures about the way science develops. Quantitative researchers rely heavily on counts of citations to do this, while ethnographers and relativists tend to take part in scientific culture in some way, and interest theorists concentrate on the qualitative analysis of historical documents. Nevertheless their fundamental goal is shared.

Discourse analysts have adopted a rather different basic goal. Instead of trying to produce definitive versions of scientists' actions and beliefs, and thereby to develop a theory of scientific change, they are concerned with the procedures through which scientists themselves construct their accounts of actions and beliefs and the way these accounts are organised in different social contexts. Although this is a considerably more limited goal than that common to traditional approaches it is necessitated by certain unexplicated methodological problems which beset these alternatives whether their data is generated through citation counts, interviews, or documentary analysis. As I show in chapter one, each of these alternative perspectives embodies shortcomings which arise out of their failure to deal adequately with scientists' discourse in its different forms. In particular, these studies fail to accomodate successfully the variability in scientists' discourse and the specific interpretative tasks for which participants' fashion their discourse.

The analytic materials used in the specific studies are verbatim transcripts of scientific conferences. These are particularly appropriate for study because at conferences scientists meet one another for direct communication and are able to gain immediate responses to each others' knowledge claims. Conferences provide a situation in which discursive data can be collected naturalistically, with little or no direct interference from the researcher. Conferences are a novel situation for analysis - I have identified only one other study which uses conference material in the area of social studies of science; and this was brief and flawed (see chapter two). In addition, no other studies have looked at records of face to face interaction of an advesorial character.

The specific analytic chapters of this work address the topics of theory application, values in theory choice, scientists' categorisations of their fields, and scientists' interpretations of their own discourse in transcribed form. In each case the approach adopted is the same. Participants' discourse is closely examined, paying particular attention to the variability in accounts and the particular situations in which certain kinds of accounts are used. The findings question the conclusions of traditional research on these topics while providing further evidence for the necessity of a systematic study of scientists' discourse. In particular, the studies corroborate work on other areas of science which has documented the operation of two broad interpretative repertoires or accounting systems. These repertoires are used when participants account for the applicability of theories, when they depict the role of values in constraining theory choice, and when the events in a section of transcript are characterised. These studies also identify a number of detailed interpretative procedures through which discourse is fashioned to suit the specific context of use.

Finally, it is worth commenting on the issue of anonymity. It must be strongly emphasised that the intention of this work is not to offer any criticism of the participants whose utterances and writings are examined here. Indeed, it is their very ordinariness as instances of scientific language which makes them analytically interesting. Nevertheless, it is not usual for participants' off-the-cuff statements and early drafts to be exposed to quite the same degree of detailed attention as they are here. Consequently to prevent any undue attention being paid to the contributions of specific individuals I have used pseudonyms throughout.

CHAPTER ONE

SCIENTIFIC PRACTICE AND DISCOURSE

In this initial chapter I intend to discuss some of the main approaches that are current in the social study of scientific knowledge. It would be folly indeed, in the light of the claims which are to come, to suggest that this is an entirely dispassionate review of the literature. And it is certainly not meant to be an exhaustive survey which rakes over every last varied inch of this fast growing field[1]. Instead it is intended to fulfill a number of considerably more circumspect goals.

This chapter is firstly meant briefly to summarise some of the main theoretical perspectives which are focussed specifically on the production and reception of scientific knowledge, rather than on purely organisational or psychological issues. Thus I will ignore insitutionally orientated questions concerning, for instance, scientists as a social class, the relation between research in universities and industry, or the growth of scientific specialties; and also issues with a predominantly psychological orientation, such as the personalities of scientists, the age at which they produce their best work and questions of scientific genius. Topics of this kind may occasionally be referred to in this and subsequent chapters, but only insofar as they are of direct significance to scientific knowledge itself or are drawn upon by scientists in their own writings or talk.

A second goal of this chapter is to explicate the way theoretical perspectives on scientific knowledge are based on certain analytic practices and to illustrate the sorts of studies which are taken to confirm or disconfirm such perspectives. Throughout this thesis I will be concerned primarily with approaches to scientific knowledge which are empirically based. Where philosophically orientated theories are discussed, for example in chapters 5 and 6, my interest is in their empirical implications rather than in their adequacy as philosophical schemes.

The third goal of this chapter is to document certain shortcomings in each of these perspectives on scientific knowledge which arise out of their failure to deal adequately with scientists' discourse in its different forms, and in particular with their inability to accomodate to the role of discourse in both constructing and making sense of scientists' social worlds. This will lead into a discussion of those approaches to scientific knowledge which attempt to face up to the difficulties of analysing scientists' writings and utterances. It is here that I will outline the theoretical and analytic perspective which will be adopted throughout the rest of this thesis and at the same time raise some of the questions which will be addressed empirically in later chapters.

Let me begin, then, by discussing four central perspectives on scientific knowledge and its production: quantitative approaches, exemplified in the work of Hewell White, Dan Sullivan and Edward Barboni; 'constructivism', exemplified in the work of Bruno Latour and Karin Knorr-Cetina; 'interest theory', exemplified by the work of Barry Barnes, David Bloor and Brian Wynne; and 'relativism', examplified in the work of Harry Collins. These examples were chosen to be as representative as possible of the broad spectrum of contemporary social studies of science.

In addition to covering a variety of different theoretical perspectives, a strong contrast in methodological approaches is also exhibited. While White, Sullivan and Barboni argue for large scale quantitative analyses of features of formal scientific publications such as citations, Barnes, Bloor and Wynne attempt to reconstruct historical episodes through qualitative interpretation of scientists' formal and informal writings. On the other hand, although both Collins and Latour and Knorr-Cetina use a combination of participant observation and interviewing, Collins has tended to emphasise the different perspectives found in a widely separated network of scientists while Latour and Knorr-Cetina have concentrated on the activities of scientists within a single laboratory.

Overall , then, these studies exemplify the breadth of both theoretical and methodological positions adopted when conducting studies of science. In each case, after giving some general introductory remarks to the theoretical aims of the programme I will concentrate on the analytic practice as seen in a representative study. By taking this approach I hope to address critically at least one example from each of the most important traditions in this broad and heterogeneous field.

Quantitative Studies

Unlike the other approaches which I will discuss there is no single coherent theoretical programme underlying quantitative analyses of science. However, quantitative studies, whether of communication, quality or the reconstruction of historical episodes in science, tend to presuppose a very similar set of assumptions about the way scientific activity may be operationalised in terms of categories which may be counted in some way. For instance, citation is often used as an indicator of communication (in the sense that the cited paper is taken to be used in some way by the citing author), influence (assuming that the citing author's work has been affected by the cited article) or quality (with the number of citations used to measure the degree of impact). In each case, although different theories may be tested, the units and form of analysis are standardised: a numerical feature of an area of scientific literature is treated as revealing some feature of scientists' activity or some attribute of their work[2].

The study I will examine in detail is White, Sullivan and Barboni (henceforth WSB): The interdependence of theory and experiment in revolutionary science: The case of parity violation[3]. It is concerned with the sociological investigation of philosophical schemes. It will thus be used not only to illustrate problems with quantitative research on science but also with the application of broad philosophical models to actual scientific episodes.

WSB use citation analysis to examine the changing relationship between theory and experiment in a specialty of particle physics called 'weak interactions'. Their analysis is 'guided' by Lakatos's philosophical schema for characterising scientific progress and they focus on the idea that progress can be defined as the theoretical anticipation of experimental results: 'Therefore, according to Lakatos, in progressive research programmes... theory will anticipate experiment, and experiment will be directed by theory.'[4] WSB maintain that weak interactions has undergone a scientifically progressive phase following the 'discovery' that one of its previous basic assumptions - the conservation of parity - was untenable. They suggest that the 'V-A theory', which in due course provided a general solution to the anomoly of parity nonconservation, is a remarkably successful theory. The point of their analysis is to show how the interdependence between theory and experiment changed after the 'discovery of parity nonconservation' and at the same time to compare Lakatos's speculations about progressive science with an actual instance. The assumption that weak interactions is progressive is thus essential to the logic of their paper; it is this assumption which allows them to judge the adequacy of Lakatos's analysis by examining its ability to cope with the case of weak interactions.

WSB proceed by categorising articles and citations in the field of weak interactions between 1950 and 1972 according to whether they were experimental, theoretical or phenomenological. Their citation data are presented in the form of graphs which show the deviation of each year's citations from the frequencies which would be expected if citation were random. WSB suggest that by examining the changes in citations across categories it is possible to measure the dependence of each category upon the others, and thus, the dependence of each kind of scientific activity upon the other two kinds of activity. By plotting these citation ratios over time, WSB attempt to identify the changing patterns of interdependence between theory and experiment and thereby furnish a dynamic measure of Lakatos's concept of scientific progress.

WSB find that it is only for one short period, immediately after the publication of the theory which led to the abandonment of parity conservation, that their data are consistent with Lakatos's scheme. During this period there was a large increase in the citation of theoretical articles by experimental articles. WSB interpret this to mean that theory was anticipating experiment; as it should in a Lakatosian progressive research programme. However, soon after this, experimentalists seem to have become less concerned with theory, or as WSB put it:

> these data suggest that the number of 'instances verifying excess [empirical] content' were decreasing in frequency. Thus, if we take a very strong position on the relevance of these data, and if we take Lakatos seriously, we must conclude that weak interactions subsequent to 1959 was experiencing a period of 'declining progress'. (5)

WSB describe the specialty of weak interactions as being revolutionary and progressive. Yet, according to their operational measures of Lakatos's criteria, progress declined after 1959. As a result of this apparent inconsistency between their results and Lakatos's interpretation of scientific development, WSB suggest that their findings, in conjunction with other recent work, weaken our confidence in the adequacy of the Lakatosian scheme. They point to a parallel between weak interactions and radio astronomy. For the latter discipline also experiences an undeniably progressive period of growth in which 'experiment almost always led theory'[6]. Thus neither of these two fields seems to conform to Lakatos's model of progressive science. One response, they accept, could be to retain Lakatos's conception of progress and to classify radio astronomy and weak interactions as non-progressive 'by definition'. But this, they suggest, 'hardly seems sensible', because it would go against participants' strong conviction that these fields are in fact progressive[7]. It appears to them, therefore, in view of the accumulating empirical evidence, that Lakatos's model of progressive science is in need of revision.

In their concluding remarks, WSB seem to distinguish between the correctness and the usefulness of Lakatos's speculations. Lakatos may not be right and more appropriate emprical classifications will have to be devised, but his schema did provide a helpful point of departure. WSB emphasise the contribution made by Lakatos's writings in stimulating them to create useful ways of measuring the relationship between important kinds of social action in science. They maintain that these measures have led, independently of Lakatos's philosophical speculations, to a much richer understanding of the dynamics of intellectual change in this specialty[8].

I will now examine in more detail how WSB apply

Lakatos's philosophical categories to their specialty. WSB treat this exercise as if it were quite straightforward; various terms in Lakatos's philosophy are taken to refer to certain observable features of weak interactions. Crucial to their argument is the claim that the conservation of parity is part of the 'hard core' - in Lakatos's sense - of weak interactions. For they claim that the overthrow of parity conservation led to the formation of the 'first truly "progressive" research programme in weak interactions'[9]. Yet WSB relegate the justification of this claim to a footnote, where they state that there is 'much evidence' that parity conservation was part of the hard core. As an example of such evidence, they quote three lines from a speech given at a Nobel Prize ceremony, in which a participant refers to the assumption about the symmetry of elementary particle reactions which was held 'almost tacitly'[10].

Lakatos himself writes only in very general terms about the hard core being made up of essential or fundamental assumptions. It is not clear, therefore, what either WSB or Lakatos mean by 'fundamental', although there is nothing to suggest that Lakatos simply means 'tacitly held'. Moreover, WSB's procedure of referring to one brief phrase in a Nobel speech in order to identify a fundamental component of the hard core seems strikingly asociological. We cannot take a Nobel presentation speech as a colourless factual record of the development of weak interactions. It is surely the case that such speeches are designed for the occasion, in such a way that the nature of the 'achievement' being celebrated is fully recognised. Thus it is equally possible to treat the passage quoted by WSB as an example of scientists reconstructing events in a way which makes their award of the prize appear entirely appropriate and natural[11].

Despite their claim to be checking Lakatos's ideas by means of rigorous quantitative methods, WSB's identification of the hard core in the text of their paper depends on a highly selective and rather simplistic use of a participants' account of the 'central assumptions of the field'. This leads me on to a more basic question about the equivalence of participants' and analysts' categories, namely: in what sense is weak interactions a research programme or a series of research programmes? Lakatos's seemingly commonplace terminology makes it tempting to equate a research programme with the social units recognised by participants. However, the Lakatosian concept has an explicit philosophical meaning, part of which suggests, for instance, that it refers to a series of theories, each adding clauses to the last[12]. WSB do not acknowledge this in their paper and they do not check whether the entity 'weak interactions', as defined by participants, corresponds systematically with the Lakatosian concept of 'research programme'.

WSB avoid facing this problem of conceptual correspondence directly by simply re-interpreting Lakatos's concepts in participants' terms. Thus, the research programme under investigation is treated as identical to the specialty of weak interactions; the hard core is treated as equivalent to the assumptions of parity conservation/nonconservation; theory and experiment are defined in terms of participants' distinctions between theorists, experimentalists and phenomenologists; and so on. Throughout their text, WSB move frequently and unreflexively between analysts' and participants' categories, usually treating the two kinds of concept as equivalent, whilst consistently adopting participants' terminology, definitions and interpretations as their own.

WSB not only give participants' categorisations precedence by allowing them to subsume Lakatos's philosophical concepts, but they also prefer what they take to be participants' interpretative claims to those of Lakatos whereever there appears to be a discrepancy. This can be seen most clearly in WSB's decision to accept the 'accumulated wisdom'[13] of participants as providing the most convincing index of scientific progress. WSB appear to work on the assumption that if enough scientists say that a field is progressive, then it offends commonsense to maintain that it could be otherwise. The point which I want to emphasise is that central parts of WSB's analysis consist of restatements of what they take to be the general view of the field as expressed by participants.

There are several problems with this use of scientists' accounts. First of all, the analysts tend to ignore the diversity of these accounts. Did the participants all say exactly the same thing on all occasions about, for example, the progressive character of the field/programme? If not, how have the analysts obtained their simple summary of participants' views? We cannot answer these questions for weak interactions, but the variability of participants' characterisations of radio astronomy, and their lack of uniformity or of clear consistency, are well documented[14].

This leads us to a second problem, that of temporal reference. To which precise period does the 'accumulated wisdom' of participants refer? WSB find that weak interactions was strongly progressive in the Lakatosian sense only during the period 1957-9 and they conclude that in Lakatosian terms 'weak interactions subsequent to 1959 was experiencing a period of "declining progress"'[15]. This, they suggest, seems inconsistent with participants' characterisation of the field as 'progressive'. But WSB offer no careful examination of members' accounts in order to show that these accounts are clearly incompatible with the results of their citation analysis. It may well be that, although participants refer loosely to 'the progressiveness of weak interactions', they would be quite willing to accept that the field was more progressive in the late 50's than at any other time.

Thirdly there is the question of what participants mean when they refer to 'progress' or when they use some equivalent term. Thus a participant might say: 'Radio astronomy was certainly progressive during its first two and a half decades in the sense that new kinds of data and new realms of study were being rapidly identified. But no major advances in scientific understanding occurred then. Thus real progress, which of course depended on these earlier observations, occurred only after the mid-1960's when the task of theoretical interpretation began in earnest'. In this hypothetical, but plausible, statement the notion of progress is used in a subtle way to encompass the whole development of the field, yet at the same time to allow for different degrees, phases and facets of progress. By varying the meaning of the term 'progress', the speaker can claim both that radio astronomy has undergone one continuous progressive sequence and that it has been progressive only since the mid-1960's. These, and many other easily conceived and easily documented, accounts are quite possible.

The same kind of possibilities presumably apply equally in the case of weak interactions. Thus participants' interpretative accounts cannot be used in the simple manner exemplified in, but by no means restricted to, WSB's study.

So far I have examined the way that WSB either rely on selective examples of participants' actual accounts or summary versions of participants' supposed accounts as they try to conceptualise such basic Lakatosian notions as 'hard core', 'research programme' and 'progress'. A similar failure to deal carefully enough with participants' interpretative work is evident in their quantitative methods. Once again, the way in which they deal with members' accounts undermines their analysis.

WSB do not propose that quantitative methods should replace qualitative methods, but they do see them as providing a 'check' on claims derived from qualitative evidence as well as a 'finely calibrated' assessment of research areas[16]. Their study, they claim, contains quantitative findings that prove to be particularly revealing[17]. Central to their method is a three-fold classification of articles on weak interactions based upon participants' own categories. It is easy to allocate articles to these categories, they suggest, because it is 'well known' that elementary particle physics papers are 'quite easily distinguishable'[18] into those which concern general theory, phenomenology and experiment. WSB appear to mean by this that there seems to be considerable agreement among participants about the usefulness of such a classification of articles. Because they are presupposed in the interpretation of citation data, it is crucial for WSB's paper that these categories are valid and reliable. More specifically, each category of papers is taken as representing a discrete class of social action and the citations between categories of papers are taken to represent interdependence between these classes of action. Thus WSB's measurements of the interdependence of 'theory' and 'experiment' ultimately depend on how they allocated papers to these three categories.

WSB place research papers in the three categories ostensibly by reading the formal text of each paper and by inferring from the text what kind of scientific action was involved in generating the text. Thus a 'basic experimental paper' is one which 'is not explicitly related by its authors to any guiding theoretical work'[19]. A 'theory-testing experimental paper' can be recognised when authors include a statement like the following: 'These theoretical considerations have stimulated us to undertake a search for long-lived neutral particles'[20]. Phenomenological papers are said to involve ideally 'an interface between theory and experiment'[21]. They are treated as a distinct class of theoretical papers whose authors are concerned with 'building mathematical models of fairly narrow categories of empirical data generated by experimentalists'[22]. The authors of 'general theoretical papers', in contrast, are dealing with 'the very nature of weak interaction, not with any particular set of particle decays'[23].

One immediate problem with this procedure is that it appears to take the formal text of the published paper as a reliable guide to the actions involved in producing it and to other actions on which it reports. Yet there is clear evidence that researchers can describe a given set of experiments in quite different terms, depending on the context. For instance, an experiment can be described in the published paper as a new method for measuring the known value of a well-established phenomenon, whilst being described in an interview as a moderately convincing test of a controversial theory[24]. It is therefore suggested that there is no way in which WSB can infer the nature of participants' actions from the formal text alone.

A second problem is that the scope of WSB's categories seems extremely vague. This is hardly surprising, given that they are taken over from the everyday discourse of participants. But while such loose terminology may be perfectly adequate for the ordinary interpretative tasks facing participants, it furnishes an insecure basis for WSB's attempt at rigorous quantitative measurement of social action. For instance, it is not easy to see any clear distinction between 'theory-testing experimental papers' and 'phenomenology papers' dealing with the interface between theory and experiment. It is not even required that the authors of 'experimental papers' publish their own original data. For WSB count as experimental papers those where <u>experimental</u> particle physicists have obtained raw data elsewhere and have analysed it[25]!

Similar problems beset their citation analysis. They assume that citations can be used as an indicator of 'dependence', or as they put it 'an indicator of the degree to which theory, phenomenology, and experiment were found formally dependent on each other'[26]. In a footnote, WSB state that 'formally' here means 'in the published literature'[27]. They have, therefore, moved from the Lakatosian conception of dependence of theory on experiment to the much more restricted notion of 'dependence in the published literature'. This (unexplicated) translation allows them to maintain that they are measuring, solely by counts of citation, Lakatos's complex analytical terms dealing with scientists' actions and beliefs; for example, theory is treated as dependent on experiment if theoretical articles cite experimental articles. Yet they give no coherent rationale for this. The equation of 'dependence' and 'citation' is established entirely by means of an analysts' fiat. That this type of move is commonly made by other citation analysts does not justify it, particularly as in other cases citation data are taken to be a direct indicator of scientists' recognition or even of the quality of the cited work[28]. Each of these variables is quite different and yet no argument is offered as to why citations should measure one rather than another in any given analysis. These analysts simply take over participants' conventionalised versions of cognitive interdependence, which have been produced for the specific context of the formal literature, define them arbitrarily as equivalent to a Lakatosian concept, and treat the ensuring numbers as analytically unproblematic.

Even within WSB's paper there are indications that the notion of dependence is not exhausted by citation counts alone. Referring to a period when cross citation between 'experimental' and 'theoretical' articles was low, they say that they are:

> not suggesting that general theorists were unaware of experimental data, or that they did not try to influence the conduct of experiments during this period. We suggest only that their current research was not immediately dependent on current experimental results. (29)

This passage makes it clear that WSB are quite aware that there may be connections between 'theory' and 'experiment' that is, between the actions of 'theorists' and 'experimentalists', which are not revealed in their citation counts. This does not, however, lead them to search for more adequate indicators of 'dependence'. Nor does it lead them to state their findings in more modest form. Thus the figure which summarises their findings on weak interactions is entitled 'The Interrelationship of General Theory, Phenomenology and Experiment etc.', rather than 'Participants' Versions of Interdependence, as expressed through their Citations to Various Categories of Published Papers'[30].

When we look at the manner in which WSB actually use their quantitative data, we find that they do not use it as a check on their qualitative material. Instead, their interpretation of quantitative data is based upon a qualitative assessment of the field; which in turn seems to derive in a largely unspecified manner from participants' accounts[31]. WSB introduce their quantitative data in the context of a brief intellectual history of weak interactions. If quantitative data were being used to check qualitative material we would expect that, in the case of disagreement, the qualitative analysis would be reworked. Yet, this is not the case.

A good example occurs in the discussion of V-A theory. V-A theory is crucial to WSB's analysis, because they present it as the turning-point in the development of weak interactions. They suggest that the quantitative data support their qualitative estimate that this theory led weak interactions to be thoroughly progressive; that it was 'an intellectual tour de force which <u>anticipated</u> experimental results for several years'[32]. Their quantitative data consist of ratios which measure the rates of citation between the three kinds of research papers and which are taken to represent interdependence between the corresponding kinds of social action. WSB use a ratio of 1.0 to represent random citation. Less than 1.0 means that one category is citing another less often than would occur if citation were random. More than 1.0 means that a category is being cited more frequently than in a random pattern. The graphs go from 0.0 to 3.0, but the great majority of (non-self referring) data points are in the range 0.0 to 1.0.

The V-A theory was published in 1957 and is described by WSB as 'extraordinary in the degree to which it seems to meet or exceed all criteria by which theories are generally evaluated'[33]. It is suprising to find, however, that WSB's quantitative data on the impact of V-A theory include only one data point above 1.0 (1958 in Fig. 8)[34]. WSB point out that 'in the years 1957-60 the ratio of actual to expected random references', from experimental to general papers 'was close to 1.0'[35] and they describe this as a 'huge pertubation relative to the years 1952-56'[36], when the value was close to zero. But this summary of the quantitative data omits 1951, which has a value close to 1.0 and above those for 1957 and 1960 when the V-A theorv is supposed to have been making experiment extraordinarily dependent on theory.

Moreover, not only are data points below the random level taken as evidence of interdependence for the period immediately following the publication of the V-A theory (1959, 1960), but this interpretation of the quantitative data is quite inconsistent with that carried out elsewhere in the paper. Thus in Fig. 5, WSB deal with citations in the opposite direction, that is, from theory to experiment. In this case a ratio of almost 1.0 (1953) and several close to 1.0 (1962-3) are simply discounted. WSB merely assert that the relatively high level of citation by theory of experiment in 1953 does not represent dependence of theory on experiment. And despite an overall level of citation of experimental papers by theorists which is at least as high as that of theoretical papers by experimentalists, WSB choose only to recognise the dependence of experiment on theory. Thus WSB's quantitative data, rather than furnishing a 'finely calibrated assessment of the state of a research programme'[37], is freely reinterpreted or ignored where it appears to conflict with the qualitative intellectual history that they decide to tell. It is by no means clear where this qualitative history of weak interactions comes from. However, it seems likely that it derives in some way from that class of scientists' folk history in which crucial experiments and theoretical tours de force provide the main interpretative components[38].

WSB's overall account of the development of weak interactions stays close to the interpretative conventions which scientists maintain within the formal research literature. Elements of social contingency hardly enter into their version of events; presumably because such elements are almost completely excluded from scientists' discourse in the formal setting. Thus the development of the network and the relationships between its members are presented as unfolding in accordance with, and as a result of, scientists' formulation of an increasingly accurate theory. The abstract accounts of 'theoretical' and 'experimental' actions which scientists employ in their papers provide, for WSB, the appropriate categories for capturing researchers' concrete actions in the lab, the conference hall and at the coffee table. As we will see in the next section, a rather different kind of study is produced when sociologists focus on accounts produced in the relatively informal context of the scientific laboratory.

The Constructivist Programme

The term 'constructivism' has been used to apply to various kinds of social study of science. Gieryn, for example, brackets together work associated with Collins and Knorr-Cetina, along with studies of scientific discourse, as the 'relativist/constructivist programme'[39]; and researchers concerned with the role of social and cognitive interests have frequently stressed the constructivist nature of science[40]. Here, however, I will concentrate specifically on the work of two researchers: Karin Knorr-Cetina and Bruno Latour[41]. While their approaches are not identical they share a strong emphasis on the central importance of scientists' practices within the laboratory along with an associated methodological stress on the need for direct observation of scientists going about their daily tasks. In this section my concern will not be to detail points of agreement and disagreement between these researchers but to show how their general theoretical perspective is based on an analytic approach which has certain crucial shortcomings associated with its unselfconscious use of scientists' discourse.

Both Latour and Knorr-Cetina contrast their perspectives with those of traditional philosophical and sociological approaches which emphasise the central role of socially invariant criteria in the constitution of knowledge and which suggest that scientists are principally concerned with describing reality. Knorr-Cetina stresses the breakdown of this 'objectivist' view of science which assumes that 'the world is composed of facts and the goal of knowledge is to provide a literal account of what that world is like'[42]. Instead, Knorr-Cetina suggests that facts are fabricated or constructed.

> Rather than view empirical observation as questions put to nature in a languaoe she understands, we will take all references to the "constitutive" role of science seriously, and regard scientific enquiry as a process of production. Rather than considering scientific products as somehow capturing what is, we will consider them as selectively carved out, transformed and constructed from whatever is. And rather than examine the external relations between science and the "nature" we are told it describes, we will look at those internal affairs of scientific enterprise which we take to be constructive. (43)

This stress on the constructed nature of facts, along with the suggestion that scientists are not orientated towards a natural reality is mirrored by Latour and Woolgar.

> If facts are constructed through operations designed to effect the dropping of modalities which qualify a given statement, and, more importantly, if reality is the consequence rather than the cause of this construction, this means that a scientist's activity is directed, not towards "reality", but towards these operations and statements. (44)

For both Latour and Knorr-Cetina, then, 'reality' is seen as a 'product' of scientists' laboratory practices rather than the object with which these practices are concerned.

The explicit epistemological consequences of these proposals are unclear. Latour and Woolgar, for example, oppose their constructivism to the realist philosophy espoused by Bhaskar[45]. For them the realist position depends on the circular argument which explains the findings of science as a product of 'the natural world' and 'the natural world' as a discovery made using the methods of science[46]. For instance, Latour briefly reconstructs the history of paleontology in an attempt to illustrate a continual and viciously circular alternation between explaining the real nature of dinosaurs in terms of science and the findings of science in terms of the real nature of dinosaurs.

Knorr-Cetina, on the other hand, draws more approvingly on Bhaskar's work. Indeed, she implies that his position, while retaining an outmoded notion of the goal of science being to decipher nature, can underpin a constructivist approach to science through stressing, for instance, the causal role of experimenters in creating lawlike conjunctions of events[48]. Moreover, Harre, in his preface to Knorr-Cetina's The Manufacture of Knowledge, is even less ambivalent about the realist nature of Knorr-Cetina's work. He suggests that science only makes sense in realist terms, and that Knorr-Cetina's research 'is a realist enterprise, an attempt to truly represent the social order of life in laboratories and institutes of research, just as they are'[49]. And indeed Knorr-Cetina draws heavily upon Harre's 'ethogenic' theory of social life, which attempts to reveal the structure of the social competence which he takes to enable the generation of social activity[50].

Whatever their detailed epistemological proclivities, both these authors stress that the production of knowledge is intimately bound up with the idiocyncracies and possibilities inherent in particular scientific locations. For Knorr-Cetina the products of science are,

> contextually specific constructions which bear the mark of the situational contingency and interest structure of the process by which they are generated, and which cannot be adequately understood without an analysis of their construction. (51)

Similarly, Latour and Woolgar emphasise the role of the specific social context and, moreover, the way scientific products are constructed to appear independently of it. They claim that,

> science is entirely fabricated out of circumstances; moreover, it is precisely through specific localised practices that science appears to escape all circumstances. (52)

Latour and Knorr-Cetina's analytic emphasis on the central importance of ethnographic, observational studies of scientists' practice flows directly from their theoretical insistence on the dependence of scientific products on the contingencies of specific locations. For Knorr-Cetina in particular it sometimes seems as if the metaphor of knowledge manufacture is taken so literally as to imply that knowledge is produced in the laboratory like loaves of bread in a bakery:

> ...it is clear that the question of how scientists produce and reproduce their knowledge refers us to the site of scientific action. It prompts us to look (and as closely as possible) at the process of manufacture of knowledge on the spot. In other words, we must dismiss the battery of intermediary tools normally used to negotiate with social reality, and immerse ourselves directly in the stream of scientific action. (53)

It is interesting to note that, despite Knorr-Cetina's swingeing attack on notions of science which emphasise representation and description, visual metaphors of this kind are omnipresent when she characterises her own analytic practice. In the above extract, for instance, we are asked to 'look', and look 'closely', and thereby become 'directly immersed' in the 'flow' of scientists' actions. Furthermore, Knorr-Cetina's text is organised to suggest that there is a specific 'site' for production, a 'spot' at which to look for the formation of knowledge. Although this recurrent metaphor sustains the idea that ethnography is central for the study of scientific knowledge it does so only by presupposing a highly asocial conception of knowledge which places paramount stress on construction and validation processes occurring within the laboratory at the expense of broader social processes[54].

Latour and Woolgar take a rather broader view of the process of fact construction. In their analysis of the making of a particular fact they treat the factual status of scientists' claims as dependent upon broad acceptance by a particular scientific community[55]. The actions found within any specific laboratory can be at most only part of this process. This means that the kind of close observational study advocated by Knorr-Cetina is quite impractical when dealing with the entire process of scientific knowledge production[56]. Not only is the process dependent upon the responses of a dispersed social network but it is most unclear what exactly one would be seeing which would constitute the acceptance of a fact. Latour and Woolgar use a citation analysis and study the changing modalities on claim statements as indicators of the taken-for-granted status of a fact. Knorr-Cetina wishes to transcend such intermediary tools; yet what is observable without them she never makes clear.

Despite being forced back on to indirect measures in their study of the construction of a fact, Latour and Woolgar appear to concur with Knorr-Cetina on the issue of the mediation of scientific reality. They suggest that observation can avoid the problems which arise when trying to use scientists' accounts of various kinds which may be misrepresentations or merely conventionalised reconstructions of scientists' actual practices.

> scientists' statements... systematically conceal the nature of the activity which typically gives rise to their research reports. ...the fact that scientists often change the manner and content of their statements when talking to outsiders causes problems both for outsiders' reconstruction of scientific events and for an appreciation of how science is done. ...it is necessary to show through empirical investigation how such craft practices are organised into a systematic and tidied research report. In short, how is it that the realities of scientific practice become transformed into statements about how science has been done? (57)

It is only through examination of scientific practices at a sufficiently close level, argues Knorr-Cetina, that the analyst is able to 'differentiate between knowledge-constitutive procedures and rationales'[58].

Despite their stress on observation as a solution to problems of unreliability in characterising actions from scientific discourse, both Knorr-Cetina and Latour accept that in practice there can be no simple distinction between observation and the use of scientific accounts. For scientific accounts must be used to make sense of observations; the observations are only given meaning in terms of scientists' local semiotic systems[59]. Or, as Knorr-Cetina puts it,

> ...understanding... cannot be gained by observation alone. We must also listen to the various forms of talk about what happens... For the scientists, the savage meaning of things is contained in their laboratory reasoning; and the talk which centres around this reasoning must be our major source of information. (60)

Furthermore, when we examine Knorr-Cetina's analytic practice this caveat takes on a crucial importance. In virtually no cases does Knorr-Cetina present data in the form of direct observations. Instead it takes the form of either excerpts from drafts and papers[61], sections of scientists' talk[62], or vignettes describing broad social episodes[63]. Likewise with Latour: the vast majority of the data he presents consists of verbal or written traces of one kind or another[64]. It seems, then, that in the work of both Latour and Knorr-Cetina there is an important tension between the analytic claim to be observing scientists, so as to avoid the distortions of indirect measures, and the analytic practice which is heavily dependent upon scientists' versions of their actions embodied in their discourse. The implications of this can be seen more clearly by examining the way specific analytic claims are arrived at.

Let us take as a specific research example Latour's paper: Is it possible to reconstruct the research process?; Sociology of a brain peptide[65]. This article is particularly apposite for examination here because it claims to be illustrating the utility of the constructivist approach to science by reference to a particularly straightforward case study. It also shares many features and claims with Knorr-Cetina's work. Latour describes the goal of the paper as being to demonstrate the appropriateness of a number of 'external' concepts in the social explanation of the synthesis of a single brain peptide. He starts it off by commenting on the traditional distinction between 'external' (social) and 'internal' (rational, cognitive) factors in the production of science, and he suggests that concepts traditionally used to explain 'external' factors have been shown by recent social studies of science to be equally applicable to 'internal' factors: 'indeed, the whole process of fact construction has been shown to be accountable inside a sociological framework[66]. The paper is organised in sections which suggest, in turn, that the research process is 'contextual', 'heterogeneous', 'opportunistic', 'idiosyncratic' and 'fiction building'. I will follow Latour's sequence, and for the sake of brevity concentrate in detail on only the first three of these concepts[67].

Latour claims that the research process is 'contextual', that is, that the meaning of scientific statements is dependent upon the context in which they are produced. However, he does not wish to restrict his claims simply to nominal features of scientific talk; Latour suggests that whenever the peptide somatostatin is used in a new research programme,

> the meaning of the original molecule, and then the very nature of this molecule, is modified and recreated. (68)

We must be clear about what Latour is claiming here. On a first reading it might appear that Latour is implying that the actual physical makeup of the molecule is changed whenever it is used in a new research programme. However, it is clear from the discussion of contextualisation in Latour and Woolgar[69] that this is not what Latour intends; rather he is proposing that as the 'meaning' of the molecule shifts so the participants will see the actual structure of the molecule differently.

For example, Latour describes how prior to 1974, in a particular laboratory, the somatostatin structure meant 'an order from the brain to stop releasing growth hormone'[70]. After this time, however, in another laboratory, it was suggested that the peptide inhibits insulin. This in turn led to a search for a form of the peptide which would inhibit the glucagon that is dangerous to diabetics while releasing the vital insulin; and this was big business.

> Of all possible analogs, the ones that have to be devised in priority are the ones able to mean: "block glucagon, release insulin", because each ofthem is worth millions of dollars if it could be some help in treating diabetes. (71)

In what sense has the 'meaning of the molecule' changed across these different contexts? Take, for instance, a hypothetical example. A coffee grinder is initially used for grinding coffee; it might, however, be later noted that it can be used for grinding nuts to go into vegetarian recipies (on a large scale, surely a multi-million pound industry!). The sense in which the meaning of 'coffee grinder' has been modified when it is given this new function seems to be trivial; it is not at all clear that users (and they are crucial for Latour) would say that the <u>very nature</u> of the coffee grinder had changed. Likewise, we would not expect a traditional historian of science necessarily to claim that the meaning, and therefore the users' understanding, of the nature of somatostatin has been changed as new uses are found for its derivatives; and this is crucial because Latour's account is essentially based on just such a traditional reconstruction. He simply takes what must be a typical participants' potted history and recasts it to produce the effect of meaning change; that is, despite his stress on contextual changes in meaning he presupposes a a high level of meaning invariance.

We can see why this is so, if we examine his notion of context more carefully. Essentially he has taken over this notion from participants' conventional classifications which divide science into broad units such as research programmes. These are seen as organised around specific questions: can we find a way for this peptide to inhibit glucagon?; can we make it release insulin? Such questions are likely to appear prominently in grant proposals and in participants' generalised characterisations of other groups in the area of neuroendocrinology. Latour thus seems to take his analysts' category of context directly from participants' own representations of divisions within the discipline. The only time he identifies meaning change is when participants' identify a shift in research programme. Moreover, as I have tried to suggest with the coffee grinder example, it is not clear that participants would treat Latour's analysis as anything more than a redescription of their own folk version of the changing research process and the way attitudes to the molecule and emphasis on different aspects of its structure change according to the goals of the programme.

All this is not to suggest that the meaning of somatostatin is not variable; rather it is intended to illustrate the inherent limitations of Latour's specific approach to variability. His stress on the significance of scientists' statements of the molecule's structure ought to have allowed him to look at how they were modified across different contexts. Yet he was constrained by his simplified participants' definition of a context. If he had examined neuroendocrinologists' discourse more critically, with a more sophisticated conception of context, he might indeed have been able to show that the very structure of the molecule was differently understood or formulated in various contexts. For instance, we might speculate that certain kinds of version of somatostatin's structure (for example those stressing its potential for alleviating diabetes) would be regularly used in proposals for obtaining funding or in the presentation of the field to laypeople[72]. It might also be the case that neuroendocrinologists would use different versions of the structure of TRF, the molecule, when emphasising or denying their shared membership of certain categories of scientists[73]. This approach could have provided strong evidence for the socially occasioned meaning of somatostatin; yet it is closed off to Latour by his over-ready acceptance of broad participants' glosses which obscure exactly the sorts of discursive variability which would have been analytically interesting.

Latour's second claim is that the research process is 'heterogeneous'. By this he means that 'no matter how close one tries to be from (sic) the research process, no homogeneous set of factors that could be called 'internal' or 'purely internal', is visible'[74]. Instead, many factors, originating in different areas of the social world contribute to the production of any particular findings. Latour uses extracts from interviews as support for this claim. These suggest the importance of 'gut feeling' and chance in addition to rational consideration.

> All the Alanine modifications had been done... From the literature it is known that Trvptophane is important biologically... There is also a gut feeling... I just had received some D-Trp... for LRF... I tried the first D-modification (instead of the levoratatory form only existing in nature). It turned out that I hit right in the bull's eye. (75)

They also suggest the importance of issues concerning power and authority between individual scientists.

> There were tensions in the laboratory... also I had trouble to cyclize somatostatin... something seemed to be missing. Then I supposed that the structure of natural somatostatin was not the published one and that homocystein was necessary; the synthesis would have been made easier and I would have proven that X [his chemist competitor in the lab] was wrong... (76)

For Latour, the heterogeneous nature of the research process is documented by jumps between one line of reasoning to another in these extracts, from social to rational considerations. However, it is even more strongly in evidence, suggests Latour, when these accounts are compared with those from other scientists who formulate totally contrasting versions of these actions. For instance, Latour cites another scientist's reaction to the first of the two extracts quoted above.

> It is not by chance at all! N came with a model of the molecule; he gave a seminar or something; his molecule was folded at the eight position; I immediately suggested to put a D-Trp at this position; that was the only way of reinforcing the molecule, probably, N's model was wrong, we know that now... Anyway, we would have done it sooner or later. That was systematic. But we saved, maybe a year by doing it in the first place. (77)

As we will see, this problem of variability within and between scientists' accounts has been found to be pervasive in studies which attempt to use scientists' discourse in whatever form[78]. However, Latour's response to the problem is deeply flawed; he takes these contradictory accounts as evidence that the actual process is in fact heterogeneous. That is, he treats variability in accounting as an indication that various different processes - some rational, some social - all have an impact on scientists' practice. The implicit approach seems to be that there is a core of truth in all accounts even though they can be contradictory. It seems that Latour accepts that there are no criteria for separating those accounts which are true from those which are false; yet at the same time he wishes to infer from them substantial claims concerning scientists' practice. He thus adopts this compromise position which implies that there is some, but not the whole, truth in each account. This, however, forces Latour into the uncomfortable position of having each additional account imply that the research process was actually that bit more hetrogeneous. The more variously accounted an episode (think of Galileo and planetary rotation!) the more heterogeneous they actually become. This approach to scientists' discourse is all the more surprising as elsewhere in the paper Latour notes that 'there is nowhere any account of research that could be something more than a fiction'[79]. Taken seriously this might have been the basis for a more viable analytic practice (this is discussed above); indeed, Latour might have found that there was a highly organised structure to the accounts which, taken literally, implied only chaos and confusion[80]. As it is, this claim stands in uneasy tension with his analytic practice.

If we examine Latour's next claim, that the research process is 'opportunistic', a very similar difficulty in in the use of participants' accounts becomes apparent.

Latour takes 'opportunism' to mean that scientists react to local circumstances by creating context dependent chains of reasoning and formulating provisional rules, rather than by the application of generalised, preformed rationales. Again, this is an interesting and important claim; yet Latour's evidence for it is by no means unproblematic. He contrasts two sorts of account of the research process. One of these depicts it as being a logical sequence of reasoning and operations derived from a small number of broad premisses. For instance,

> If you give me a peptide, I could devise several hundreds of analogs, just from what is already known in the literature: the D-series, the Alanine series, the replacement by Gly; the deletion series; all that is known, it is logical. (81)

The other kind of account characterises the research process in a much less systematic fashion.

> But see you have to be systematic <u>and</u> opportunistic this little word 'and', is the reason why JR so much despises 'industrial scientists':
> They do everything systematically; they screen everything; just screen; it's not science; it's just a computer job. (82)

Latour clearly treats the latter kind of account as providing a more correct version of what actually goes on in the process of investigation. His overall description of the opportunistic nature of research is virtually a gloss on the second of the two extracts; and his detailed account of the different modifications of the peptide analogues is apparently based on just such informal accounts. Yet he offers no criteria for accepting the latter kind of account rather than the former.

Further on in his paper Latour shows that the more orderly, rational version of the research process, as seen in the first of the two extracts above, is the one used in the research literature of the field. The pattern of accounting he discovers thus mirrors that found by Gilbert and Mulkay in their study of formal and informal accounts of the research process[83]. However, instead of looking at the <u>organisation</u> of these different forms of accounting, and the way they are <u>fashioned</u> to fulfill specific interpretative tasks as Gilbert and Mulkay do and as is advocated here, Latour's concern is with the facticity of accounts and what they can reveal about the scientific practice which he sees as lying beyond them. To take one particular example, Latour describes the rules used by these scientists as small scale and flexibly moulded to meet specific contexts. Yet he nowhere examines the interpretative uses which participants make of such rule formulations or the systematic variation of such claims to rule use[84]. If he had done this he might have come to very different conclusions about the significance of his data.

Overall, then, Latour's analysis has an ambiguous observational status. As we have seen, his account seems to be more often built up from participants' discourse of various kinds; yet in doing so he selectively ironises and reifies participants' versions in order to achieve his unified analysts' version of what goes on in the scientific laboratory. Furthermore, although I have not discussed it here in detail, the same critical points are applicable to Knorr-Cetina's work. She too makes selective and inconsistent use of participants' interpretative formulations with the final goal of recovering the actual practices through which knowledge is constructed.

The Theory of Social Interests

In this section I will discuss the theoretical perspective which attempts to explicate the content of scientific knowledge in terms of various kinds of social interests. Some of these interests may be sited in the local disciplinary context in which scientists work, and to this extent there may be a limited overlap with the studies of laboratory practice discussed in the previous section and also with the 'relativist' perspective discussed in the next. However, this approach is distinctive in stressing the influence of scientists' background culture and social allegiance and it is this aspect of the approach on which I will concentrate here.

The theory of social interests is often referred to as the 'Strong Programme' in the sociology of science[85]. This title is intended to contrast it to traditional sociology of science which is taken to be concerned only with the explanation of scientific error and the elucidation of the conditions which facilitate the formation of correct scibelief. This position is particularly associated with Barry Barnes[86] and David Bloor[87] and has been developed in empirical studies by their associates Shapin, Mackenzie, Wynne and others[88].

In this chapter I will not be primarily concerned with the epistemological features of interest theory. These have been critically discussed elsewhere from a number of contrasting positions[89]. Furthermore, unlike the other perspectives discussed in this chapter, there have recently been two detailed critiques of the analytic practice of interest theorists[90]. These critiques use an approach very similar to the one adopted here and will be drawn upon throughout this section. Although my intention is to concentrate on the specific form of analysis conducted by these theorists, it is necessary to give some account of their general epistemological stance in order to show how it underpins their particular analytic approach.

One common device which interest theorists use to characterise their stance is the 'Hesse-net'[91], a metaphor derived explicitly from the 'Quine-Duhem thesis'[92] which treats scientific beliefs as necessarily being joined to and acquiring their meaning from an interconnecting web or network of inference relationships. In its classic Quinean form, of course, this metaphor was meant to demonstrate the indeterminate effect of sensory evidence on a system of knowledge. Because of its interconnected nature - the argument runs - any particular belief statement may be retained in the face of a contradictory observation statement by making a readjustment somewhere else in the system: by modifying or abandoning a general theory, say, or even a logical law[93].

The ultimate consequence of this model is that there will inevitably be a conventional element in any scientific evaluation. For, with every novel observational or theoretical statement, a range of adjustments in the network will be possible. Therefore the actual effect of any novel statement will be dependent not only on the meaning of the statement, but also on some general coherence conditions which specify what sorts of transformations of the network are acceptable[94]. It is these coherence conditions which will be conventional in character. As Barnes puts it,

> different nets stand equivalently in relation to 'reality' or to the physical environment... alternative classifications are conventions between which neither 'reality' nor 'pure reason' can discriminate. (95)

In his original article Quine talks only in vague terms of the network tending towards conservatism and simplicity. The point made by the interest theorists is that criteria such as these, or the more elaborated lists which have been produced more recently, are insufficient for making determinate evaluations. Following Mary Hesse[96] they argue that coherence conditions are made up not only of general criteria such as simplicity, or even utility, but are also a product of scientists' goals and social interests. Bloor summarises this point,

> [coherence conditions] can come from nature being put to social use as well as practical use. Certain laws are protected and rendered stable because of their assumed utility for purposes of justification, legitimation and social persuasion. Since these activities are meant to further interests we can say that interests are coherence conditions. And since interests derive from, and constitute social structures, it will be no suprise to find that putting nature to social use creates identities between knowledge and society. (97)

Overall this position claims to be opposed to the 'Manichean mythology'[98] which treats the development of knowledge as a struggle between the forces of 'good' and the forces of 'evil' (objectivity and reason vs. culture and convention). It argues that all systematic knowledge is necessarily dependent on both of these things. Indeed, it is claimed that without the goals and interests which underpin particular coherence conditions there would be no way of dealing with the world at all. This is because goals and interests are seen as necessary for making intelligible particular criteria of technical and empirical adequacy; i.e. it is only in the light of a particular goal that a decision is possible, say, about whether a finding is technically adequate[99].

Expressed in programmatic terms interest theorists suggest that the proper sociological study of science ought to be naturalistic[100]. For Bloor this means that the Strong Programme should be: 1) causal, 2) impartial, 3) symmetrical, and 4) reflexive. We can see how the discussion of Hesse-nets may lead to tenets 1, 2 and 3. In the first case, as goals and interests are taken to be properties of broad social groups, the interest theorist will be concerned to elucidate causal links between the goals and interests of social groups and the contents of particular scientific knowledge claims or cognitive systems. Secondly, analysis will deal impartially with beliefs considered true and beliefs considered false. As we have seen, it is argued that all knowledge is dependent upon interests, not just that which is considered false. Moreover, interest theorists claim that they are not explaining away knowledge by adducing interests and therefore claim social interests are irrelevent to the perceived correctness of knowledge. (This kind of argument is developed more fully in the relativist programme discussed in the next section). Thirdly, and for the same reasons used to support the second tenet, explanations of scientific belief are applied symmetrically. The same kind of explanation is used irrespective of the truth or falsity of beliefs[101].

As before, I will not attempt to discuss critically the whole body of analysis linked to this perspective. Rather I will concentrate on a representative study. In their critiques both Woolgar and Yearley concentrate on Mackenzie's study of the role of interests in statistical theory[102]. This was chosen because of its sophistication and detailed documentation and because it is highly regarded by interest theorists. However, rather than produce yet another critical gloss on Mackenzie's paper I will apply a number of Woolgar and Yearley's points to Brian Wynne's paper: Physics and Psychics: Science, symbolic action and social contol in late Victorian England[103]. The ground for this choice are similar to those for choosing Mackenzie's paper: it is a detailed historical study which draws strong conclusions about the social determinations of an area of important natural scientific knowledge. It is taken to be exemplary of the use of interest theory by Bloor[104], and Shapin[105], and reproduced as such in Barnes and Edge's recent collection[106]. Moreover, in his recent book on Kuhn Barnes selects Wynne's study to present as a detailed exemplar - a paradigm case! It is, he claims, 'as good a model as any of what is needed'[107].

The central argument of Wynne's paper is straightforward. Wynne is concerned to show the role of social interests in the transformation of the concept of the 'ether' employed by the 'Cambridge physicists' during the last century. He wishes to demonstrate that their concept of the ether systematically reflected features of their social circumstances and, moreoever, was used as part of a moral discourse to legitimate their own social ideals. In particular, it is suggested that the Cambridge physicists' social interests led them to invert the traditional material theory of the ether and to produce an etherial theory of matter which held that the ether was a fundamental unifying entity underlying matter and giving coherence to a variety of physical phenomena.

Wynne claims that the etherial theory of matter mirrors the general social and religious beliefs of the Cambridge physicists which stressed the 'organic unity of knowledge, metaphysical realism, and the unseen world'[108]. Furthermore, these social beliefs are seen as opposing the fast growing secular ideology of scientific naturalism and individualism which, according to Wynne, was a byproduct of industrialisation and the increasing power of the bourgeois middle class. For Wynne, then, there is a two way causal connection between the different contexts in which the concept of the ether was used: its formulation was influenced by broader social concerns and it was also used to effect those very concerns[109].

I will discuss a number of interrelated issues in turn, each of which has parallels in Yearley's and Woolgar's critiques of Mackenzie. First of all I will take the question of Wynne's causal explanatory structure. As I have noted above, in their general, programmatic statements advocates of the Strong Programme tend to characterise the proper explanation of the content of scientific knowledge as causal. At the start of his paper Wynne follows suit by stressino that his study is aimed at elucidating 'causal connections' between social and scientific contexts in both directions[110]. He cites Mary Douglas in support of the general claim that the set of causes which will be found in nature is generated by an understanding of what is necessary and right in society[111]. However, in his analytic practice Wynne formulates these relationships in interpretative rather than causal terms. For example, he attacks the idea that there can be any necessary connection between ideologies and social practices, stressing instead the role of ideologies as social resources and the importance of the understandings which members have of these connections. Thus he suggests that although it is widely held that

> ideologies contain an intrinsic, ideal logic which leads inevitably to the related social practices... it is more valid to see them as weapons in an authority struggle with the ideological justifications of threatening social groups. (112)

In practice, then, Wynne takes the connections between systems of belief and activity to be interpreted according to the specific occasion of use; beliefs or systems of ideas are not seen to causally constrain actions in these detailed formulations[113]. Whenever they make use of this dual emphasis, interest theorists seem to be trying to have their cake and eat it. Their general epistemological warrant for the theory of interests is based on an assumption of direct causal connections between the interests of social groups and the content of natural scientific knowledge. Yet in their actual analyses (at least in Wynne's and Mackenzie's case[114]) this is dropped in favour of the empirically more fruitful interpretative formulation of the connection. As a consequence, the central explanatory notion which takes scientific knowledge to be a reflection of social interests becomes problematic.

A second difficulty becomes apparent when we examine the way Wynne prepares the way for a social explanation of the Cambridge physicists' scientific beliefs. One problem for interest theorists is to justify imputing social interests when purely technical accounts of the knowledge being examined are available. As we have seen, it might be argued that the technical standards <u>themselves</u> are related to social interests. However, it is not clear how this could be demonstrated in actual cases. Wynne's response to this issue is to suggest that the content of the Cambridge physicists' beliefs cannot be understood by reference to technical standards alone:

> this transformed conception [of the ether] is not readily intelligible as being "required by the state of experiment and observation" (115)

for all that they had a received concept of ether, the ether of the Cambridge physicists was essentially their own construction. And it was a construction which other physicists did not deem to be required by the technical state of their discipline. (116)

This characterisation paves the way for Wynne's social explanation of the content of these beliefs. For insofar as they cannot be understood in purely technical terms, it seems to follow that there must be a further process at work:

> Although the constructed ether and associated metaphysics of the "Cambridge School" are difficult to understand entirely in relation to the technical concerns of the esoteric scientific context, they are very readily intelligible in the more general [social] context of use. (117)

How is it that Wynne can claim that the Cambridge physicists' beliefs are not in fact a result of purely technical concerns? He is doing more than reproducing the implication from the Quine-Duhem thesis that any scientific evaluation is underdetermined by evidence. He claims a specific and important disjunction between 'technical conditions' and the actual transformation of the concept of the ether produced by the Cambridge physicists. One possible answer is implied by the extract cited above. In this Wynne suggests that it was 'other physicists' who saw a disjunction between the technical conditions of the discipline and the specific theoretical formulations of the Cambridge physicists. It thus appears that in his analysts' account Wynne has implicitly adopted the participants' evaluation of certain (anonymous) 'other physicists'.

It is important to note that as far as one can tell from the extracts quoted in Wynne's text the Cambridge physicists are not without arguments which they treat as providing a technical justification of their theoretical position. For instance, one extract stresses the role of the theory of the ether in simplifying important areas of physics. Moreover, the writer suggests that the concept would be abandoned if it ceased to provide this orderly, simplifying role.

> Our conviction of an orderly connection between things constitutes the conception of a cosmos... The only ground for postulating the presence of this medium is the extreme simplicity and uniformity of the constitution which suffices for its func-

> tions. Needless to say, there remain many unresolved features, some still obscure, but hardly contradictory. But should it ever prove necessary to assign to the aether as complex a structure as matter is known to possess, then it might as well be abolished from our scheme of thought altogether. (118)

This scientist goes on to emphasise the drawbacks of an alternative phenomenalist science which shies away from the postulation of generative mechanisms. This is echoed in the following extract by another scientist.

> Consequently, [Ostwald's] attempt to deal with nature in a purely inductive spirit is unphilosophical as well as unscientific. The view of science which he puts forward - a sort of well arranged catalogue of facts without a hypothesis - is worthy of a German who plods by habit and instinct. A Briton wants emotion - something to raise enthusiasm, something with human interest. He is not content with dry catalogues, he must have a <u>theory</u> of gravitation, a <u>hypothesis</u> of natural selection. (119)

This scientist can be seen to argue against inductivism in favour of a realist or hypothetico-deductive view of science which, shorn of its jingoism, would not be out of place in a modern context.

Clearly, then, technical or rational grounds <u>are</u> adduced (at least as far as the Cambridge physicists are concerned). Yet Wynne treats these as insufficient and adopts the evaluation of other (critical?) physicists who dispute the technical basis of the Cambridge physicists' theory of ether. In this case Wynne appears to have abandoned the impartial stance towards different scientific beliefs specified in tenet 2 of the Strong Programme. Indeed, he has produced a classic 'error account' which explains away false belief as a social product[120]. In doing so he has started to side with certain participants, accepting their evaluations but not those of the Cambridge physicists.

Wynne might have avoided this if he had looked more closely at the <u>organisation</u> of these different accounts of the inadequacy of the ether theory and examined the way the appearance of technical adequacy was <u>achieved</u> or <u>contested</u> in particular interpretative contexts. But he does not do this. Nor does he take seriously the possibility, implicit in the 'post-empiricist' philosophy of Kuhn, Lakatos and the rest, much drawn on by interest theorists, that the connection between technical criteria and specific theoretical formulations is itself interpretative. It is only through presupposino that he can formulate a definitive analysts' account of the connection between technical criteria and theory that Wynne's specific social explanation can begin to bite.

This raises the general issue of the way Wynne deals with participants' discourse. He uses a number of extracts from scientists' writings to document his claims. For instance, 7 extracts from the writings of Cambridge physicists are used to illustrate the nature of their broad theoretical beliefs[121]. However, the way this is done presupposes a highly oversimplified and asocial model of the workings of scientific texts. Extracts from different authors or different papers from the same author are combined to reveal a single theoretical position. These texts are treated as neutral documents which, when assembled, can disclose a coherent entity beyond them, namely the ether theory. Quite apart from basic analytic questions, such as whether these extracts are typical of the Cambridge physicists' work as a whole, or even of the particular author quoted, this technique presupposes that scientific writings are neutral documents which innocently depict theories, results, the actions of scientists, the beliefs of competitors and so on. Yet, as we will see, there is a fast-growing body of work which suggests tht this notion of texts is no longer adequate[122].

To take one example, Yearley has shown that there are systematic modifications made when scientific analyses are formulated in summaries or introductions[123]. And it may well be that Wynne has used extracts from broad formulations of the ether theory which have tended to emphasise its grander, more metaphysical aspects at the expense of its limitations and empirical uncertainties. We are not told if in other contexts the Cambridge physicists gave accounts which were more modest about its worth and more concerned with its consistency with experimental findinos.

The same sort of simplistic notion of textual functioning is apparent where Wynne documents examples of the ether notion being used as an 'explicit weapon'[124] against material cosmologies. This identification of the social functioning of ether theory is important because it is used to warrant the attribution of an implicit social function to the general theoretical writings of the Cambridge physicists. It is, however, very difficult to evaluate because it is presented almost entirely in paraphrased form.

Take the following extract from Wynne's paper.

> Stewart, an SPR [Society for Psychical Research] member, also wrote a paper which appeared as an appendix to Barrett's book On the Threshold of the Unseen (1895) in which he sought a "higher law" than the materialists could see in nature. It was a cardinal tenet that this "higher law" - of manifest moral significance - would make itself known in as (sic) experimental science, which was why the SPR's activities were "of unusual importance." Spiritual realities would be incorporated in a higher natural law, and thus be demonstrable through science. (125)

For Wynne this is a revealing example of ether theory being used as part of a legitimatory moral discourse. Yet, even in Wynne's paraphrase, it by no means forces a functional, legitimatory reading. It could easily be read, for example, as expressing touching faith in the possibility of 'spiritual realities' being scientifically demonstrated. It is not clear that because certain scientists can connect theoretical and deistic notions in this way that there were deistic motives behind the formulation of ether theory. Indeed, as we have seen, the interpretative connection between systems of belief which Wynne uses elsewhere in the paper questions the possibility of any straightforward causal influence across the two realms. This is not to claim that religious or similar notions could not have been involved in the formation of ether theory - it is merely to suggest that the kinds of homologies that are adduced between realms of ideas are not necessarily indicators of causal influence. Such homologies could equally stand as a testament to the interpretative skills of the participants in making different sets of ideas appear 'the same'. What for Wynne is an analytic resource used to justify the claims of causal connection could, for the participants, be the interpretative achievement of characterising religious beliefs as justified by scientific beliefs.

As a final issue I will examine more directly the 'work' done in Wynne's own text to achieve his coherent account of the social ideology, scientific beliefs and social positions of the Cambridge physicists and the way these differed from the scientific naturalists. I will consider the way Wynne constructs the different 'sides' of the debate. The notion that there existed two coherent sides is absolutely fundamental to Wynne's analysis. However, they are by no means empirically self-evident entities. As space is short I will concentrate only on the identification of the membership and not consider the important issue of the identification of the content of the scientists' beliefs[126].

Wynne's construction of the membership of sides depends on a variety of what I will call 'homogenising devices'. These can be thought of as membership descriptions which are used in an unselfconscious manner to warrant similarity of beliefs and interests. The most fundamental of these in Wynne's text is the category 'Cambridge'. Teaching or working at Cambridge is taken to be a strong warrant for the possession of a certain set of social beliefs, even for those physicists who subsequently left Cambridge. This social ideology is explicitly documented only for the 'Cambridge intellectuals' Maitland, Seeley, Sidgwich and Maurice[127]. The analysis depends on this set of beliefs being shared by the 'Cambridge physicists'. Yet these beliefs are not documented for the physicists themselves. Rather it is assumed that as they too were at Cambridge, they must share the same beliefs. Cambridge is thus taken to be an undifferentiated, unified whole rather than an internally structured grouping, whose members have conflicting beliefs and interests. Furthermore, early in Wynne's text he notes specifically that proponents of scientific materialism (the position the Cambridge physicists were supposed to be implicitly attacking) were to be found within Cambridge itself[128]. Yet this allusion to fragmentation does not prevent Wynne from assuming its homogeneity elsewhere in the text.

In the same way, a whole set of homogenising resources are used to sustain the identity between Cambridge physicists and supporters of the Society for Psychical Research (where it is alleged that the ideologically potent features of ether theory are most apparent). However, these varied resources - mainly the role relationships: friends, colleagues, research assistant/researcher, between Cambridge physicists and SPR supporters[129] - are assumed in an uncritical way to indicate identity of beliefs. It is important to ask if these relationships do indicate such an identity and, moreover, whether there were not similar relationships between Cambridge physicists and people of a more scientifically naturalistic bent; a question which Wynne's text nowhere addresses.

Finally, there are indications in the text that the Cambridge physicists are not such a homogeneous group as is repeatedly suggested. For instance, Lodge, who is quoted more than any other single physicist as exemplifying support for the ether theory combined with an interest in psychical research, is described as having 'utilitarian leanings' which contradict the spiritual, organic emphasis of the other Cambridge physicists[130]. Unless we are to assume that he is engaged in an implicit attack on his own political beliefs, we have further evidence here of the interpretative connection between sets of beliefs as well as the fragility of Wynne's social categories.

The Relativist Programme

In this fourth section I will examine a perspective on scientific knowledge which its author refers to as the 'empirical relativist programme'[131] or 'special relativism'[132]. Collins argues that such an approach is a necessary prerequisite for the study of social processes in science. He maintains that any non-relativist programme will end up explaining what went wrong in cases of beliefs considered false, and how processes of knowledge production worked smoothly in cases of beliefs considered true. Such approaches, claims Collins, assume that what is taken to be valid scientific knowledge needs no sociological explanation; scientists' acceptance of such knowledge is thought to be adequately explained by the nature of the phenomena under investigation. The ultimate function of any such programme will therefore be to legitimate any current scientific status quo.

The correct approach, according to Collins, is to assume that 'the natural world in no way constrains what is believed to be'[133]. This means that scientists' claims about the truth or falsity of different accounts of the natural world cannot be used by the analyst as a resource for the construction of sociological explanations. Instead, if they are considered at all, they must be treated as a topic to be explained. What is of interest to the analyst is not the natural phenomena themselves, and not the quality of any explanations of the phenomena, but participants' beliefs about the phenomena or about the quality of explanations and, furthermore, the effects of different scientists' actions on those beliefs[134].

Collins argues that this approach is a necessary corollary of tenets (2) and (3) of the Strong Programme which was discussed in the previous section[135]. For, if the sociologist is to be impartial with respect to the truth or falsity of scientists' beliefs, and approach each symmetrically with the same style of explanation, no use can be made of the natural world in constructing explanations, nor of the related concepts of truth, rationality and scientific progress. If such notions did enter the sociologists' explanation as resources then it would cease to be impartial and symmetrical, because one set of beliefs would have been given priority over another[136]. Such categories can only be used as a part of a sociological explanation when they are treated strictly as actors' categories which the analyst wishes to explain. As Collins puts it:

> This is not to say that we must eschew all mention of truth, rationality, success or progressiveness, but only that any such mentions must be made in such a way that they are applicable symmetrically to that which is false, irrational, unsuccessful and degenerative. This is possible if the categories are only mentioned and treated as actor's categories. (137)

This, then, is Collins's basic theoretical and analytic premise, which he takes as underpinnino his research programme[138].

Collins suggests that the activity of scientists falls into three distinct classes: that where there is a high degree of consensus over methods, problems, findings and so on (which Collins identifies with Kuhnian 'normal science'[139]); that where scientists attempt to undermine the consensus and propose an alternative set of methods, problems etc.; and that where scientists attempt to make partial changes in the consensual structure, and consequently become embroiled in controversy[140]. It is the last of these which is seen as the most fruitful as far as sociological research is concerned. For in this case all the taken for granted rules and practical competences on which scientific activity depends are thrown into question. This has the twin payoff that these rules and competences are more likely to be explicitly formulated by participants, and therefore are more available for study, and also that the researcher is better able to maintain the relativistic attitude of indifference to the way things 'really are' because this will be anyway in dispute. I will later suggest that both of these justifications are suspect. For the moment, however, let us examine in more detail the way Collins sees the specifics of the relativistic study of science.

Collins treats the study of controversy in science as falling into three discrete stages[141]. The first two are the most significant for, as Collins notes, the third is only a conceptual possibility at the present time. Studies which move through stages one and two are typical of this research; Collins describes them as follows:

> The first stage is the empirical documentation of the interpretative flexibility of experimental results. This part of the work has shown what part experimental data plays in the practice of science, and what part is played by the touchstones of certainty such as replication. The second stage... is concerned with the way that the limitless flexibility of data are closed down. The mechanisms of closure have been found to include various rhetorical presentational and institutional devices working within a context of 'plausibility' and other conservative forces. (142)

What exactly is involved in these two stages is clarified in an exchange between Collins and Knorr-Cetina[143]. She suggests that Collins's approach has converged with that of certain modern philosophers of science (notably Mary Hesse[144]) who suggest that although no <u>single</u> experiment or finding will be beyond criticism, rational scientific choices can nonetheless be made on the basis of an <u>accumulation</u> of experimental data along with criteria of <u>coherence</u> with other findings and theories[145]. In reply, Collins acknowledges that his own work, along with that of other authors working within the relativist programme, <u>does</u> illustrate the importance of accumulations of evidence and coherence with existing programmes in restricting scientific action; yet he maintains it shows more than just this. It shows that the role of social and political interests is also crucial in limiting scientific debate. For instance he describes his later work on gravity waves as stressing,

> that accumulation of experimental results was not, and could not, be decisive in settling the controversy. Here we see that a variety of political and rhetorical strategies were mobilised to end the debate. (146)

Collins accepts that Knorr-Cetina would have been right if his study and ones like it had been documenting just the significance of coherence and accumulated evidence. It is clearly essential for Collins's position, therefore, that he satisfactorily document the constraining function of these shared, extra-rational factors on scientific activity. So let us now examine in more detail the example that Collins cites above as doing exactly this.

Collins's paper Son of seven sexes: The social destruction of a physical phenomenon[147] is a continuation of his earlier research on gravitational radiation and the claims of Professor Joseph Weber to have detected it under experimental circumstances[148]. In this later paper the emphasis is on the way scientists in the field of gravity radiation came to disbelieve Weber's claims to success. The aim of the paper is,

> to show that there were no purely cognitive reasons that would 'force' scientists to disbelieve Weber's claims. Their incredibility is a social product - though they are none the less incredible for that. (149)

In the light of Collins's response to Knorr-Cetina we are able to disregard a large part of the paper, which is concerned with documenting the facts that no single study is taken unequivocally to undermine Weber's claims and that the accumulation of different experimental findings[150] along with the lack of coherence of Weber's findings with the predictions of relativity theory[151] is central to the change in belief among scientists in the field. We need not examine these findings because, as Collins himself notes, they are not what is crucial for deciding whether his own relativist position is supported or not. Instead, let us concentrate on his documentation of the 'political and rhetorical strategies' used by participants, for it is these that are at the crux of his claim that 'the physics and politics of experimentation are not seperable'[152] and that extra-scientific means must be used to end scientific debates[153].

For us, therefore, the crucial part of the paper is the section where the responses of a scientist called Quest to Weber's work are described. It is Collins's explication of these responses which constitutes the second stage of his relativistic analysis; for he sees their function as being to close down the potentially limitless debate over the status of gravity waves.

> Quest acted as though he did not think that the simple presentation of results with only a low key comment would be sufficient to destroy the credibility of Weber's results. In other words, he acted as one might expect a scientist to act who realized that simple evidence and arguments are not sufficient to settle unambiguously the existential status of a phenomenon. There is no reason to think that Quest was unsuccessful in his aims... (154)

Collins's argument thus becomes dependent upon two separate claims: firstly that Quest's papers did 'settle unambiguously the existential status' of the phenomenon of gravity waves and, secondly, that they did so not through mobilising arguments and data but through 'rhetorical' presentation and utilisation of 'popular' outlets. Each of these claims becomes suspect in the light of a careful examination of the examples of participants' discourse cited in Collins's paper. Moreover, as we will see, Collins can only sustain his version of events by a process of selectively ironising and reifying this discourse. I will illustrate this for each of these basic claims in turn.

Let us take the suggestion that Quest's papers were significant in ending the controversy first[155]. Collins's data in support of this claim are rather thin. Although in a number of places in his text Collins asserts that this was the case, his documentation amounts to three short extracts from interviews with gravity wave experimenters. Before discussing these it is important to digress for a moment to note how Collins intends such extracts to be taken. They are not so much intended to be data, but 'dramatic indicators and aids to communication'[156], and they are meant to function as illustrations of wht the social researcher has come to understand through participating[157] with the scientists he is studying. That they are intended in this way, however, makes it all the more surprising that the extracts implying that Quest had a significant impact are balanced out by extracts which queried his importance.

In the following interview fragment Quest's actions are described as unscientific and to constitute 'going after' Weber,

> I felt that it spoke for itself, and that those few people who knew about it were enough. But Quest did not feel that way and he went after Weber... and I just stood on the sidelines covering my eyes because I'm not really interested in that kind of thing, because that's not science. (158)

Collins also describes how other scientists claimed they were tempted to disregard Quest's experimental results because Quest embarked on this as a sort of holy crusade[159] and that Quest was a 'dangerous man'[160]. And he gives a further example of a scientist critical of Qust's action,

> [Quest and his group] are so obnoxious, and so firm in their belief, that only their approach is the right one and that everyone else is wrong, that I immediately discount their veracity on the basis of self delusion. (161)

Weber himself, of course, is shown as disagreeing with Quest's position. Interestingly, however, the largest part of Collins's discussion of Weber's reply to his critics is not taken up with his response to Quest. Indeed Quest is mentioned just once in the entire discussion of Weber's responses. The majority of Weber's comments concern the experiments on gravity waves which, according to Collins, were accepted as satisfactory by all of Weber's critics. This seems rather surprising if Weber believes that it is Quest's work which is crucial in closing down the debate and settling the existential status of gravity waves. Surely if that were the case Weber would have attempted to undermine this work above all. Moreover, it is hard to believe that the other scientists mentioned by Collins are both highly critical of Quest <u>and</u> fully persuaded by him! As Collins himself notes, Quest's work was 'nearly always discussed with reservations'[162].

It is of course <u>possible</u> that these scientists were persuaded by Quest, and that his work <u>did</u> decide the status of gravity waves by closing down the debate; yet Collins's selections from scientists' talk are very far from exemplifying this in an unambiguous fashion. Some extracts seem to suggest the debate is closed down and others that it is not. Others seem to suggest that, insofar as the debate is closed, it has been made so by contributions quite separate from those of Quest. Collins gives no criteria which justify his readings or, as he would have it, his participants' knowledge that Quest's papers had the claimed effect. Indeed, he does not even clearly demonstrate that there was 'almost universal disbelief'[163] in Weber's claims[164]; even though this is the central platform on which his explanation is erected.

What about Collins's second claim, that it was the rhetorical, extra-scientific features of Quest's work which were crucial? For, as we have noted, even if Quest's work was persuasive Collins's argument rests on the suggestion that it was more than its scientific merit that made it so. This immediately seems to conflict with the participants' claims (mentioned above) that Quest's 'holy crusade' made them wary of, or even reject, his experimental results. In these cases Collins's suggestion is inverted: instead of experiments being a vehicle for powerfully persuasive rhetoric, Quest's manner of presentation is treated by members as casting doubt on the veracity of his experiments. Nevertheless, let us examine Collins's evidence for his suggestion.

It is worth discussing in some detail the three extracts from interviews (mentioned above) which are taken by Collins to show that Quest's work had a high impact 'because of the way it was presented'[165].

> ...as far as the scientific community in general is concerned, it's probably Quest's publication that generally clinched the attitude. But in fact the experiment they did was trivial - it was a tiny thing... but the thing was the way they they wrote it up...
>
> Quest had considerably less sensitivity so I would have thought he would have made less impact than anyone, but he talked louder than anyone and he did a very nice job of analysing his data.
>
> [Quest's paper] was very clever because its analysis was actually very convincing to other people, and that was the first time that anybody had worked out in a simple way just what the thermal noise from the bar

should be... It was done in a very clear manner, and they sort of convinced everybody. (166)

Collins uses these quotes to show that the impact of Quests's study was due to its form of presentation rather than its scientific content. Yet, how is the analyst to decide what is the form of the presentation and what the scientific content? The extracts makes reference to things like 'the way [Quest] writes up', and that Quest 'talked louder than anyone'. However, they also note Quest's 'very nice job' of data analysis, his clarity and his novel solution to a particular problem. Are these things necessarily part of a paper's form rather than its content? Collins certainly takes them to be so but - and this is the crucial thing - how do the participants view them? Are there tacit criteria (as Collins appears to assume) for dividing form from content in this field? Collins's model of closure demands that <u>extra</u>-scientific, rhetorical devices were mobilised; however, he is nowhere explicit about how this category of 'the rhetorical' is arrived at. If it is a participants' category we need to know if it is consensual; if it is an analysts' category we need to know how it can be impartially and symmetrically applied.

There is a further interesting feature of these extracts. Each of them is concerned with the effect of the paper on <u>other</u> scientists. The speakers do not characterise <u>themselves</u> as taken in by what they see as the papers' style. We therefore have to take on trust that these speakers can give an accurate social account not only of the influence of the papers on a large number of other scientists but also of <u>exactly what feature</u> of the papers was responsible for the influence. Of course, Collins treats such extracts as examples of his own participant's understanding; yet this must have been generated with the aid of just such accounts. What Collins does not do is treat them as utterances constructed to perform particular interpretative tasks. For instance, these speakers may be engaged in 'accounting for error'[167] (a notion that we have seen in the previous section and will meet again on a number of occasions in coming chapters). In this case the error account might be used to make sense of the conflict between what the speaker views as the triviality of the work and the fact that it became widely accepted. This potential conflict is eliminated by deploying the notion that the work, though trivial, is packaged in a way which is rhetorically very effective. I will not elaborate on this suggestion here; all I wish to indicate is that there are ways of analysing such accounts which are not dependent upon extrapolating from them to the realities of the scientists' social world.

If we return briefly to Collins's rationale for the study of controversies, it can now be viewed rather differently. It is possible to see that what may be interesting is not that they make normally tacit rules explicit, but that controversies occasion a more elaborate repertoire of accounting procedures. For example, there may be little need for error accounting except when there is conflict between scientists' claims. One danger for Collins's approach is that it reifies occasioned practices of accounting as normally tacit rules of conduct. Moreover, although the conflicting views of participants in controversies may attune researchers to the contingency of scientists' beliefs, it may also obscure the analytically important difference <u>within</u> the discourse of individual scientists.

The most significant implication of the way Collins deals with participants' discourse within his own relativistic framework, is that it actually leads him away from a neutral, relativistically indifferent stance with regard to questions concerning the real existance of gravity waves. To accept evaluations of experiments in the way he does, as providing neutral indices of the experiments' worth, amounts to the same thing as evaluating the relative worth of two different accounts of the natural world. To paraphrase one of Collins's own conclusions from the earlier gravity wave work[168]: negotiations about the value (rhetorical or genuine) of a particular experiment are, ipso-facto, negotiations about the character of gravitational radiation. Thus, when he accepts certain participants' evaluation (that Quest's work was trivial but rhetorically effective) he is inevitably starting to make judgments about the natural world. Collins's social version of events in the field, then, cannot be consistently separated from participants' own versions of how to allocate truth, rationality and scientific progress. As long as Collins attempts to 'read through' participants' accounts to one single definitive version of actions and beliefs in the field he is forced both to ironise and to reify members' accounts; thereby tacitly rejecting and appropriating the positions produced by various scientists on specific occasions.

Let us take one final example from Collins's paper to clarify this. Collins notes that Quest's group performed a later experiment with slightly different equipment to the earlier ones. He describes Quest's rationale for this as being to utilise available equipment in the most profitable way. However, one of Quest's co-workers gives a rather different justification:

> we just felt that we hadn't been convincing enough with our small antenna. We just had to get a step ahead of Weber and increase our sensitivity too. At that point it was not doing physics any longer. It's not clear that it was ever physics, but it clearly wasn't by then. If we were looking for gravity waves we would have adopted an entirely different approach...
> there is no point in building a detector like Weber's other than the fact that there's someone out there publishing results in Physical Review Letters... (169)

Collins clearly treats Quest's own account as a defensive gloss, which does not reveal what the research was actually intended for. He concentrates on the implications of the alternative account, treating it as an accurate characterisation of Quest's research[170]. Collins concludes 'it can be said with some degree of certainty that Quest and his group set out to kill Weber's findings in the shortest possible time'[171]. In this case, then, Collins ironises Quest's account while reifying an alternative in order to maintain his own specific conclusion and thereby to support his general analytical position. Yet he offers no criteria for his contrasting practice in interpreting accounts (except, perhaps, his analytically unavailable 'experience' of the field). Moreover, when Collins accepts the account which (he claims) depicts the work as unscientific he begins to accept certain claims about the natural world and how it is, or is not, revealed in the Quest group's experiments.

To put this another way, the analysts' ability to decide whether Quest's experiments are scientific nor mere vehicles for rhetoric depends on his specifying further features of the situation and, in particular, beliefs about the reality with which these experiments are concerned. Thus when Collins decides that these experiments have rhetorical rather than scientific significance he implicitly sides with supporters of certain beliefs against the supporters of alternative beliefs; that is, he treats some beliefs as literally constitutive and others as not. It is impossible for him consistently to maintain his neutral, relativistic stance <u>and</u> give definitive social accounts of actions and beliefs in his area of study[172]. Ultimately, then, his analytic practice ends up in subverting exactly the main pillar of justification of the relativistic approach, namely that it should remain neutral with regard to the status of different accounts of the natural world.

At places in his work it appears that Collins has started to recognise this problem. For instance, he has characterised the goal of relativistic analysis as being to 'describe what kind of talk is reasonable talk in the [scientific] society in question'[173] and to elucidate 'what was <u>counted as</u> acting rationally'[174]. Furthermore, he has claimed that instead of treating beliefs as actually being of such and such a kind - out of touch, say - 'attention [should be] immediately shifted to the ways that competing sets of beliefs are made to <u>appear</u> out-of-touch or unprofessional'[175]. At the same time he argues that the investigator cannot concentrate on those beliefs held by the most prestigious members of a scientific community; impartiality with respect to such things is essential otherwise the research may simply naturalise a prestige relationship which is actually a social construct[176]. If Collins had followed these suggestions through in practice he would have arrived at a position very similar to the one advocated in this thesis. His concern would have been with methods of sense making and systems of accounting.

Why, then, if Collins is suggesting this approach to scientists' social life, does he need to make the radical distinction between the way the sociologists must view the physical and social worlds[177]? Why could not Collins approach the social world in the same relativistic fashion as he does the physical? If he had stuck to the questions concerning the way beliefs are made to <u>appear</u> out-of-touch and so on he could have done so. However, as we have seen, in practice Collins wants to do more than this; he wants to provide definitive accounts of particular social events which go beyond the perceptions and characterisations of the participants and to claim to reach the actual social processes themselves. Instead of documenting the way that, say, Quest's work was <u>depicted as</u> rational or rhetorical by different participants in the course of accounting, Collins attempts to make much stronger claims which treat certain actions as <u>in fact</u> rhetorical and others not.

Collins's radically different approach to the physical and social worlds is intended to allow a naturalistic approach to social life to be combined with a relativistic approach to physical reality. Yet, as we have seen, the distinction cannot be maintained: as he starts to make definitive characterisations of social processes he ceases to remain neutral with regard to the natural world. He cannot decide that an experiment was really done for rhetorical purposes <u>and</u> claim to be suggesting nothing about its scientific quality. At present, then, although some of Collins's theoretical claims suggest a more consistent analytic practice, his research embodies fundamental inconsistencies which undermine its relativism.

A Systematic Analysis of Scientists' Discourse

In the first sections of this chapter I have outlined four very different methodological and epistemological positions and discussed the various different ways in which their conclusions become dependent on participants' discourse. Because this discourse varies systematically in accordance with changes in interpretative context, it is possible for analysts to produce conclusions which are plausibly based upon that discourse, yet which are radically different. I have suggested that the conclusions of these analyses are open to a range of objections, many of which derive from analysts' failure to deal in a satisfactory manner with the interpretative work embodied in scientists' discourse. I will first summarise these objections for each of the approaches in turn and then outline an altternative perspective which is intended to respond to these problems.

In the case of WSB, I have suggested that their analysis is composed of three elements which are taken over from participants' discourse: sometimes they adopt the terminology and the accounts of action and belief explicitly provided by scientists in the formal literature; at other items they adopt the notions of cognitive interdependence which are taken to be implicit in scientists' patterns of citations; and at certain important junctures they re-state what they take to be the general view of the field as expressed in participants' empiricist folk-history. The central defect in their analysis is that it is participants, rather than analysts, who carry out crucial parts of the sociological interpretation. The major contribution made by the analysts is that of selecting out and ordering a limited class of interpretative material taken from the full range of such material actually generated by participants.

Because WSB base their analysis primarily on the discourse characteristic of the formal scientific literature and because they consider only those social actions which participants subsumed under the formal concepts of 'theory' and 'experiment', their interpretation closely resembles the formal scientific literature in eliminating virtually all reference to personal or social contingency. As a result, their version of events inevitably provides a rational reconstruction of scientific development which, although it differs in detail from that of Lakatos, continues to represent science as a self-contained, progressive and internally coherent endeavour. This view of science is given its ultimate validation by reference to scientists' own accumulated wisdom.

However, the apparent plausibility of WSB's analysis is achieved only by ignoring certain important aspects of scientific discourse. In particular, WSB undertake no systematic examination of scientists' informal discourse, in the course of which quite different, and often highly variable accounts of action, belief and scientific development are likely to occur. Furthermore, WSB make no allowance for the fact that scientists' accounts vary from one social context to another; for instance, from the formal to the informal context. Once these features of scientific discourse are acknowledged, WSB's conclusions come to be seen as essentially a by-product of the analysts' highly selective adoption of one context-dependent form of scientific discourse.

Although the conclusions of the constructivist researchers Latour and Knorr-Cetina are very different from those of WSB, and although their methodological approach to the social life of scientists contrasts markedly with that adopted by WSB, their research is also flawed by fundamental problems associated with the use of participants' discourse. Latour and Knorr-Cetina have made much of the supposed methodological value of direct observation of scientific activities as they happen in the scientific laboratory. In their emphasis on observation they appear to be formulating a considerably more critical approach to participants' discourse than WSB's. Indeed, they stress the difficulty of producing descriptions and explanations of scientific activities because scientists often provide conventionalised reconstructions or self-serving rationalisations of events. And they display considerable scepticism about the veridicality of accounts of activity embodied in the formal scientific literature. Yet, having addressed this problem and stressed its seriousness, their attempt at a solution is inadequate.

In the first place, their proposal to replace indirect indicators of action and belief with direct observation is unworkable; and in fact they accept in practice that scientists' accounts are essential for the interpretation of observations. Thus the latter cannot be used as an unproblematic data base. Moreover, very little of their empirical analysis is based directly on 'observational' data. For the most part verbal or written traces of one kind or another are drawn upon. In other words, despite Latour and Knorr-Cetina's evident recognition of the shortcomings of using discursive data as a basis for the construction of definitive accounts of scientists' activity and belief, this is often exactly what they attempt to do.

This vacillating over the status of observations is compounded by Knorr-Cetina when she treats the metaphor of knowledge manufacture as if it were literally true. Despite attacking models of science which take it to be observational or descriptive in essence, she continually characterises her own practice in visual terms; she takes ethnographic observation of scientists in the laboratory to capture the manufacture of knowledge 'on the spot'. The implication that there is a single, identifiable site for manufacture leads to a highly asocial conception of scientific knowledge, which emphasises those events occurring within the laboratory at the expense of broader social processes.

Like the approach developed in this thesis, Latour stresses the context dependence of scientific claims. However his notion of context dependence is very limited. Indeed, he appears to have taken the notion over from the participants' classifications of their fields into conventionalised research units. His account of the way meaning changes across contexts thus becomes no more than a redescription of the differences between the aims of competing research groups. If Latour had examined endocrinologists' discourse more critically he might well have developed a more sophisticated notion of context and even demonstrated change in the way the meaning of the molecule was characterised. However, he failed to take this opportunity, and his findings remain crucially dependent on participants' own self images.

Unlike WSB, Latour and Knorr-Cetina place much of their emphasis on informal accounts, that is on interviews and conversations with single scientists, and participants' descriptions of events which occurred within the laboratory. In so doing they start to build up a picture of scientific activities which differs significantly from that of WSB. This picture stresses the role of socially contingent factors in the production of scientific knowledge and the way ideosyncratic, localised events impinge upon this process. Even where informal accounts which stress the order and logic of the process are given they are not taken to reveal the genuine organisation of scientific procedures. It is therefore possible to see the difference between WSB's and Latour and Knorr-Cetina's analytic conclusions to be a direct function of the different forms of accounts on which their studies are based. In each case the analytic findings become dependent on the participants' own interpretative work and the way this is organised according to the specific context of discourse.

Despite marked theoretical contrasts, and disagreements over methodological issues, Wynne shares many features of Latour's and WSB's flawed approach to utterances and writings. In the first place, Wynne's general analytic goal is to provide a causal explanation of a specific set of scientific beliefs in terms of the social interests of the scientists who hold them. However, he takes note of the shortcomings of traditional sociological explanations which infer causal links on the basis of analytically adduced homologies between the structure of scientific beliefs and the ideology or interests of social groups. He therefore emphasises the relevance of scientists' interpretative accounts to his attempts to identify actual connections between social groups, their ideologies and the content of bodies of technical knowledge. Yet if he had done this systematically he would have been forced to abandon his general causal approach. For there is no necessary relation between participants' accountino and actual causal processes. Indeed, scientists' accounts of the connection between background culture, social interests and so on and scientists' beliefs are highly variable. Only by a process which selectively ironises certain accounts and reifies others could any semblance of a causal model be maintained.

This, however, is exactly what Wynne does. He treats certain accounts as if they were literal descriptions, others as if they embodied implicit social ideas and certain accounts are disregarded altogether. Thus Wynne clearly treats certain physicists' descriptions of what the 'technical state of the discipline requires' as literally correct and incorporates them into his analytic account. By treating certain participants' accounts as literally correct, Wynne is able to discount or to ignore those other accounts which suggest an entirely rational (non-social) explanation for the beliefs in question.

In the course of his analysis Wynne treats a set of discrete texts as revealing a single theoretical perspective and claims a coherence in participants' beliefs on the grounds of their shared place of work (Cambridge), relationships of friendship, and other associations. Again, such an analytic procedure is suited to producing apparently clearcut and consensual analytic entities (theories, social groups, etc.) which might be seen to stand in an orderly causal relation to one another. Yet such an analysts' construction is only possible through an extreme simplification of varied participants' accounts. For instance, Wynne uses a number of extracts from physicists' writings to document the nature of the ether theory. Each is treated as a neutral document to the genuine nature of the theory. However, this procedure ceases to be satisfactory if we allow that different versions of a theory may be formulated in different contexts; stronger versions in summary conclusions than in results sections, say. Once we start to take all participants' discourse equally seriously, and not attempt the futile task of sifting the literal from the rhetorical, the possibility of clearcut, definitive explanations fades into the distance.

Like Wynne and Latour, Collins draws upon a much wider range of discursive material than WSB. He also recognises the constructed and occasioned nature of participants' discourse. Nevertheless, instead of treating all discourse in the same even handed fashion - which he might have been expected to do if he had taken his general relativistic premise seriously - he adopts an innappropriate form of analysis which leads him to accept certain participants' accounts as literal. These become a basis for Collins' analytic conclusions.

The way Collins selectively deals with participants' discourse to enable him to construct a definitive version of the effect of social processes on scientific events is very like the approach taken by Wynne. Collins adopts as quite literal certain scientists' third person accounts of the role of social processes in a widely distributed social network, while rejecting certain other scientists' first person accounts, which explain the same knowledge in rational terms. In this way Collins comesto reify certain structured variations in scientists' discourse, as did the researchers discussed earlier in this chapter. His approach can be seen to make the inverse error to WSB who simply adopted scientists' formal, first person accounts along with a version of participants' folk history of their field. In each case insufficient attention is paid to the interpretative functions of accounting.

Collins, of course, does suggest a criterion for the choice between alternative accounts, which is his 'participants' experience' of the field. He suggests that it is this which enables him to divide accounts into the true and false, literal and rhetorical. However, not only does this raise the general issue of how research based on a completely unavailable entity such as this may be assessed, but it can also be seen to be unsatisfactory in practice. For, as we saw above, even the quotes from participants which Collins uses to typify particular findings are open to varied and contradictory readings.

Despite this, many of Collins's broader analytic claims are closer to the position adopted here than either Wynne or Latour and certainly WSB. He places considerable stress on the centrality of participants' own categories and interpretations and the way their discourse is organised to create certain kinds of effects. If these suggestions had been consistently followed through his research would have been quite compatible with that advocated here. Unfortunately, in his attempt to produce strongly social explanations which connect contingent social events to the formal constitution of knowledge, Collins is forced to reify and ironise scientists' own interpretative work and he ends up far away from his relativist ideal.

Let me now draw out some of the more positive implications of these points. The goal of my discussion of these studies has not been merely to demonstrate their inadequacies as such. My main concern has been to elucidate certain fundamental and unresolved methodological problems with the way social studies of science have dealt with participants' discourse in its varied kinds. Some of the main conclusions of this discussion can be summarised as follows.

1) In each of the studies discussed above scientists' discourse in one form or another constituted the fundamental analytic resource.

2) The social researchers' conclusions and analyses were built up in part from participants' <u>own</u> interpretative work which was adopted without serious critical examination.
3) In no case was sufficient attention paid to the variability of scientists' discourse and the manner in which it is socially generated.

The overall implication from this discussion, then, is that there is a need for a systematic and rigorous investigation of the way scientific discourse is produced in different social circumstances. Indeed, insofar as research <u>throughout</u> the various approaches to the social study of science is flawed by an inadequate and unexamined approach to discourse, such an investigation will be an essential precursor to more traditional forms of research. It remains to be seen how far such an investigation would eventually place this traditional research on a firmer empirical basis or if it will only provide an even more comprehensive demonstration of its essential misconception. As long as analysts continue selectively to adopt scientists' own interpretative formulations, and as long as they continue to disregard the contextual dependence of scientific discourse, they will fail to provide viable descriptions or explanations of scientists' acts, beliefs and social circumstances.

The most fundamental shift adopted by this new approach is - to use a rather well worn phrase - that scientists' discourse is treated as a <u>topic</u> rather than a <u>resource</u>. In practice, this means that when an issue such as social categorisation is dealt with, instead of treating scientists' categorisations as more, or possibly less, accurate descriptions of actual scientific groupings, they are examined in terms of their construction, organisation and function. That is, we start to ask: how are certain textual formulations made to appear as literal descriptions of categories of scientists?; what different kinds of categorisation accounts are there and in what interpretative contexts are they used?; and what consequences do different kinds of accounts accomplish?

The significance of these questions is that they imply a different approach to the data on which all forms of analysis are ultimately based: scientific discourse. In those studies discussed in the early sections of this chapter discourse is treated as a pathway to the actions, beliefs, events, etc. which are considered to be the real topic of analytic interest. The discourse itself is only considered interesting insofar as it provides an indirect discovery technique. However, with the kinds of question which are the concern of discourse analysts it is the discourse <u>itself</u> which is the central topic for study. Language is treated as its own reality rather than as a medium through which reality must be teased. This is not to say that scientific discourse is somehow transparent or immune from interpretative disputes[178] - indeed such disputes are exactly the analytic topic of chapter 7 of this thesis[179]. Rather, this approach is meant to avoid unwarranted extrapolation from scientists' discourse to the 'entities' - theories, choices, ideas, interests, paradigms, etc. - which it perpetually formulates.

A fundamental assumption of this approach is that scientists' accounts are highly variable; specifically that they vary in accordance with the constraints and opportunities provided by particular interpretative contexts[180]. This, of course, is a claim with empirical implications. I have documented in the above discussion of more customary analytic positions a number of instances of significant variability in the scientists' discourse which is quoted or referred to. However, this does not show the full extent of this variability, nor the regularity of its organisation. In the chapters to come I will document systematic variation not only <u>between</u> different scientists - which is quite unexceptional in controversal fields - but also <u>within</u> the discourse of single scientists. The approach adopted here does not presuppose the homogeneity of individuals' beliefs and actions as they appear in their accounts. On the contrary, it is suggested that the same scientists will characterise scientific beliefs, actions, the nature of theories and so on, in systematically varying ways according to the specific interpretative task at hand.

Furthermore, the same goes for the texts of a particular author or even an individual scientific text. In each case significant variability in accounting has been documented[181]. It cannot be assumed - as do all the analysts discussed above in one way or another - that texts or bodies of texts are seamless, unified, coherent entities; or that those textual features which are problematic can be excluded as inconsistent with the essential or overall meaning of the text. Of course, as we have seen the flexibility of ordinary language is such that a unitary version of a text or an event can always be produced[182]. However, a close analysis of the procedures used to do this - such as that I have begun to carry out on the studies above - will reveal the selective process of reifying and ironising scientists' accounts and the unexamined dependence of analysts' claims on participants' repertoire of sense-making resources.

One of the main concerns of participants is with the creation and sustenance of coherent versions of their social worlds suitable for specific interpretative contexts. They draw upon their repertoire of available resources accordingly. Yet as analysts have customarily adopted this same goal (in some form at least), they end up competing with scientists themselves on more or less their own terms. Analysts may legitimate their activity by describing it as social research on science but the fact remains that their techniques and procedures are glorified versions of those of the actors they are supposedly studying.

Discourse Analysis and Ethnomethodology

So far I have been concerned to show how a programme of discourse analysis arises directly from the inadequacies found in more traditional studies of science. At its most basic, it can be argued that a more careful examination of discursive data coupled with a consistent adherence to the tenets of symmetry and impartiality requires a systematic analysis of discourse. The careful examination of discursive data would acknowledge its pervasive variability; and a rigorous impartiality would prevent the treatment of certain accounts as more true than others. Furthermore, taken seriously, impartiality would prevent the analyst elevating any one accounting context - the formal research paper, say, or the 'revealing' informal interview - over others - coffee table anecdotes, perhaps, or scientific jokes. Discourse from any of these contexts would be treated as an equally viable account, from an analysts' point of view, of scientists' actions and beliefs. The logic of this process can be clearly seen in Collins's 'TRASP' paper, to which I referred earlier, in which he generates a position very close to discourse analysis by direct extrapolation from tenets 2 and 3 of the Strong Programme. It is only his desire for a more definitive (but therefore more partial and less relativist!) story which eventually prevents him from arriving at a full blown programme of discourse analysis.

In addition to the emphasis in the foregoing discussion on the necessity for discourse analysis as a response to problems inherent in more traditional studies of science it is also compatible with, and indeed may be warranted by, a number of recent developments in sociolinguistics, semiotics and ethnomethodology. This is not the place for an elaborate theoretical discussion which would fully document the areas of agreement and disagreement. In later chapters I will draw upon and discuss some of the theoretical notions of Halliday and his followers and use certain of the concepts developed by the continental semiologist Barthes. To end this chapter, however, I intend to address some questions concerning the relationship between discourse analysis and ethnomethodology.

These questions are significant because of the recent appearance of explicitly ethnomethodological studies of science. It is important to know whether these studies can simply be combined with those done under the rubric of discourse analysis as a general 'ethno-discursive' programme, or whether there are significant disagreements between their methods and claims. In addition, a discussion of the differences and similarities of these two approaches will provide further explication of what is contained in the proposal for an analysis of scientists' discourse.

It would, of course, be an enormous task to try to distinguish discourse analysis from ethnomethodology in general as the latter now consists of a huge and heterogeneous body of work. Moreover, it could involve dealing with basic questions such as whether Garfinkel and

Cicourel originated different basic approaches[183] or whether conversational analysis constitutes a separate field with its own methods and theoretical perspective. However, the task can be made considerably more manageable by concentrating mainly on those studies which explicitly attempt to formulate an ethnomethodological approach to science[184].

In their overview article Temporal order in laboratory work[185] Michael Lynch, Eric Livingstone and Harold Garfinkel (henceforth LLG) deny that their article is meant to 'define a programmatic basis for ethnomethodological studies of work in the sciences'[186]. Nevertheless, they do make some general assertions as to the nature of an ethnomethodological approach to science which are of interest here. One focus for their studies of scientific work is the relationship between 'instructions' and 'practices'. They note that rules can never be sufficiently explicitly formulated to fully specify the actions they are intended to guide; that is, they note that "something more" is necessary for engaging in actual practice than even the most detailed of instructions. This identifies a topic for ethnomethodological analysis.

> It is this ubiquitous "something more" that delimits a field of investigatable phenomena which is not thematized in formal accounts of scientific methods. (187)

One of the features of this topic is its concern with a phenomenon which is not explicitly formulated in formal scientific writings. LLG's topic is thus the practice of science, the actual activities of scientists in the laboratory, rather than formal accounts of that practice:

> ethnomethodological studies attempt to discover and to demonstrate the ways in which various scientific practices compose themselves through vernacular conversations and the ordinariness of embodied disciplinary activities... [they] consult locally observable sequences of conduct that make up the details of a discipline's daily work. (188)

These practices are not captured in formal scientific discourse:

> accounts of scientific activities, i.e., "inscriptions," factual statements, documentary records, and published reports, become disengaged from the actual courses of scientific activity that produced them. (189)

Not only is this practice not fully and explicitly formulated in scientists' formal accounts but, according to Lynch, it contrasts with accounts produced by social researchers concerned with science:

> [The participating ethnomethodologist] can begin to specify the numerous ways in which the actual practice of science contrasts with the many versions of science which are extant in the science studies literature. (190)

When LLG and Lynch formulate the ethnomethodological programme for studying science in this way it becomes almost indistinguishable from the methodological (as opposed to theoretical) claims of constructivist researchers such as Knorr-Cetina and Latour. Indeed, LLG suggest that Latour and Knorr-Cetina have 'taken up topics affiliated to the literatures of ethnomethodology'[191]. Both of these approaches stress the importance of examining the actual practices of scientists because of the fallibility and distortions inherent in accounts of activity. The main difference between them comes in their theoretical aims: Latour and Knorr-Cetina's goal is the production of a constructivist theory of scientific practice; while LLG oppose exactly that. They describe constructivism as,

> a theoretically postulated descriptive philosophy: a philosophy that remains endlessly embedded in academic arguments about science with no attention being paid to the endogenously produced variants of argument and practice that constitute the technical development of ordinary scientific inquiry. (192)

This methodological convergence between ethnomethodology and constructivism[193] raises the issue of whether they are prey to the same problems. For instance, in their suggestion for studying the "something else" involved in rule use LLG seem to suggest that a definitive account of that "something else" is possible, thus allowing the ethnomethodologist to show exactly how a rule is followed. Despite their rejection of theorising and their rejection of other research on science, this analytic goal is a very traditional one in social studies of science[194]. It is very like the question of how rules, for defining a proper replication, say, <u>really</u> operate[195]. In chapter 5 I will develop a rather different approach to rules[196].

I will examine this question of the convergence/divergence between ethnomethodology and discourse analysis in more detail with Garfinkel, Lynch and Livingstone's (henceforth, GLL) paper: The work of a discovering science construed with materials from the optically discovered pulsar[197]. This is based on the analysis of a tape recording and laboratory notes made 'on the evening of the discovery of the optical pulsar at Steward Observatory'[198]. I do not intend to produce a detailed critique of this entire paper. Rather I shall concentrate on its status as study of 'discovery'[199].

In my discussion of Knorr-Cetina' work on the production of scientific knowledge (pp. 16-18 above) I noted that by treating knowledge as if it was 'manufactured' at a geographically specific location she produced a highly asocial conception of knowledge. Effectively she ignores any broader social processes which might be involved in the constitution of knowledge and in particular she gives no account of its acceptance (or not) by the relevant scientific community. The issue of scientific discovery, which is never explicitly raised in GLL's paper is of the same order. Brannigan has stressed the significance of a number of essential contextual features without which the term 'discovery' is unlikely to be applied by scientists.

> The attribution of the status, discovery, is founded on the processes of social recognition by which the announcement of an achievement is seen to be a substantively relevant possibility, determined in the course of motivated scientific investigations or schemes of research, whose conclusion or outcome is convincingly true or valid, and whose announcement is, for all appearances, unprecedented. These are the central elements in the apprehension of scientific discoveries, both for the individual scientist and his or her community. (200)

Furthermore, Brannigan has stressed that discoveries are made out as such; that is, discovery accounts have an irreducably performative status and cannot be taken as mere descriptions of 'natural' objects.

It seems, however, that GLL fall into exactly this trap. Throughout their paper they assume the unproblematic status of the discovery of the optical pulsar. When GLL list the constituents of the essence of the astronomers Cocke and Disney's 'discovering work' no reference is made to the processes through which the observational runs are constituted as a discovery by the scientific community.

As Collins has pointed out[201], this means that GLL's analysis does not even address the issue of the social constitution of the discovery. The analysis would, in other words, be identical even if it were later decided that the findings did not after all indicate the presence of an optical pulsar but simply a fault in the apparatus. Under these conditions GLL's claims to be looking at the essence, the 'quiddity', of discovering work becomes rather far fetched!

> In general, LLG suggest that they are explicating,
>
> the geneology of the object by tracing the "Galilean Objects" of science back to their origins in the embodied inquiries that make up ordinary technical activities in science. (202)

However, this notion of 'origins' is highly simplistic. Like Knorr-Cetina's image of knowledge being manufactured in amongst the chemicals on the lab bench, it seems to owe much to scientists' own folk models of knowledge production, whilst paying scant attention to the role of the scientific community in warranting knowledge and the interpretative contexts in which knowledge claims are formulated[203].

It seems, then, that ethnomethodological approaches to science will not <u>necessarily</u> mesh with discursive approaches. There will be areas of methodological and theoretical disagreement: in the case discussed, disagreement over how discoveries may be studied and over what sort of entity discoveries can be. This does not, however, imply that there will be a general incompatibility between ethnomethodological approaches and discourse analysis. Indeed, with respect to discovery, Brannigan's own work is explicitly derived from the literature of ethnomethodology; yet it is wholly compatible with a discourse analytic approach. Likewise, Woolgar's work on scientific texts, which will be drawn upon in later chapters, has ethnomethodological sources[204], but can be easily integrated into the present framework.

More generally, there is a considerable stress in both of these approaches on the indexicality of expressions[205] and the treatment of participants' ways of making sense of their worlds as a topic rather than a resource[206]. However, as I have indicated, these emphases have arisen in discourse analysis in response to fundamental analytic issues concerning the study of science rather than in the central theoretical and methodological problematics of ethnomethodology. Indexicality - the occasioned nature of scientists' discourse - becomes a crucial and unavoidable issue when the pervasive variability of scientists' accounts is recognised. Furthermore, a rigorous adherence to symmetry and impartiality prevents the analyst from adopting participants' own interpretative work. For to do so would be to begin to adopt their versions of the natural world[207].

Conclusions

I have now outlined six different programmes for the social study of science: quantitative analysis; constructivism; interest theory; relativism; ethnomethodology and discourse analysis. I have raised serious problems with each of the first four of these programmes which are centred on the way they deal with scientific discourse. None of these programmes develops an analytic practice which deals satisfactorily with scientists' writings and utterances. I have argued[208] that if these problems are to be resolved a shift in perspective must take place such that analysts approach scientists' discourse directly and explicitly rather than considering it as a mere pathway to recovering scientists' actions and beliefs. The discourse analysis which I am advocating has a number of similarities with ethnomethodology which has led to a similar set of criticisms of more traditional research. However, not all of its claims or practical analyses are compatible with discourse analysis.

Having established in general terms the need for an analysis of scientific discourse, the next task is to outline the specific research topic for the present thesis and the analytical problems it raises. In the chapter which follows I will discuss issues of a practical nature to do with the problems of data collection and transcription, and give an initial account of the contexts in which data were gathered.

CHAPTER TWO

ANALYTIC PRELIMINARIES

In this chapter I intend to describe the analytic materials which will be used in the five emprical chapters which follow; namely transcripts of scientific conferences attended by psychologists. Conferences have received remarkably little attention by those interested in social processes in science. Just over a paragraph of Ziman's Public Knowledge[1] makes up virtually the entire corpus of work which considers the general role of conferences, and this only consists of a brief unsupported speculation concerning their role in displaying scientific consensus[2].

There is a study - by Ian Lubek - which attempts to perform an analysis of conference proceedings and thus show how they were influenced by the 'power relationships' between the different participants. In particular, Lubek suggests that the 'power structure of the discussion' will reflect the power structure of the discipline as a whole[3]. Unfortunately, what the study does best is illustrate the pitfalls of traditional approaches and premature quantification. The author divides participants into 'visibles' and 'not-so-visibles' according to how recognisable their names would be 'to a large number of North American and European' scientists in this field (social psychology). By analysing the quantity of contributions to the discussion Lubek shows that visibles contributed disproportionately more than not-so-visibles. However, as a demonstration of power relations the study is totally inadequate. There is no evidence that the not-so-visibles were forced to act in that way; indeed, they are actually more represented in the discussion after editing (suggesting benevolence, perhaps, rather than despotism!). In fact Lubek's findings appear to be totally circular - the not-so-visibles are merely acting according to their classification. Furthermore, Lubek accepts that the content of the discussion (about the nature of the discipline) shows that some visibles were highly critical of the 'present order' while some 'not-so-visibles manifested clear aspirations to the legitimate order'[4]. Overall, then, the study suffers from exactly the flaws of quantitative methods discussed in the first section of chapter 1.

In some respects this general absence of serious research on conferences is surprising; conferences are virtually the only situation where scientists from different establishments and theoretical persuasions are gathered together for face to face communication and are enabled to confront each other's knowledge claims with immediate feedback. In many ways conferences seem like a prime site for revealing the operation of a variety of social processes in science.

Some of the possible reasons for this neglect will be discussed below. However, I will argue that despite the lack of attention paid to them conferences are occasions where many different kinds of scientific account are produced, and they therefore provide an ideal data base for addressing questions concerning the nature, organisation and function of scientific discourse. After this I will describe some of the more pragmatic considerations which led to the selection of the particular conferences for study and then discuss the technical issues which arise in transcribing scientific discussions and the preparation of such transcripts for analysis.

Throughout this chapter I will draw on Arthur Koestler's book: The Call Girls as a heuristic device for clarifying some of the issues involved in studying conference discourse[5]. This book is a fictional account of an international symposium concerned with the 'survival of the human species'. A number of 'colourful' scientific authorities in different fields (mainly related to social sciences: behaviourism, psychoanalysis, developmental psychology, cybernetics, neurology) are gathered for a week at an exclusive Swiss conference centre. Outside of their ivory tower there exists an international crisis and rumours of war. Koestler ironically contrasts the pettyness of the scientists' squabbles, and their moral impotence, with the potentially grave significance of their research for the broader international situation . As the dust cover notes: the book moved the Evening News to suggest that 'scientists and philosophers... are just like ordinary people'! Despite its status as a work of fiction (or partially so: 'the authors, publications and experiments quoted by [the characters] are authentic'[6]) it is probably the most complete account of a scientific conference which exists at present[7].

My interest in Koestler's book is not with its accuracy in capturing the essentials of scientists' activities at conferences. Its purpose here is to provide a convenient checklist of some of the various kinds of activities which are taken to occur at conferences and some of the ways they are made sense of: indeed, Koestler's book itself can be seen as another form of discursive data. Likewise, my general approach to conference discourse is not with the goal of reconstructing what went on at any conference: the nature of alliances, the persuasive power of presentations and so on. As I have emphasised in the previous chapter, the aims of discourse analysis are not of this kind. Instead conferences are of interest because they provide a rich source of data which throws light on the way participants' construct their discourse in accordance with specific social circumstances.

Conference Discourse as Data

A) Advantages

As I have indicated, there are a number of benefits which accrue from the use of conference data. It is worth describing them in some detail.

1) Conferences are a novel source of data. Two kinds of novelty are involved here. Firstly there is an absence of systematic research, or indeed research of any kind, on the general topic of scientific conferences. More importantly, there is also a dearth of research which examines direct, face to face interaction between scientists. Although the ethnographic workers Latour and Knorr-Cetina have stressed the importance of studying scientists in the laboratory, very little of their analysis is based upon discourse between scientists; the majority of their verbal data is collected in the more traditional form of individual interviews. More recently exceptions have appeared to this

in the form of studies by Lynch[8], Garfinkel, Lynch and Livingstone[9], and Williams and Law[10]. The first two of these use transcripts of interaction produced in the course of experiments or observational runs; the third is based on a transcript of scientists discussing the redrafting of a paper for publication.

A common feature of these latter studies is that their samples of scientists' discourse are produced in the course of joint activity, while participants are engaged in a shared project of some kind. They do not have the adversorial character which is a feature of much conference discussion. Koestler's fictional text strongly emphasises this feature. It is organised around a series of antagonisms between speakers, and he indicates that undermining others' positions may be an almost ritualised aspect of conference interaction:

> After a few seconds' silence, Dr Valenti lifted a well manicured hand, but Bruno got in first. He had missed his chance in the morning session to demolish Halder, but Harriet as a target would do just as well. He was not sure, he informed Mr Chairman, whether Dr Epsom had spoken in earnest, or, to put it in a different way, whether she had intended her proposal to be taken into serious consideration... (11)

In this passage Koestler implies that the victims are interchangable; it is the general procedure of demolition which is all important. Although only a few of the transcribed conference discussions I have examined could be classed as 'demolitions', searching, critical questioning is the predominant form. The conference transcripts are thus more variegated than those of the cooperating scientists which have been analysed up to now.

2) Transcribing conferences allows data to be collected naturalistically. That is, the data are collected with almost no interference from the researcher in a situation which would occur whether the researcher was present or not. Moreover, conferences and workshops are a common and routinised activity in all scientific disciplines.

The importance of naturalistic studies for analysis of scientific discourse is not the same as in more traditional social research. They are not considered significant because they avoid interviewer biassing, for instance. Because in the case of discourse analysis the aim is not to construct an accurate, unbiassed picture of actions and beliefs. Nevertheless, naturalistic studies do have certain practical and theoretical advantages over interview studies, particularly those interviews in which graduate students interrogate high status scientists. As Gilbert has noted, such scientists typically present science in the way that they habitually presented it to students[12]. The problem is not that this form of scientific accounting is biassed, or even that it is uninteresting - it can be very revealing about certain features of scientists' interpretative practices - rather it is that in these situations scientists are drawing on only a limited range of their accounting systems[13]. The conference data allows exploration of some of the systems which would not usually appear in interviews. (It must be remembered, however, that there are certain systems which are readily used in interviews but more rarely so in conferences.)

Although the traditional question of bias is not relevant here, there is still a question ofthe generality of findings. Despite the fact that it is not assumed that interview talk can be used to construct what actually happens in a particular social realm it <u>is</u> assumed that the interactional and interpretative work within the interview is similar in some respects to that which occurs in more usual scientific contexts. It is thus essential to analyse naturalistic data, if only as a check on the generality of interview based findings[14].

As a final note on the naturalistic status of the conference transcripts: in three cases out of five I made my own audio recordings and drew attention to the possibility that participants' contributions would be used for research purposes. This could, of course, have unspecified effects on the discourse. However, tape recording conferences is a regular practice, and it is not especially unusual for edited transcripts to be included in published versions of conference proceedings. In one form or another this was the intention in three of the conferences I used. It was also the intention of the organisers of Koestler's fictional conference. With its use being this common it seems unlikely that the tape recorder would have been an unusually disruptive influence.

3) At conferences, scientists' positions are evaluated by experts who have an authentic participants' understanding of the fields in question[15]. This can be duplicated in interviews to some extent by the social researcher having a thorough grounding in the relevant literature, by joint interviewing by a social scientist and an expert in the field[16], or by the interviewer participating in some way in the scientific area under study[17]. However, these are limitations on such approaches. It would clearly not be acceptable to generate too much conflict with the participants. Apart from any ethical consideration - which is no less important here than with 'ordinary people' - high conflict might lead to the termination of the interview. At conferences, on the other hand, heavy sarcasm and aggressive criticism is not uncommon and considerable heat can be generated on occasion.

More interestingly, conference discussions can allow for persistent and informed questioning by experts in a number of different fields directed towards what are seen to be weak points in scientists' belief systems. Likewise, replies must be formulated to satisfy a varied and skilled audience. It is this general level of participants' working competence that interviews wouldbe hard put to duplicate. Such expertise may not always be an advantage, of course. Points directed at an interviewer who is seen as a lay person can be very revealing. Nevertheless, the conference may generate patterns of accounting which have a subtlety and technical complexity difficult to achieve in interviews.

4) From the perspective of discourse analysis, one of the most important features of conferences is that their general structure allows for the production of a number of different versions of the same research. Typically there will be a written paper which will form the basis of a verbal presentation and will be published in a volume of proceedings (some form of publication took place in three of the conferences I examined). There may be a first draft of this paper which is precirculated and subsequently modified. There is also the version of the paper which is actually presented, the version 'in' the transcript. This may range from a reading of the written paper, which just

makes small adjustments for verbal fluency and timing, to the researcher speaking off the top of his or her head, without notes, and with no existing paper necessary. In addition there are versions of the research which appear in discussion periods: speakers may redescribe their own research in response to questions and questioners may formulate the nature of the research in the course of asking or commenting.

This multiplication of versions enables the analyst to start to relate the form of these various accounts of research and theory to the nature of the occasion of their use. In the analytic chapters which follow these kinds of comparisons will be made repeatedly.

B) Disadvantages

Despite the various advantages of using conference material as data there are also certain drawbacks. These can be classed as two kinds: pragmatic and theoretical.

1) The central pragmatic disadvantage is the lack of control the researcher has over the course of the discussion. One of the great rewards of interviews is that topics can be addressed in a systematic fashion and each participant can be asked to comment on each topic. This allows for instance, the analyst to compare every member's version of a particular issue. This is not always possible with conference data, because not all scientists will address each point and often topics of interest appear in an ad hoc manner throughout a transcript.

This difference should not be exaggerated, however. Even with interviews, topics cannot always be easily confined to their scheduled 'slot'. It may be found that issues not initially scheduled by the researcher become important, yet not all participants have provided comments on them. With conference and interview data the approach is the same; every occurrence of a topic of interest is coded so that analysis can be based on as complete a sample as possible. Typically a number of different scientists will contribute to a topic and some will comment on a number of occasions. This is often enough to document the existence of a particular interpretative practice, and its form; although it does not always allow the analyst to assess its generality. This lack of control does have a payoff, however. It is likely that those topics which occur often do so because they are seen as important by the participants - this may not be true when they are scheduled in semi-structured interviews. In addition scientists in interviews may pay unusual attention to proto-sociological speculations if they believe these to be the central interest of the researcher.

2) The theoretical difficulty with conference data is, I think, one of the reasons why they have received so little serious attention in the past. It is centred on the suggestion that conference discourse is 'only rhetorical', that while scientists act in the laboratory, they perform in the conference hall[18]. Koestler at one point has one of his characters emphasise this view of conferences.

> When you read [the participants'] stuff or get them alone in a relaxed mood, you realize their qualities - but the moment you put them together in a conference room, they behave like schoolboys performino a solemn play. They are worse than politicians, because politicians are ham-actors by natural disposition, whereas most academics seem to suffer from arrested emotional development. Politicians take their pride in making impassioned speeches and indulging in rhetorical flights; scientists pose as dispassionate servants of Truth, free from all emotional bias, while ambition and jealousy steadily gnaw away their entrails... Discussion? Interdisciplinary dialogue? There is no such thing, except on the printed programme. (19)

Koestler distinguishes between the versions scientists produce in different contexts, stressing in particular the faults and biasses produced in conferences where presentations are determined by psychological and sociological interests hidden behind a veneer of scientific objectivity. However, this sort of suggestion must be unpacked very carefully.

The charge of purveying rhetoric rather than facts is not confined to conferences alone. In the previous chapter Collins's work on gravity wave scientists showed some of them claiming a particular journal article was rhetorical. Yet this claim was far from consensual among gravity wave scientists, and certainly could not be used as an unproblematic analytic resource. Likewise with conferences. Occasionally a presentation will be described as rhetorical, but this is likely to be a signal for dispute rather than agreement; speakers do not regularly describe their <u>own</u> presentations as rhetorical. The logic of discourse analysis's impartial stance towards accounts prevents the acceptance of either of these claims. Indeed, they take on an analytic interest in their own right. In the same way that I suggested that Collins should look at the way rhetoric is attributed and not attempt the futile task of trying to discern which articles are <u>actually</u> rhetorical, the use made of such claims at conferences, or about conferences, can be examined.

It is interesting to speculate on the origins of this jaundiced view of conference discourse. One important difference between conference presentations and formal research articles is that verbal presentations are inevitably personalised. When speakers are presenting their own research, it is not possible to achieve the homogenised impersonal style nor to easily avoid attributing agency to the researcher by devices such as 'results suggest', 'studies indicate' and so on. Furthermore, individual differences of various kinds (sex, tone of voice, height, competence in delivery, dress, etc.) are apparent between scientists. These are all features of the 'contingent repertoire' (see chapter 3) which is a system of concepts for explaining scientific belief not as a result of the application of procedures of rational assessment to experimental evidence, but as a consequence of distorting social and psychological factors[20]. It thus seems likely that presentations of this kind will be seen as more rhetorical - or are liable to be described in more rhetorical terms - because of these features which conform to the contingent repertoire. It will be more difficult to make the presentation appear to be simply describing features of the external world rather than directed at persuading the audience of some claim. It may be that only written papers can conform fully to the impersonal empiricist ideal[21].

Another possible reason for the neglect of conferences by social researchers may be the implicit division between 'production' and 'context', with the production of scientific knowledge being treated as more important than the context in which it is evaluated and communicated. This sort of distinction can be seen particularly in the work of ethnographers and ethnomethodologists (documented in chapter 1) where the wider context plays very much a secondary role to the local site of 'manufacture'. However, it must be reemphasised that there are no adequate grounds for this imbalanced perspective; a consistent and impartial approach must examine both - indeed, such a study may throw the distinction between production and evaluation into doubt. In general, the suggestion that conferences are 'more rhetorical' must be resisted - it cannot form the basis for a viable analytic practice.

Psychology as an Area of Science

It is worth discussing one other topic to do with the suitability of the analytic materials; this is the question of the adequacy of psychology as a field in which to study scientific discourse. In many respects the arguments against this are like those concernino the rhetorical nature of conferences. The suggestion is made that psychology is not a 'true science' and thus to study it will not throw light on the actions of 'genuine' scientists. Again this is by no means a consensual suggestion. Indeed, many psychologists regularly emphasise the scientific nature of their discipline and point to the heavily empiricist orientation of the major journals[22]. Typically textbooks of psychology spend some part of their initial chapter emphasising the scientific nature of psychology.

As with the 'rhetorical status' of conferences, it is not the place of the discourse analyst to intervene in such debates and legislate one way or another as to the status of psychology. What is of great interest, however, is the way in which the such claims are proposed and defended. The manner in which the attribution of 'scientific' to a discipline or area is <u>achieved</u> in a particular context may be particularly revealing. Examples of this kind will be examined in chapters 5 and 6 below. The present approach, then, does not assume that the interpretative practices documented in psychology will be exactly the same as those of sedimentology or biochemistry. However, just because the latter disciplines are commonly thought of as 'hard', 'natural' sciences this does not mean that <u>their</u> interpretative practices are necessarily identical either. In general, discourse analysis takes a considerably more skeptical attitude to such global suggestions than traditional approaches.

The Pragmatics of Conference Selection

The conferences used in this study were selected more for their availability than by the systematic application of criteria. This would perhaps have been unnacceptable if I was intending to use them for the construction of an overall picture of a particular research network, the state of belief in a particular field, or the kinds of communication which take place between specialties. It would also have been unnacceptable if I was attempting to explicate the specific nature of conferences as a social situation. In the latter case the representativeness of the specific conferences examined would have been of particular importance; in the former their composition. However, in the present study the role played by conferences is that of generating interactional material on a number of topics of theoretical interest. Each satisfied this criterion and overall the transcripts have been found to be even more fertile than was expected. In addition to the studies discussed in this thesis a variety of further questions of analytic interest may be elucidated through further analysis in the future.

All the conferences chosen did satisfy one general requirement: they programmed a much larger proportion of discussion than is usual. Typically conferences do not allow for much more than a brief few questions after each speaker and discussion periods can often be squeezed out for administrative reasons. Koestler sketches some of the problems of this kind of conference.

> [The organisers] liked to cram forty to fifty papers into a five-day conference, which put the participants into a condition not unlike that of punch-drunk boxers, and left no time for discussions - although the discussions were the declared primary purpose of the whole enterprise. 'I am afraid', the harassed chairman would say, 'that the last three speakers have exceeded their allotted time, so we are running behind schedule. If we want to get some lunch before the next paper, we must postpone the discussion to the end of the afternoon session.' But when the last paper of the afternoon session had at last been delivered, it was time for cocktails. (23)

For the present purposes it was important to have as much discussion as possible so that the accounts of a variety of participants could be obtained. None of the conferences I have drawn upon had less than a third of their time devoted to discussion, and three had a format based almost entirely around discussion.

Description of the Conferences

Because of the requirement that the anonymity of participants be maintained the conferences will only be described in general terms. I have used the generic name 'conference' throughout this chapter as there are no precise criteria for distinguishing symposia, workshops, etc.; some of the following might, however, be better desribed by the latter terms. They are presented in chronological order.

1) The Goldberg Conference[24]

This was a one day conference, attended by about seventy people (a mixture of lecturers, researchers and graduate students mainly from departments of psychology, sociology, and urban planning) convened to discuss the theoretical perspective developed by a particular social scientist, Goldberg. Goldberg himself, on a brief visit to the UK, presented the first paper; his co-worker the next; and then there were presentations from British workers using the Goldberg theory. There were long discussion sessions inbetween papers. The presentations and discussions were videotaped by the organisers with the intention of producing a book of proceedings reproducing the main papers and some of the discussion. The organisers had the complete conference transcribed. Unfortunately the transcript did not identify individual participants, thus making it impossible to compare all of a particular contributor's statements or to develop an accurate understanding of the organisation of the discourse. Consequently this conference is not used directly in any of the specific studies which follow.

2) The Theoretical Perspectives Conference

At this conference psychologists gathered for four days to discuss fundamental theoretical issues; no participant used the opportunity to present original data. All the researchers present were invited to participate, the invitations being made according to the organisers' (and their advisers') understanding of which researchers had made important theoretical contributions. Many of the participants were established and well known figures within psychology. The graduate students who attended were nominated by their supervisors.

Roughly half of the time was allotted to the formal presentation of papers, which were delivered from a raised podium at the end of the lecture hall. There were no parallel sessions. All of the papers had been precirculated to discussants and other presenters. Non-presenters had been precirculated an extended abstract of each paper. The other half of the time was allotted to discussion. This was further split into specific sessions discussing each paper (lasting 20 minutes each) and general sessions on conference themes which lasted for up to three hours.

The papers and discussion were recorded (in triplicate!) by the organisers. I obtained permission to use this recording for research purposes and transcribed all the discussion periods, giving a total transcript of about 150,000 words. The recordings were of very high quality and there were few places where it was difficult to understand what was said (one European speaker with a strong accent caused slight problems). Furthermore, as participants were asked to identify themselves before speaking there is very little ambiguity over who said what. This transcript forms the basis for the analysis discussed in chapters 5 and 6.

3) <u>The Application Conference</u>

At this conference 13 European social psychologists met for a period of three days in a small seaside town. The conference was specifically convened to discuss issues surrounding the application of social psychology. In some cases this involved presenting particular 'applied' research projects, in others the general practice of social psychological application was discussed. As in the previous case, the conference was attended by a mixture of researchers along with some graduate students. The group included three professors of social psychology and a number cf respected researchers in the field; all had published articles or books on the topic of application.

Each participant had prepared a paper for the conference and these were circulated at the start so everyone had a written version of each talk available. For the most part papers were not read out but were summarised to provide a basis for discussion; each paper was discussed for about an hour. In between sessions the group ate meals together and went to local pubs together - clearly further discussion took place on these occasions which was out of reach of electronic ears.

Each session was tape recorded, with all participants agreeing that their talk could be used for research purposes. Two half sessions were lost through a fault in the recording process. The recording was transcribed verbatim (120,000 words). The quality of the recording was good and there was no difficulty in identifying the individual participants. In only one case did a particularly thick accent lead to difficulties. This transcript forms the basis for the analysis discussed in chapter 4.

4) <u>The First Construct Theory Conference</u>

Personal construct psychology is a slightly marginal area within academic psychology, although a significant number of UK researchers have done research using it. It is somewhat more established within the field of clinical psychology where a number of its leading figures are placed. At this conference, of the 12 participants, 7 held clinical posts and 5 academic research positions. The meeting took place in a magnificently situated Lakeland hotel over a period of 3 days. There was no fixed agenda. Instead a number of themes were proposed at the start and a discussion loosely organised around them. One figure, a key person within UK personal construct psychology, described it thus:

> The whole workshop remains in my mind as a fantastic experience. I have never before been involved with a number of people with a common interest in such an unstructured situation for such a long period of time. (25)

As before, each session was tape recorded, with all participants agreeing that their talk could be used for research. Only the first session was lost, as it seemed inappropriate to broach the topic of recording during a heated debate over the agenda and aims of the conference. A high quality recording made transcription straightforward. This transcript (130,000 words) is used in chapter 7.

5) <u>The Second Construct Theory Conference</u>

This was convened as a follow up to the first construct theory conference. It was held near an attractive South Coast fishing port. Seven of the original participants attended, along with four new people. Once again there was no formal agenda, and the first session was concerned with elucidating themes for discussion.

As is described in detail in chapter 7, all participants received two extracts from the transcript of the previous construct conference prior to this meeting, and one session of the meeting was devoted to a discussion of these extracts. Although all of the session were recorded, only the discussion of precirculated extracts was fully transcribed. This forms the basis for the analysis discussed in chapter 7.

Transcription of Proceedings

As I have indicated above, I transcribed all the conference recordings which were eventually used in the analysis. This has the advantage of ensuring that the analyst was not only present at the original conferences but was forced to listen very closely to the entire audio recording of each. This familiarity eased the problems of the initial coding of data into topics for study. In fact, the process of transcription is not too dissimilar to the analytic process of 'slow motion' reading which Barthes recommends[26]. The magnitude of the task should not, however, be underestimated. At the top rate of about 1,000 words per hour the transcription of the materials for this study took about four months of continuous work (in practice of course it was spread over a longer period with other work carried out at the same time). One of Koestler's characters notes 'transcribing tapes costs a lot of money'[27]; he might also have added 'blood, sweat and tears'.

The transcription was performed in a way intended to facilitate the maximum readability of the finished product.

It was structured into sentences and punctuated according to the conventions of written English. This is not to say that the audio tape presentation was 'cleaned up' in any way; word orders were not changed; hesitations and corrections were not deleted; and for the most part neither were filler words like 'um' and 'ah'. Furthermore, heavy stress was indicated in the transcript by underlining, while laughter and pauses were shown by these terms used in brackets. In this sense the transcript aspires to be as accurate a representation of the audio recording as possible. However, it differs from Gail Jefferson's elaborate system of notation[28] and even from the more restricted version described by Cuff[29]. For instance, the Jefferson system attempts to capture the characteristics of speech delivery - extension of sounds, some kinds of inflection - and gives timings of spaces between utterances accurate (it is claimed) to within one tenth of a second. In addition, spellings may sometimes be altered to convey colloquial features of pronounciation.

Although it is suggested that the use of thse elaborate conventions enables the transcript to 'look to the eye how it sounds to the ear'[30] it is not at all clear that the system facilitates the accurate use of verbal materials. Indeed, it places a particular barrier to those not versed in decoding such systems. It is also significant that conversational analysts - who have predominantly adopted this system - rarely analyse segments of data more than a few sentences in length. The system appears to be particularly unwieldy for dealing with the extended monologues which are characteristic of scientific discourse at conferences. Many turns of talk are well over 300 words in length, and where research is being described in detail turns of several thousand words are common.

Aside from these practical reservations, there is also the analytic question of how useful such detailed schemes are. With the possible exception of projects which are explicitly concerned with intonation - which I will discuss in a moment - it is very rare for the extra information embodied in these elaborate schemes to be explicitly drawn on in analysis. Of course, it might be argued that the analysts' interpretation of the transcript is dependent on the extra information provided, and it is merely that the nature of the dependence is not specified. However, in many cases it is very difficult to see how the analytic conclusions would be changed by the removal of pause times and semi-phonetic spellings. The necessity of marking these sorts of features has yet to be clearly demonstrated.

The case of intonation is rather different. Its role in language has been a source of considerable debate amongst linguists.. For example, O'Connor and Arnold suggest that intonation conveys information over and above that which can be conveyed by words and their grammatical structuring[31]. This additional information, it is claimed, is used to convey speakers' attitudes to their current situation. In contrast, Halliday, while not denying that intonation can serve the attitudinal function that O'Connor and Arnold suggest, argues that it does the work of certain grammatical forms, tense for instance[32]. Brazil has developed this suggestion further, and proposes that there actually exist two parallel 'channels' by which discourse can be formed: one via grammatical structuring and the other using intonation patterns[33]. A particular section of meaningful discourse can thus be created by either using the appropriate grammatical construction or by drawing on a particular pattern of stress and voicing.

All these views support the notion that intonation can be a significant carrier of information; although Brazil notes that it has yet to be demonstrated for an acceptable number of cases that intonation provides the sole realisation of a distinction[34]. In a more recent study Marga Kreckel has suggested that families or close-knit groups of friends may develop specific and partially idiosyncratic systems of rules which they can use to decode the patterns of stress and tonicity to reveal the specific speech act which an utterance performs[35]. For instance, a piece of speech which an outsider would classify as the act of 'giving information' could be described by a family member as 'ordering'.

The question raised by these kinds of studies is whether the present analysis is flawed because it fails to deal directly with the intonation information in the audio tape. Coulthart suggests that linguists 'ignore intonation at their peril'[36]. Nevertheless, the dangers for the analysis of scientific discourse seem rather more circumspect. In the first place the enterprise is rather different from that of the linguists discussed above. For instance, the way intonation is used to mark clauses in complex utterances, or to mark speech acts as threatening rather than informational, is not of great relevance to the kinds of questions to be addressed here. In particular, the present concern is with the way discourse is organised over different occasions of use rather than its role in providing information about psychological states. And although the present analysis is fine-grain, at places looking at substitutions of single words in sentences, it is not attempting to address fundamental linguistic issues of how words may be constructed into sentences and sentences into coherent discourse. Those kinds of more basic process are largely presupposed in the analyses to follow.

In the second place, all the data for the present study were collected from situations where groups of relative strangers met. This meant that for the most part participants made points explicitly and in relatively formal terms. The kinds of eliptical and largely tacit communication which Kreckel documents are rare. In certain cases intonational information is essential for the determination of meaning, for instance, when marking declaratives as questions. However, even here reference to the context can clarify meaning. Thus declaratives can be seen to be questions when they are treated as such by the following speaker. And in cases where the transcript alone was ambiguous it was generally possible to resolve the ambiguity by referring back to the audio tape.

It is also important to note that the discourse analysis conducted here is not based on speech act theory. There need be no attempt, therefore, to use subtleties of intonation to identify the particular speech act performed: stating, threatening, promising or whatever. Indeed, the present emphasis on particpants' accounting skills and the pervasive reformulation of the meaning of actions and beliefs in the course of interaction is somewhat at odds with a theory which attempts to give definitive characterisations of the 'illocutionary force' of statements[37].

With these analytic preliminaries in mind it is now possible to advance to the substantive studies which are the core of this thesis. The next five chapters are concerned with the detailed analysis of psychologists' discourse. The first two concentrate on issues to do with the application of science. Following them are two chapters broadly concerned with the role of values in theory choice and the organisation of scientific categorisations. After than comes a detailed analysis of the way a particular group of psychologists interpret and explain their own discourse. The final chapter will make more explicit some of the methodological and theoretical conclusions that these studies lead to. Before that, however, the issue of application.

CHAPTER THREE

NOTHING SO PRACTICAL

In this chapter and the next the analytic perspective outlined in chapter one will be applied to a topic which, despite its considerable importance, has been generally neglected in the sociology of science, namely the utilisation of scientific knowledge for practical ends[1]. The significance of the question of application for those sociological theories which claim that scientific knowledge is socially contingent will be discussed. Then the pervasive penetration of a traditional model of utilisation will be documented in both philosophical and sociological arenas of meta-science. Broadly speaking this model implies that modern technology is straightforwardly dependent on advances in scientific theory. The presence of this model will then be illustrated in two different areas of social psychological discourse: general articles concerned with the nature of applied social psychology and a transcribed interview with a particular applied social psychologist. It will be suggested that in the interview the traditional model of application is modified in a number of ways and considerably more socially contingent accounts of utility produced.

The discussion in this chapter will pave the way for a more detailed analysis of utility accounting in social psychologists' discourse described in the following chapter. There I will also review some theoretical alternatives to the standard model of application and attempt to develop an approach which is consistent with the present analytic perspective. Let me start, however, by examining the traditional model, which I will call the 'standard utility account'.

Theory, Application and Epistemological Privilege

The question of the practical application of theoretical knowledge has particular significance for the sociology of science. Its importance derives from a common, although often implicit, argument against the full-blooded sociological explanation of scientific knowledge. This argument assumes that the supposed 'products of science', such as bridges, atomic bombs, silicon chips, and so on, justify its having a special status vis a vis sociological analysis. Although much of the debate about the status of the sociology of scientific knowledge has been conducted in epistemological terms, this 'common sensical' supposition is recurrently drawn upon by philosophers, sociologists and scientists themselves. It is worth giving some examples to illustrate its pervasiveness in disparate areas of meta-science.

Philosophers of science have generally been unconcerned with science's application[2]. However, the notion that science has 'products' (for which science was a necessary condition) has been used as a resource in certain debates. For example, Shapere[3] has criticised the strongly relativistic reading of Kuhn[4] which suggests progress in any scientific discipline is simply a sequence of incommensurable conceptual world views. Shapere argues that science's 'products' and 'increased control over the world' amply demonstrate progress. We all know, Shapere implies, about science's increased effectiveness over time, for its consequences confront us everywhere we look. It thus becomes absurd to question the generally progressive development of scientific knowledge.

Similarly, sociologists of science have, for the most part, taken an intimacy between scientific knowledge and utility for granted. For example, the following quote is taken from a recent discussion of the sociology of knowledge-application.

> Science is man's most rigorous and successful mode of knowing the world of things. In this sense, scientific knowledge has a privileged status. The knowledge system relying on science has very different consequences for the degree of control over itself, than alternative types of knowledge systems... There can be little doubt that there is a critical difference between the science-based knowledge and competing beliefs with regard to adequacy for practical action in the external world. (5)

As a consequence of this assumption sociologists have often suggested that science has a special status. For instance Stark, in his classic work on the sociology of knowledge duplicates the claim of the previous quote when he suggests that:

> whereas man has more than once shifted his vantage point for the consideration of social facts so that these facts appear to him in ever new and often surprising, outlines, he has always kept to the same spot for surveying the facts of nature... so that these latter have always offered to him the self-same surface. He has merely learned to look more closely... (6)

And Johnston has argued in a similar fashion.

> When we say that science 'works', what we mean is that it provides us with the capacity to manipulate and control nature... the enormous attainments of modern natural sciences... represent a fairly conclusive proof of their superiority over other systems invented by man... (7)

In each case the special nature of science is warranted by pointing to its supposedly uniquely powerful understanding of and control of nature.

Claims such as these are not restricted to mainstream sociology. Certain traditional Marxist perspectives have claimed that scientific knowledge should be tested through practical application. When it is correct, scientific knowledge would enable new forms of practice. Lenin, for instance, argued that a leap forward in knowledge is necessary for the realisation of any radically new forms of action in the world[8]. Cornford is explicit on this relation between science and utilisation:

> up to modern times people had only superficial knowledge of chemical processes, and so there could be little effectively planned use of the processes in production. But modern chemistry enables us to break substances down... split atoms... even create new man-made elements. (8)

Other quasi-Marxist positions are more concerned with processes of structural determination than the epistemological foundations of knowledge. Even so, they draw upon the same set of basic assumptions. Albury and Schwartz suggest that a modern mythology has arisen which devides science into the 'pure' and the 'applied'[10]. This division, they claim, mystifies the political and economic regulation and consequences of science by implying that much of it is concerned merely with generating knowledge in the abstract with no practical consequences. Yet this, it is suggested, is an ideological myth which obscures the central role of science in providing material support for the state and its apparatusses. For instance, in

> 'defence' (war) research the myth of 'pure curiosity'... serves a purpose. In order to provide a veneer of respectability the military often give money for what they claim to be pure or basic research so that scientists can accept it gracefully without political or economic qualms... 'basic research' with its connotations of truth, impartiality and objectivity is an excellent place to hide military science. (11)

This final perspective has an interesting double implication for the sociology of science. It suggests that there ought to be a structural analysis of the role of science which treats it as an institution as embroiled in societal conflicts as any other. At the same time it strongly implies that a sociological anlaysis, however radical its perspective on social structure, will have little to say about the contents of scientific knowledge itself. In this view the very political potency of science is derived from its uniquely effective manner of commanding nature.

In general, if we accept the assumptions behind these kinds of claims, namely, that successful practical application provides uneqivocal validation of scientific theories and that a substantial proportion of scientific theories have been validated in this way, we seem to be led inexorably to the following conclusion: that many, and perhaps most, scientific theories transcend the limits of social context and that they must be accepted as epistemologically and sociologically privileged. If this strong connection between knowledge and utility is upheld, it can be plausibly argued that the scope of the sociology of science is drastically truncated. Or, to put it another way, it seems hard to reconcile the notion that scientific theories are the contingent products of socially contextualised interpretations with the idea that they produce vaccines, Saturn rockets and non-stick frying pans. To take a specific example, if scientific knowledge is socially contingent, and if aeroplanes are a direct result of the application of such knowledge, how is it that aeroplanes fly successfully across social boundaries? By such references to the universal effectiveness of modern technology it can well be claimed that 'practical application' provides a socially invariant criterion which validates the bulk of scientific knowledge and shows the nature of its content - if not the choice of topics to research - to be independent of social factors.

In the next chapter I will discuss criticisms of this argument which question whether 'products' validate science in the way implied, and even whether much technological innovation is dependent on science at all. For the moment, however, having shown the significance of this argument for the sociology of science, I intend to examine the role of this model of application in scientific discourse itself, and in particular in the writings and talk of social psychologists.

Social Psychologists' General Accounts of Application

In this section I will look briefly at social psychologists published characterisations of the applied nature of their discipline. This in no way claims to be a survey of the generality of the model discussed - although an initial examination does suggest it is quite widely distributed. It is merely meant to illustrate the presence of a particular model and to indicate a few of its formal features. The articles examined can be thought of as participants' own meta-science. Although social psychology does not abound with such articles discussing application in general terms they do exist and, irrespective of whether they affirm or deny the applied potential of social psychology, they tend to draw upon a standardised version of the science-utility relationship.

Take, for example, an article by Helmreich[12] which generated a large secondary literature[13] and argues that social psychology is not fulfilling its applied potential. Helmreich diagnoses a number of problems which beset applied social psychology: simplistic statistics, student subjects, aimless experiments; but he sees as crucial a 'pernicious schism' between theoretical and applied social psycholgy. There are two camps of researchers:

> those whose applied interests focus on theory validation in natural settings, demonstrating replicability of theoretically important findings, and making data

> available for application and those social psychologists who feel that the dominant concern of the profession should be social engineering and improvement of society. (14)

Both camps, argues Helmreich, contribute to application. The former has potential for application and can be translated into practice. The latter are engaged in that practice in various ways. Awareness of this line between inquiry and implementation should, Helmreich claims, ameliorate some of the criticisms that are commonly made about applied social psychology:

> if the line... were drawn somewhat as I have suggested, it seems that social psychologists could work on both theoretical and applied problems in the laboratory and the field without perceiving a difference in orientation. (15)

Helmreich can thus be seen as perceiving a dislocation of the 'natural' continuum between theory and application and suggesting that repair of this continuum would cure the (putative) lack of social psychological application. Yet his conclusion appears to be more a product of the standard model of application than any data he has produced to demonstrate the potential applicability of theory in social psychology. The implication is that effective practice must be dependent upon theory, so if there is a paucity of successfully applied social psychology this must be because practice and theory have somehow become dislocated.

When we examine another example, which gives a considerably more favourable assessment of the applied potential of social psychology, the same general form of application account can be detected. Eiser argues that the practical returns from social psychology accrue from the application of theory to 'real-life' situations:

> there exists a strong body of [social psychological] theory which, while it may not necessarily deal with universal principles, is still sufficiently general and integral to encourage the hope that it will be applicable to real-life situations. (16)

He treats the process of application, much as Helmreich does, as being one of putting the symbolic generalisations of social psychology to work in practical contexts. Social psychology is taken to require no special features in order to make it applicable:

> "basic" academic research questions in social psychology can be studied in applied settings. All that is needed is a greater responsiveness to contemporary

social situations and a greater willingness to present the reasons for one's research in a way that is intelligible to people outside one's own discipline. (17)

If we finally examine an introductory textbook on social psychology (Goldstein[18]), which includes a chapter on application, we can see that it mirrors an increasing quantity of social psychological publications which refer back to the writings of Kurt Lewin, who was an influential social psychologist writing in America in the 1940's and 1950's[19]. The striking thing about these references is that they generally do not talk about Lewin's own applied work (which has been dismissed as trivial by at least one commentator[20]) but use a single quotation: "there is nothing so practical as a good theory"[21]. Goldstein's introductory text actually uses this quote in the title of one of its subsections[22].

The significance of this quotation is that it encapsulates, very economically, the standard utility account which was used in more extended form in the previous two articles. It is highly ambiguous, and merely suggests that the most practical theory will be the 'good theory', while giving no indication of what should count as a good theory. It deals, as it were, with the hows and whys of application in nine short words. Some possible functions of these sorts of account will be discussed in the next chapter; for the moment it can be taken as a further indication of the pervasiveness of the standard model of utility.

Examples such as these suggest that there is a congruence between the model of utilisation which is common in sociology and philosophy of science and the model which scientists themselves use. In many ways this is hardly a startling idea; after all, the very language used for talking about science encourages and underpins this model[23]. What scientists do is said to 'have application to the real world'; bridges and non-stick frying pans are taken to be the 'products' of science. On television 'Tomorrows World' and 'Horizon' tell us how science is 'changing our lives' and advertisments show us fridges and washing machines 'beamed down' from the space ships of the planet Zanussi, as 'the appliance of science'. Nevertheless, when we take even a single researcher's account of the practice of applied social psychology and examine it in detail the picture becomes considerably more complicated. In fact it becomes very difficult to see the 'standard model' of application as a straightforward representation of the science-utility relationship.

Social Psychology and Social Skills Training

The remainder of this chapter will consist of a detailed analysis of an account given by an applied social psychologist. This psychologist specialises in social skills training and the account centres on his practice and its relationship to social psychological theory. What connection is there between the theoretical formulations of social psychology and the activities and techniques involved in social skills training (henceforth SST)? I will examine in turn the synchronic and diachronic interrelations depicted between theory and utility, the transformation of theory in the process of application and the way broader social influences are said to direct the form and content of SST. Before this, however, some general comments concerning SST and about the particular practitioner interviewed.

Social skills training has been arguably the area of most growth in applied social psychology in recent years. The number of practitioners at present doing this work in Britain is in the hundreds. A range of social skills are taught in a variety of different settings. It is probably most commonly associated with clinical work, where people who are socially inadequate in various ways are taught basic skills such as shopping, dating, opening conversations and posture. At the other extreme business men may be trained in handling clients and subordinates. SST is also used in the penal services, by councilors of all kinds, and in womens' groups as assertion training.

SST involves a variety of techniques. One of the most common is 'role play' where the client is taught a skill or a set of skills by getting them to act out a particular role. For example, a client may act out the role of buying groceries from the supermarket. This may involve the trainer emphasising that the client adopts normal posture whilst shopping, makes appropriate conversation whilst paying for the groceries accompanied by eye-contact which is neither furtive nor staring.

Typically it is theories of non-verbal communication and non-verbal behaviour (by no means regarded as the same thing) which are associated with social skills; there is not the same amount of theoretical development in the area of verbal skills. To give a simplified example to illustrate the general principle, Hall[24] has suggested that there are various normal distances for standing apart from others. These depend on the interactants' degree of intimacy, the number of people present and the public or private nature of the situation. It appears to be only a small step from talking about correct distances for interaction to attempting to train people with problems how to stand at the correct distances[25].

There are a number of reasons why social skills training should serve as a particularly apposite case study of the application of social psychology. First, as noted above, there appears to be a relatively developed body of theoretical work associated with the area. It thus seems, a priori, to be an area where the traditional model of scientific application should hold true. Secondly it is a clearly social psychological area of application. It can be argued that social psychologists' attempts to deal with prejudice for example, or unemployment, may be ineffective because they try to deal with essentially political or economic problems as if they were social psychological. No utilisation is fully immune from such criticisms, but they seem less relevant to SST. Thirdly, as a relatively new field it might be easier to trace the role of theory. It might, for instance, be the case that in relatively established fields theoretical notions have become 'embodied' in standard procedures in such a way that participants can no longer recover them in formalised terms. Also, compared to say advertising or organisational interventions, the impact of the practice might be more clearly assessable. This was not important because I wished to evaluate the success of the practice - I did not; rather it was to facilitate the collection of the respondent's accounts of criteria for success.

The particular respondent was chosen because of his respected position within the community of social skills trainers. He was legitimated by having both a university and a hospital based position - which involved him in both teaching and clinical practice - and by contributing to both conferences and the literature of SST. Of course, it is not possible to claim that his practice was not deviant in any way, for deviancy is clearly a highly indexical term. However, no reason was found to doubt that his practice was considered anything but top rate amongst other trainers. There was no hint from the interview that the respondent's practice was abnormal or unusual.

The interview schedule was constructed using a fairly simple theoretical model of the relationship between theory and application with the aid of the social studies of science literature (see Appendix A). The respondent was interviewed for about 2 hours, covering the questions on the schedule. Subsequently the recording of the interview was fully transcribed verbatim. The following discussion is based on this transcript.

Synchronic Interchange Beteen Theory and Application

The section from the interview which is reproduced below shows the point at which the interviewer first broached the qustion of theory in SST.

1. Interviewer. (1)What would you say were the important theories that you use in social skills training?

   (2)(Pause)

   Respondent. (3)Important *theories*?

   Interviewer. (4)Yes.

   Respondent. (5)I suppose it's a learning theory model really, or what I consider learning theory to be. (6)I see social skills training very much, really, as based on an educational model. (7)It is *skills* training. (8)And one can relate to much more to an education type viewpoint, which is based on learning theory.

   Interviewer. (9)And how about the particular theories of Argyle and Dean on gaze and Eckman and Birdwhistle on kinesics and so on?

   Respondent. (10)I don't take much notice of those I must admit. (11)I mean, obviously their stuff is very useful in providing information, and in terms of instructing clients one usually gives a talk on, say, eye gaze, and that may draw on the work of Argyle to just indicate the way in which eye gaze is used.

It seems clear that the interchange described by the respondent does not conform to the standard model of utilisation. Yet how are we to disentangle exactly what is going on in this extract? Up to now I have only talked in very general terms about the approach taken by discourse analysis to specific materials. It is now necessary to describe some more specific features: in particular the idea that scientists use more than one conceptual system when accounting for their activity. In the analysis which follows, and throughout the rest of this thesis, I will repeatedly draw on the notions of 'accounting system' and 'context of discourse'.

An example from Nigel Gilbert and Michael Mulkay's work on biochemists will illustrate the use of these concepts[26]. These researchers compared the formal accounts given in the introductions and methods sections of biochemists' articles with informal accounts given in letters they wrote to each other and in interviews. For the most part they found striking dissimilarities between accounts in the formal and informal contexts. In the formal context, scientists consistently adopt an 'empiricist' discourse about science; that is, scientific knowledge is presented as being determined by the controlled, experimental revelation of 'the facts' about the natural world. The production of experimental facts is taken to follow from scientists' implementation of impersonal procedural rules; and theoretical interpretation is portrayed as deriving unproblematically from the facts, as long as no personal or social factors are allowed to influence scientists' judgments.

In informal social situations, although this 'empiricist' conception of scientific action and belief is still used, and indeed, although it still continues to be primary in an important sense, an alternative repertoire is available for depicting science in general, one's own actions and beliefs, and those of other scientists. This second interpretative repertoire is termed 'contingent', because it treats action in science as much less uniform and scientific belief as much more open-ended. Emphasis is placed on the importance of personal commitment, intuition and practical skill. The production of data is taken to be a highly individual accomplishment. Theoretical interpretation is regarded as problematic and only partially constrained by experimental findings. It is accepted that social factors influence the actions of all scientists. In its strongest formulations, this repertoire enables the speaker to treat scientific knowledge merely as those beliefs which specific collections of specialists happen to have adopted for whatever reason.

It appears, then, that scientists have at their disposal two rather different vocabularies or accounting systems for describing scientific action and belief; and that each vocabulary implies a different conception of scientific knowledge and rationality. Thus an accounting system may be considered as a discourse which has its own logic, or its own set of rules for interpreting and portraying social action. As I have noted, different accounting systems seem to be used in different social situations. Although formal research papers contain almost exclusively 'empiricist' accounts, which eliminate direct reference to social actors or to the social contingency of scientific knowledge, in interviews and other informal situations both accounting systems are used, depending on detailed changes in the specific interactional context[27].

Coming back to the social skills transcript, it is possible to detect the presence of two accounting systems at work here also. One of these is very similar to the traditional model of application discussed in the first sections of this chapter, the 'standard utility account'. It takes the theoretical knowledge of science to be routinely and naturally applicable and treats practice as dependent on theory. In many ways it can be seen to correspond to Gilbert and Mulkay's 'empiricist account'. The other treats the relation between science and utility as far more problematic. Like the 'contingent repertoire', expertise and considerations of a non-formalisable nature are indicated, and on occasion it is suggested that practice is entirely independent of theory. Let us examine some instances in detail.

At the start of the interaction quoted above (extract

1) the respondent is non-plussed by the interviewer's question which, it should be noted, already assumes the idea that theories are employed in social skills training[28]. The respondent then, somewhat tentatively, proposes an example: 'I suppose it's a learning theory model really' (1.5, emphasis added). However, this proposal is then qualified as only the respondent's interpretations of the theory: 'what I consider leaning theory to be' (1.5, emphasis added). Social skills training is then distanced from learning theory by proposing an intermediate model: 'I see social skills training as based on an educational model' (1.6). The initial proposition has now been twice qualified. Then the repondent emphasises that it is 'skills training' (1.7), which seems intended to suggest that it, therefore, must involve learning theory. This final point can hardly be viewed as a reply from experience, it seems almost philsophical in nature. The respondent appears to be directino the interviewer to attend to the meaning of the term skills training and thus the implication that education must be involved.

In the next interchange the respondent is questioned about more directly social psychological theories. He produces an apparently contradictory reply: 'I don't take much notice of those' and 'obviously their stuff is very useful' (1.10-11). However, it is possible to see these comments not as straightforward conradictions but as products of different accounting systems. One system, the standard utility account, embodies the notion that relevant theories are 'obviously... very useful'; whilst the respondent's more contingent account suggests that this is not the case, that theories were not found useful.

The initial interchange (1.1-8) can also be looked at in this way. On the one hand the standard utility account (which was already implied in the interviewer's question) suggests that an answer should be provided. On the other, the respondent is not happy with this, in the context of a contingent account, and qualified the reply in the ways discussed.

The patterns analysed above are not unique to the particular extract cited. For example, a similar analysis may be performed on the following passage:

2. Interviewer. Have changes in theory, or improvements in those theories inputted into your practice in any way at all?

(Pause)

Respondent. I think they have. They inevitably have. But I don't think I have consciously applied them. Certainly in terms of one's reading and correcting one's knowledge. That obviously is used in terms of what you do, when you are working, in fact. But I can't say that I have consciously taken them and applied them.

Here we see that the input of theory into practice is 'certain', 'inevitable' and 'obvious'. And yet, within the space of a few seconds, the respondent twice notes that there was no conscious application. Again, the apparent contradiction can be resolved by seeing these utterances as products of two different accounting systems. It is important to note that the suggestion that the respondent uses two systems to answer questions is not intended to imply that he is reasoning poorly or contradicting himself. He is simply drawing upon two commonplace resources for answering the questions: the contingent and empiricist versions of the application process.

Later in the interview the respondent addressed more directly the problem of talking about the use of theories;

3. Respondent. I think why I find it difficult to think about the theories is that I am not familiar with the field from a theoretical point of view.

The respondent here straightforwardly describes his knowledge of theories in a way very difficult to reconcile with the traditional model of application. In particular it seems difficult to believe that the respondent could be both applying certain theories and unfamilar with those same theories. It could be that the respondent is implying that theories have become embodied in the the procedures of SST in some way. However, accounts of diachronic interchange between theory and application which stress the centrality of practice cast doubt on this.

Overall the analysis of this section illustrates that the apparently contradictory statements by the respondent can be made intelligible by viewing them as a product of two alternative accounting systems. The traditional view of application is consistent with those statements described as the standard utility account; the more contingent version, however, does not fit well with the traditional view. On the one hand the respondent stressed that theories are important; on the other he was hard put to give specific instances of theories which he had utilised. This point will be elaborated in an examination of more diachronic features of interchange.

Diachronic Interchange between Theory and Application

The traditional view of the science-utility relationship implies that continual interchange between the different contexts is a characteristic and pervasive feature of applied science. Not only that, but the direction of interchange is said to be predominantly from science to technology; problems are solved in the context of science and then the answer is used in the context of application. Or, as Barnes and Edge formulate it,

> the production of new knowledge is the concern of science: scientists creatively construct new hypotheses and theories, and rigorously evaluate them against observations and experimental results draws from nature. Technology is then the routine activity of working out and realising the 'implications' of scientific theories. It is a humdrum, uncreative activity crucially dependent on basic science. (29)

There is very little actual research on the longitudinal features of interchange. The social skills practitioner, however, provided a detailed account of these features of his work.

The respondent described the development, with an associate, of a package for SST:

4. Interviewer. You say you use a package. Is this something that you developed yourself in practice?

   Respondent. This is something we have developed ourselves... the package we have worked out over the years, and we use the same basic format for each session and we just use different exercises; change the exercises but the format of most of the sessions is exactly the same.

   Interviewer. Now, you say you have modified your procedure in various ways. Has this modification come mainly from your own expereince of trying to do it or through the changing literature of social skills?

   Respondent. Through our own experience, that's all. Because the basic material and the basic techniques that we use are the same really, it's just that with a group you have got the added dimension of having a group and, you know, all that that involves. And one has to introduce things other than strictly speaking social skills methods to make it function as a group [inaudible]. So we have really modified things from our own experience.

The interviewer's questions in this interaction quite broad, referring not just to utilisation of social psychological not just to utilisation of social psychological theory but also to the general literature on social skills. Even so, it is clearly experience that is seen by the respondent to be the crucial or possibily the sole factor in the development of his practice. If we look more closely at this passage, and note the nature of the exercises referred to - clients reporting a good thing that had happened to them recently, or role playing an every-day encounter for the group to give feedback - it is clear that a number of craft skills are being alluded to. As with all skills of this kind, it seems unlikely that the ability to teach these exercises could be fully formalised and communicated solely in the published literature[30]. In fact, the respondent suggested that those who attempted to set up social skills groups with only knowledge of the literature, without any training and experience of the 'behavioural approach', often ran very poor training sessions:

5. Respondent. Some of them are already doing it [running social skills groups without experience and training]. They have read Argyle and Trower's book and they have started.

Interviewer. And you think it would be very difficult for someone who has just taken a part of the literature - or something like Argyle and Trower's book - rather than talked to people who have been in clinical practice, to be very successful at it?

Respondent. What happens you see is that - I will give you a typical example. We are in a day hospital setting and we have got all these patients; why not social skills training? And so they take anybody indescriminately into the group. They don't assess. They just take them into the group. And they look at a couple of Argyle exercises. They may look at so and so's feeling about so and so. And that's the way they operate... they may well do some good, somewhere; but you don't really know what they hell they are doing. And they may do some harm.

It should be noted here that the suggestion that the knowledge needed to carry out SST is not fully available from literary sources is not meant to imply that literary sources are entirely useless. Rather, the necessity for a set of interpretative skills and resources is implied. For instance, it may be that practitioners who already possess certain skills and knowledge may read and assimilate the literature in a different way from naive readers. The important point is that this knowledge is treated as not completely formalised, and therefore the literature of both the theory of social skills and the practice of SST is seen as insufficient on its own for carrying out SST.

When questioned about the possibility of interchange from practice back to science, that is, feedback of knowledge aquired from SST into the theoretical literature on skills, the respondent said that British trainers 'are not interested in theory as such'. However, he did suggest that a contribution to the theoretical literature was a possibility.

6. Interviewer. Do you think that the results of clinical practice like yours could have an important role in evaluating theories, I mean, if you were interested in it?

   Respondent. Yes, I am almost sure it could. Yes of course it could. If one were prepared to do a lot of painstaking work. I mean, just thinking about eye gaze, if one really looked at that in the context of social skills work I am sure you could get an enormous amount of data to feedback. And it would challenge most of the ideas. Because most of the experimental work has been done out of context and it would be very nice to replicate that. Not that social skills is a very real context, but it is a different context that one might use.

The respondent here describes an inadequacy in what he sees as a central area of the theoretical literature of social skills. This again illustrates the deviation from the standard model of utility. Furthermore, the explanation given by the respondent for the inadequacy of the theoretical work on gaze indicates a further contingency in the application of social psychological theory. The respondent argues that most of the experimental work on gaze has been done in contexts which may not correspond to the natural contexts in which interaction occurs. He suggests that even though social skills training itself is not totally naturalistic, it could still be used to challenge the experimental work. A further implication of this argument seems to be that the experimental work on gaze would not support an effective practice unless, perhaps, some way could be found to translate it into more life-like contexts. The respondent himself, then, has a notion of the context dependence of reseach; unfortunately the transcript does not contain enough information to assess exactly how elaborate this notion is.

In one place in the interview the respondent discussed a theoretical issue which he and his co-trainer had 'directly addressed':

7. Respondent. In the early days we were very much interested in the difference between social anxiety and social behaviour. And we assumed that social skills was much more suitable for people who have social skills defecits , behavioural deficits, and it was'nt particularly suitable, say, for social phobics who's fear of social situations was based on anxiety rather than lack of skills. And so our assessment in the early days was really geared to looking at that. And we used to give them measures of social anxiety as well as social skills to see if there was any difference between the two. And what we tended to find was that social anxiety reduced as social behaviour improved [both laugh]. Even in our early groups. And as we proceeded it became increasingly clear that the two hold together. And now we don't worry about distinguishing between the two.

Here the respondent had assumed that the theoretical distinction between social behaviour and social anxiety would be important for practice. This was not found to be the case. The process of modification is depicted in a way contrary to the standard utility account here. Instead of practice being dependent on theory, theory is described as actually undermined by an understanding of the phenomena developed through practical experience.

On the whole, little of the respondent's detailed description of diachronic interchange is in accordance with the standard model. Although, as we saw earlier, this model is drawn upon in broad glosses of the work, it does not appear in the more detailed descriptions. Modifications in training methods are described as consequent on previous practical experience rather than on any theoretical developments in the area. The formal literature of SST, even when it is specifically orientated towards the pragmatic details of training, is described as insufficient by itself to sustain an effective and competent practice. Indeed, it is suggested that careless use of formal manuals might even lead to harm to patients. Furthermore, it is suggested that experimental research which abstracts social skills such as eye gaze from the everyday contexts of use might not hold good for those more natural situations. In fact, the respondent suggests that the understanding developed through skills training might well undermine theoretical beliefs about such skills. This idea is supported by a second example in which a particular distinction, which was expected to hold on theoretical grounds, did not do so in practice. Nevertheless, the respondent indicated that no formal attempt to evaluate theory in this way was taking place. Theoretical and practical concerns, then, were depicted as generally developing along separate lines with little formal exchange between them. The idea that practice <u>must</u> be dependent on theory ceases to be drawn on.

<u>Transformation in the Process of Application</u>

Philosophers of science in the tradition of Kuhn[31] and Hanson[32] have been less willing than their predecessors to talk of facts and theories as stable, unnegotiable, objects. For example Ravetz[33] suggests that facts and theories undergo radical reinterpretation when they pass form the contexts of pure science to those of practical application:

> it can be seen that a version of a standardised fact which is good enough for one function can be quite inadequate for another... (34)

Various studies have examined this argument about the contextualisation of facts and theories emprically[35]. Mulkay concludes that:

> the formulations of basic science... undergo major transformations of meaning as they come nearer to the realm of practical application. (36)

Some theories, of course, explicitly predict contextual changes: acceleration is not expected to be constant in different gravitational fields; cognitive dissonance is only expected to result where experimental subjects have a choice of different responses[37]. The transformations referred to by Mulkay, however, are not of this kind; they are changes in the theories themselves. Some more elabor-

accounts of the theory-utuility relationship will be discussed in the next chapter. For the moment I will concentrate on the respondent's/which involve transformation.
accounts

The notion of transformation cannot be straightforwardly illustrated in the social skills transcript because there is no instance where the respondent is unambiguously claiming to use a particular theory. Nevertheless, there is some information relating to this phenomenon. For instance, the respondent referred to the Argyle approach, sometimes as a body of literature, sometimes a set of practices. While the respondent clearly felt he was using, at least to some extent, an Argyle type approach he considered the emphasis to be different in significant ways.

8. Respondent. I don't accept the idea that there are basic social skills and that we all have them, that we can then teach if they seem to be deficient in some way. I don't believe in teaching people particular ways of behaving. I believe in providinq people with enough practice so that they can use their skills in whatever way they feel is useful to them in order to achieve their needs; which I think is a different emphasis really from the Argyle approach.

The distinction the respondent is making between his approach and Argyle's seems to be between, on the one hand giving clients practice in using a variety of skills which can then be used in any way they please, and on the other teaching them a universal set of skills appropriate for each particular situation. Unfortunately it is not entirely clear from the interview whether the respondent is claiming to have taken something like an Argyle approach and then transformed it in his own practice, or whether he is simply describing certain differences between basically similar approaches. Whichever of these is the actual claim, the possibility of transformation in this way is illustrated.

One candidate for the title of a classically applied theory is learning theory, or the 'behavioural approach' (as illustrated in extract 1.8). The respondent suggested that knowledge of the behavioural approach might be a prerequisite for doing efficient social skills training:

9. Respondent. The problem that we have in training non-psychologists is that they don't know what the behavioural approach is, they have never actually assessed problems in the sort of detail that is necessary. They are not used to even looking at behaviour as such.

Despite these strong claims for the importance of the behavioural approach, when elaborating this point later in the interview the respondent suggested some problems with it.

10. Respondent. I think I see a lot of problems myself with things like conversational skills. OK, you might teach someone a set of skills of how one might go about having a conversation. The assumed goal is that there is some end point, but, in fact, half the pleasure of having a conversation is just having the conversation with someone... I suppose what I am saying, in a very confused way, is that the behavioural model isn't a sifficient explanation of social behaviour. Social skills training is really based on learning theory. I think it works, but at a theoretical level it is not a sufficient explanation for what is happening.

It seems, then, that the behavioural approach is not simply a theory put into practice; certain craft skills are hinted at - detailed assessment, looking at patients problems in a certain way - whilst as a theory the approach is seen as insufficient. Thus although in some respects this description seems to correspond to the standard utility account, particulary with the claim that the practice of skills training is based on learning theory, this grounding is described in pragmatic rather than epistemological terms. Learning theory is adequate for practice, but not as a theory. As before, even though the standard model of application is drawn upon in the general gloss, when more detail is added it is is undermined.

Another sort of transformation which the respondent described concerns the differential interpretation of methods by practitioners.

11. Respondent. Because it is such a bandwaggon using social skills, quite a lot of people are misusing it... To give you an example; they might get somebody to role play being assertive and they will get them to do the role play and then they will start asking them how they felt about it and go into the feeling, the dynamics... That is using a different sort of model to get at it.

Interviewer. Would someone from an analytic perspective, a psychoanalytic perspective, interpret the theories on gaze and body positions, and so on, in terms of a different kind of framework?

Respondent. Yes. You see there is a lot of overlap with other sorts of activity type therapies. I mean drama, psychodrama, there is an awful lot of it going on... you might get people doing very similar sorts of exercises, but they will use them in an interpretative way. They will not be trying to teach the client behaviours.

Despite the formulation of the question in terms of theories of gaze and body positions the reply is concerned with practical social skills exercises. It is these that are seen to have different meanings dependent on the broad perspective within which they are used. An activity which for one therapist may be training the client skills becomes for another the use of a diagnostic tool for revealing underlying problems. This again raises the possibility of transformation.

The lack of any clear cut description of the utilisation of theory by the respondent makes it difficult to talk in specific terms about the transformation of theories during application. However, as I have illustrated, there are a number of sections of the interview which are relevant to this issue. The respondent mentioned the theory of gaze as one which would be found wanting in the practical context of social skills groups. In contrast he mentioned learning theory as a theory that worked in practice but was, as a theory, insufficient for explaining what was happening. In addition, the respondent describes certain procedures being modified in an ad hoc way to fit in with different therapeutic concerns. These instances, then suggest further possible departures from the standard utility account.

The Societal Context of Theory and Application

Another modification of the standard model comes when the role of broader social factors and interests is introduced by the respondent. In chapter one I developed a number of criticisms of the way 'social interests' have been drawn on as an explanatory resource by sociologists of science[38]. While criticising their deployment as unproblematic theoretical entities by sociologists, the argument was not meant to suggest that social interests, or at least their invocation by members, is not an interesting topic for analysis. Clearly it may be very revealing. In the present discussion the respondent draws on certain social interests when explaining the popularity of SST in a way which elaborates on the more contingent version of theory application.

The role of the broader societal context in SST is particularly discussed in the various sections of the interview concerned with the growth of SST in the UK. The respondent's explanation indicates that the burgeoning popularity of the field does not arise from purely scientific considerations. SST is seen to fit with a growing trend in the social services towards community based treatment:

12. Respondent. I would think that people are, in most of the services, becoming much more community orientated. In other words getting away from some sort of institutional model of dealing with problems, punishing for crime and so on. And they are moving much more into the ideal of working with the community in trying to rehabilitate them. And that assumes that you have got to teach these people how they are going to fit into society, into their community. And I suppose the idea of then improving on social skills is one way of making it fit the model you use.

The respondent points out that there is a move away from punishment towards rehabilitation. The propriety of this trend is not decided by scientific factors for it is a moral and political issue. The attention shown by the Women's Movement to SST was seen as another factor influencing growth.

13. Respondent. One of the areas in which it is widely used is in assertion training for women. That is very much the thing in the Women's Movement. And assertion training in Britain has almost become synonymous with women's groups.

The implication is that any attempt to fully explain the increase in the popularity of social skills training in this country might have to take into account of the particular interests of the Women's Movement.

The respondent also suggested that the social skills trainer's stance towards their clients might aid its popularity because of its emphasis on an internal locus of control.

14. Respondent. ...they see it as a less mechanical process than the other approach, and they see it as a way of people getting or achieving goals for themselves. I suppose that there is the implication that it is moving towards a more internally locus of control type behaviour therapy, in the sense that the individuals tend to have goals and are given the skills with which to achieve those goals.

This was not, however, seen as a feature of the Argyle type approach. In this extract, as with the previous ones, it is some extra scientific factor rather than any breakthrough in theory which is seen as leading to the growth of SST. The implication is that the choice between SST and alternatives such as traditional behaviour therapy is not guided by purely technical considerations but is partially dependent on broader social factors.

In each of these cases broader social concerns are depicted by the respondent as influencing the popularity of SST. The use of SST is taken to be congruent with a number of societal phenomena: the growth of the Women's Movement; the increasing emphasis on community rather than institutional treatment. Its popularity is also related to its stress on patients' responsibility for their own actions rather than being constrained by the will of the therapist. The respondent, however, although duplicating many features of the explanations used by interest theorists, does not relate the factors leading to the growth of SST to any explicit political or moral philosophy.

Discussion: Standard and Contingent Utility Accounts

In this chapter I have outlined the form of the standard utility account in a preliminary fashion and documented its existance in a number of different areas of metascience. I showed that in philosophy of science the traditional model could be used to undermine theories which cast doubt on the progress of science; progress is taken to be assured by the multifarious 'products' of science. In sociology the effects of the standard account are to direct attention away from a thoroughgoing social analysis of the contents of science. It is implied that social influences and distortions must be all but irrelevant in the procedures of science because these procedures lead to products which display, or appear to display, no social contingency at all. Even where a strongly sociological analysis of the role of science as an institution is suggested it is presupposed that science is in fact responsible for the technological artifacts which are the currency of modern capitalism. The very political importance of science is taken to lie in its lack of social contingency and thereby its practical potency. In each of these cases it is assumed that successful practical application provides a clearcut validation of scientific theory and that a large proportion of scientific theories have been validated in this way.

I went on to show that the standard model is not limited to analysts' discourse, but also appears in participants' own accounts of the applicability of their discipline. In general, characterisations of the applicability of social psychology articles were shown to reproduce the notion that successful practice is based on correct theory. Indeed this was encapsulated into the regularly repeated slogan that "there is nothing so practical as a good theory".

When a detailed account, by a single social psychologist, was examined the picture became rather more complicated. The standard utility account was certainly used to characterise application in places in the transcript. However, a rather different type of account was also apparent. This was less unified than the standard account. In places it simply implied that there was no relationship between social psychological theory and the practice of SST.

In other places a more complex ralationship was implied. This described social psychological theory as being transformed in certain ways when put into practice. For instance, the emphasis can be changed to suit training sessions in which clients are actively involved; or a theoretical approach can be used merely as a heuristic device in spite of basic theoretical inadequacies. The alternative to the standard approach also suggests that the formal literature on its own (even when orientated towards practical rather than theoretical concerns) is not sufficient for sustaining an effective and successful practice. Finally this alternative treated the choice of training techniques as partially dependent on broader social influences. Thus it was implied that the choice between using social skills or other behavioural techniques is not merely dependent on technical criteria but is also a function of the 'philosophical' perspective taken. SST would be chosen by those therapists, it is implied, who stress the autonomy and self-directedness of their patients.

Overall, this analysis shows that it is not possible to recover from the interview transcript one unified account of the way social psychological theory is utilised. For at different points in the interview contrasting versions of the theory-utility relationship are proffered. So far I have made little attempt to identify systematic relationships between the interpretative context and the form of these different accounts. All I have done is suggest that there seems to be a broad correlation between the contingent account and informal discourse; that is, although the standard account appeared in both the general articles and the interview transcript, the contingent version was restricted to the transcript. Clearly what is needed is a more detailed examination of the contingences of use of these differing accounts, so that we can improve on the gross, and in many respects misleading, distinction between formal and informal contexts of discourse. That is the task of the next chapter.

CHAPTER FOUR

MAKING THEORY USEFUL

In the last chapter I documented the existence of a widely distributed view of the way science is applied: the standard utility account. In this chapter I will discuss some theoretical and empirical problems with this account of application. One approach to these problems has been to develop an alternative model of the science-utility relationship. A model of this kind will be discussed later in the chapter; however, this will not be the approach adopted here. My concern will be to develop a more systematic analysis of the way scientists themselves _account for_ the utility of their work. Building on the findings of the previous chapter, I will examine the way application is depicted by a group of social psychologists at a conference convened to discuss applied research. In particular, I will try to explicate what is _achieved_ by different accounts of the utility of theory and how such accounts are fashioned to suit specific interpretative contexts. The conclusions of this analysis will then be used to make some more general comments about the role of utility accounting in various different kinds of discourse.

To start with, then, I will discuss research which is critical of the standard account of application. There are two obvious ways of challenging, or testing, this view. One is to examine whether successful practical application actually does validate the knowledge claims to which it is linked. The other is to question whether there is, in fact, a close relationship between theoretical scientific knowledge and effective 'technology'. I will take them in turn.

Questioning the Standard View:(1) Validation

As I indicated in the previous chapter, the notion that scientific theories are validated by successful practical applications is deeply embedded in the literatures of both science and meta-science. However, studies and arguments exist which suggest we should treat this notion with considerable skepticism. Not all of these can be discussed here, but I will describe some examples to illustrate the sorts of findings and arguments which are relevant.

In an important discussion of the issue of practice validating theories Bunge[1] has proposed a number of considerations that suggest problems with the standard model. By attempting to explicate exactly what would be involved in the process of validation he shows the futility of the exercise. He notes that because theories are typically made up of networks of ideas and propositions, and because not all of these would be equally involved in any practical utilisation, at most only part of a theory could be validated. Furthermore, because of the contingencies and uncertainties of the practical context there will be no attempt made to clarify what particular <u>parts</u> of the theory are relevant; indeed such an attempt would be doomed to fail. As Bunge puts it:

> A careful discrimination and control of the relevant variables and a critical evaluation of the hypotheses concerning the relations among such variables is not done while killing, curing, or persuading people, nor even while making things, but in leisurely, planned, and critically alert scientific theorizing and experimentation. (2)

This means that it is not possible to specify which parts of a theory are validated - the possibility of invalidity will always remain.

A further consideration is the difference between the requirements of accuracy in scientific and practical contexts. Bunge suggests that in applied situations the concern with accuracy is merely that it should be sufficient or safe; a high degree of accuracy, the espoused goal of much scientific research, would actually <u>impede</u> effective interventions in the uncontrolled and unpredictable environment that is the 'real world'. Coupled with this, theories may be reformulated or transformed in practical contexts. Some arguments to this effect were discussed in the previous chapter (pages 100-103 above). It is worth noting a few features of a detailed empirical study of transformation conducted by Ronald Rice and Everett Rogers[3].

Rice and Rogers propose the term 'reinvention' to refer to the way 'innovations' are changed in the process of adoption and implementation. This concept describes the situations where the actual use of a specific innovation is different to what was originally intended for it. Like Bunge, these authors stress the structured nature of theories and the possibility of selective adoption.

> The concept of reinvention also recognizes that an innovation is often really a bundle of components; it is possible to adopt some components and change or reject others. Typically, diffusion studies assume the existence of technical experts who ultimately make the decision to adopt or reject a monolithic, prepackaged innovation. In fact, there may be a fair amount of groping for a solution by concerned individuals, leading to alterations and later corrections to, the original innovation. (4)

Taking the case of 'Dial-A-Ride' responsive transportation systems, Rice and Rogers compared 10 of the systems adopted during the 1970's after promotion by the Urban Mass Transport Administration of America. They showed that reinvention was a pervasive feature of their case studies and involved a number of different phenomena. Thus some reinventions were planned and some reactive; some involved the technical hardware and others the organisation of operation. Despite a rather simplistic methodology and conception of innovation, and a case which is bordering on the non-scientific, this study is indicative of the possible transformation that can occur in utilisation. Indeed, it may underestimate the opportunities which arise when highly abstract and arcane natural scientific theories are applied.

The important point as far as the validation of theories is concerned is that if the theory is transformed in practice it becomes impossible to separate the effect of the change from the efficacy of the original theory. Moreover, if this is coupled with Bunge's observations about the complexity of practical situations and the different accuracy requirements of practical contexts, the possibility of any clearcut assessment of validity becomes remote to say the least. This means that theories which embody <u>different</u> assumptions and made <u>different</u> predictions in the scientific context can lead to <u>equivalent</u> results when used in practice. An examination by Wood of the use of child development theories in education appears to show exactly

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this[5]. When he looked at the utilisation of four contrasting perspectives on child development - those of Piaget, Chomsky, Bernstein and Vygotsky - he found a considerable agreement in the educational practices that were founded on them.

Exactly how common this phenomenon is is not the important issue. Simply the existence of such findings suggests the possibility that false theories can lead to successful practice; and this is surely the most damning finding against the idea that theories can be validated through application. Of course, this involves certain problematic issues: how are we to identify 'false theories'?; how can we ensure that a utilisation is in fact based on a particular theory? Nevertheless, there are numerous examples which appear to show that false theories can sustain effective practices. Cardwell describes a specific instance.

> In 1804 the Cornish engineer Arthur Woolf patented a new form of steam engine. The basis of the patent was a law of steam expansion that would appear to be completely wrong. By 1814 after some trial and error Woolf's engine was producing performances that one modern authority says "represented something like a 100 per cent improvement on the best performance of the Watt low pressure engine." (6)

Bryant gives a very similar account of the 'Silent Otto' engine which appeared 60 years later[7]. Again the engine was taken to be successful yet the theoretical principles used for its design were later seen to be mistaken. Even in the short participant's account discussed in the previous chapter we saw an example of a theory which the social skills trainer takes to be both false and effective in practice (extract 10, page 102).

It seems, therefore, that there are good reasons for doubting that successful practical application can be taken to validate theories. However participants may well reason along these lines. There is no uniform relationship between those theoretical formulations of science which are accredited as true and those utilisations that are accredited as successful in practical contexts. As Bunge has clearly demonstrated, 'the practical success or failure of a scientific theory is no objective index of its truth value'[8]. But this still leaves the question of the general quantity of application: is it the case that most technological innovation is dependent upon science?

Questioning the Standard View:(2) Dependence

There has been considerably more research into and discussion of the issue of dependence; that is, the claim that regardless of whether scientific knowledge is validated by technology, a great deal of modern science has in fact led to successful application and that most modern technology is science based. As Layton has noted, the results of this body of research are far from conclusive and the findings of particular studies are often contradictory[9]. However, although studies exist which do suggest that there is a strong, unidimensional relationship between science and practical application, numerous other studies come to the opposite conclusion. I will describe a few of these latter examples to illustrate the problems they raise for the standard model of utility.

In early research on the science-utility relationship, Price[10] examined the organisation of the literatures broadly concerned with theoretical science and technology. He found these bodies of published work to be organised in strikingly different ways, with the technological literature not displaying the intricately interconnected citation structure characteristic of the literature of science. Furthermore, there appeared to be little cross referencing between the two literatures. Price's conclusion is that there can only be a weak interrelationship between science and technology, and that on the whole science advances on the basis of past science and technology on the basis of previous technology.

A number of large scale studies have tried to 'trace' technological 'events' back to their scientific origins. These studies seem to have been innocently modeled on the standard version of the science-utility relationship and have been remarkably unsuccessful in documenting, let alone elucidating, the dependence of technology on science. For example, Sherwin and Isenson[11] tried to pinpoint the 'site' of innovation in US weapons systems (for which there had been an expenditure of 10 million dollars on science research). Their findings suggested that 91% of innovations originated form inside technology itself and only 9% came from scientific research events. Moreover, of these scientific events only 0.3% were in basic or pure science. And there was no suggestion that the 0.3% of innovations from pure science were any different in quality from the 91% from outside science altogether. A second project[12] was initiated to examine non-military innovation, but again was able to demonstrate very little connection betweent scientific events and industrial innovations. Barnes and Edge have recently suggested that these studies have become 'empirical phenomena in their own right, exerting a certain morbid fascination over those concerned with the history of science policy'[13].

Other work has looked in more detail at the contingendes influencing pure and applied research. For instance, Blume and Sinclair[14] studied chemists working within British universities. They found that chemists who worked closely with industry and the problems of industrial science were unproductive in comparison to other chemists. They also received poor evaluations from their peers. Blume and Sinclair conclude that 'British industry seems to have the attention of a minority of academic chemists whose committment to the scientific community is at the same time reduced'[15]. Moreover, the attention industry does have is from the least productive of the academic chemists.

On a more general level, one reply that might be made to this research goes as follow: even though detailed studies may fail to find a strong relationship, the huge increase in successful technology in the 20th Century must be a consequence of the concommitant expansion of research using scientific procedures. Mulkay[16] has indicated some problems with this line of reasoning. The central difficulty is that even if the growth in technology is a function of the growth in science - and after all many other changes have taken place in the 20th Century! - it might not be anything <u>intrinsic</u> to science which makes it applicable. Mulkay notes, for instance, that Babylonian mythological astronomy is now considered false, yet was responsible for applications considered successful[17]. It may thus be the systematic nature of knowledge, or its detail, which leads to utility; its truth may be irrelevant. As Mulkay puts it:

> given that industrial societies have devoted an ever increasing proportion of their immense 'surplus product' specifically to the production of systematic knowledge, we would expect a dramatic growth of knowledge related technology in modern society, without having to assume that the epistemological character of knowledge has altered with the advent of modern science. There is no need to assume that scientific knowledge is different in kind from 'pre-scientific' or craft knowledge, or that the 'rate of practical return' on scientific knowledge is remarkably high. (18)

Barnes has attempted to marshall the sorts of findings and arguments discussed above into an articulated model of the science-technology relationship[19]. In the terms introduced in the last chapter, Barnes's model constitutes a contingent account of scientific application. It is summarised and contrasted with the traditional model in Table 1[20]. Barnes treats science and technology as two independent and broadly equivalent subcultures, 'each with their own bodies of lore and competence'[21]. Each can draw upon the other as a resource, but technology is not constrained by the 'implications' of scientific theories and, moreover, an advance in technology will not necessarily be the result of an advance in science. When technology and science do influence one another the mediating agency is often the skills and knowledge of individual practitioners moving between realms rather than the formal, written communications found in journals and reports. Barnes does not elaborate his model in any great detail; and for the most part it is merely a systematisation of the kinds of findings discussed earlier in this section. Nevertheless, it constitutes one form of response to these results: the production of a definitive scheme for relating science to utility. The approach adopted here will be different.

Discourse and Utility

So far in my discussion I have documented research which shows that the traditional view of the strong relationship between science and technology is questionable; indeed, an entire alternative model of the relation can be articulated. But this work does not succeed in demonstrating conclusively that the relationship corresponds to the alter-

TABLE 1
Conceptions of the Relationship Between Science (S) and Technology (T)

THE INSTITUTIONS COMPARED	'BAD OLD DAYS'	PRESENT
FORMS OF ACTIVITY	S Discovery Creation of knowledge T Application Use of knowledge	S Invention T Invention
MAJOR RESOURCES	S Nature T Science	S Existing science T Existing technology
MAJOR CONSTRAINTS ON RESULTS	S State of nature T State of science	S No single major constraint T No single major constraint
FORMS OF COGNITION	S Creative/constructive T Routine/deductive	S Creative/constructive T Creative/constructive
THEIR RELATIONSHIP		
GENERAL IMAGE	S ↓ T Hierarchical dependence	S ⟷ T Egalitarian interactive
MAIN MEDIATING AGENCIES	Words	People
OUTCOMES a. For the development of knowledge	a. Predictable consequences. T deduces the implications of S and gives them physical representation. No feedback from T to S.	a. No predictable consequences. T makes occasional creative use of S. S makes occasional creative use of T. Interaction
b. For the development of competence and technique	b. S may make free creative use of T as resource in research.	b. Not a separate question. Interaction as above.
c. For the evaluation of knowledge and competence	c. S evaluates discoveries in an unchanging context-independent way. T is evaluated according to its ability to infer the implications of S. Success in T is proper use of S; failure in T is incompetent use of S.	c. S and T, both being inventive, both involve evaluation in terms of ends. No a priori reason why activity in T should not be evaluated by reference to ends relevant to agents in S, or vice versa.

Table 1. Reproduced from B. Barnes (1982) 'The science-technology relationship: A model and a query', Social Studies of Science, 12, 166-172.

native model. This latter claim remains no more than plausible. Thus we have a situation in which the analytical literature contains two contrasting, yet highly plausible and documented, views of the link between scientific knowledge and utility. The normal response to this kind of situation would be to try to decide between these two views, between the standard utility account and Barnes's alternative. Guided by the general perspective of this thesis I wish to adopt an alternative approach. It is suggested that the conflict between these two views is unlikely to be resolved because both views are continually reproduced in the social world under study through the interpretative accomplishments of participants. Analysts will be able to document both positions because they are both embodied in participants' discourse about theory application. For participants, the connection between theory and application, in general or specific instances, can be depicted as weak or strong, depending on the context in which accounts are produced. This much was tentatively indicated in the previous chapter. I will suggest below, however, that the traditional conception of a strong link between theory and practice is employed as a primary interpretative resource in certain important social contexts and that, as a result, it is this position which has come to be most firmly entrenched in the literature of metascience. It is only recently, as a variety of data have been drawn upon, that it has been questioned.

As I suggested in chapter one, the data on which analysts base their versions of the relationship between theory and application ultimately consist of participants' own interpretative formulations. Consequently, unless analysts pay attention to the systematic practices by which members interpret their own and others' actions, their analytical conclusions are likely to do no more than reflect the results of participants' interpretative procedures. There is no reason to think that the current situation in the study of theory-application is any different from other areas of the sociology of science. The analytic approach taken here, therefore, is not to try to infer from participants' symbolic products what is the <u>actual</u> relationship between theory and application in specific realms of knowledge, but to examine the interpretative repertoires and practices used by participants in organising their accounts of this relationship. I will not be concerned below with scientists' actions and beliefs as such, but with their discourse about action and belief. In other words, I will not offer another 'definitive analysts' version' of the connection between theory and application to add to the ones discussed in this chapter and the last; rather I will present some comments on how participants themselves construct their discourse on this topic.

I will develop these points by examining material taken from the discussions at a conference of social psychologists. This material is described in chapter two (page 76-77). To recap briefly, the conference studied was convened specifically to address issues in applied social psychology. It was attended by 16 European social psychologists including a number of respected figures in the field. Most sessions consisted of a presentation in which research was described, the comments of two discussants, and then a more general discussion involving the whole group. All sessions were transcribed verbatim.

I will examine three cases in detail. The first of these is not concerned with research conducted by the conference participants' themselves, but serves to articulate some of the analytic issues. The next two cases concern detailed pieces of research conducted by social psychologists present at the conference. The rationale for choosing these particular cases is that they are describing, in concrete terms, the application of specific pieces of research. Other participants either discussed problems of utilisation in general, talked about the application of disparate bodies of research, or discussed work on theories which were only described as potentially applicable[22]. The forms of accounting documented below are also present in these latter examples; however, the cases examined pose as the most straightforwardly 'applied work' described at the conference. This makes them the most apposite for study.

Example (1): Bridges and Physics

In the following passage, participants are discussing theory-application in general rather than specifically in social psychology. The passage centres on an interchange between Kopp and Leach, which refers back to a dispute between two other participants, Dunn and Friedell[23]. The initial dispute concerned whether theories should be developed in naturalistic contexts (in order to accomodate the complexity of natural situations), or whether they should be developed in abstract contexts, and then applied in combination to naturalistic contexts. Friedell takes this latter position, and supports it with an example.

> Friedell. There is nothing like a theory of bridge building; that's an application of many theories (E13).

Friedell suggests here that the practical implementation (building a bridge) is not in a one to one relationship with a single 'theory of bridge building', but is based upon 'many theories'. Although Dunn and Friedell differ in certain respects, they both present versions of what I have been calling the 'standard utility account'; that is, they both presuppose a direct utilisation of theory in practice. In contrast, Kopp enters the dispute with an entirely different picture of the relation between bridges and physical theory, which questions whether there is any kind of direct or indirect relationship between theory and the practical activity of bridge-building. This corresponds more to the view of sociologists such as Barnes who support a contingent version of utilisation. It is this claim which initiates the interchange seen in extract 1.

1. Kopp. (1)But, can I just make a point about building bridges. (2)This is that when people build bridges they don't use theories from physics. (3)And they use the tradition of building bridges. (4)They use tried and tested techniques, which aren't theoretically articulated, but they find work. (5)And if they, and they build bridges, and they model them in wind tunnels; and if they break down they remodel them. ( )

   Leach. (6)Oh, I think that is an awfully naive view of what civil engineering is all about. (7)(Kopp. But...) (8)I mean they don't use, spend five years, you know, messing about with wind tunnels when they learn the theory of civil engineering. (9)And admittedly it won't be nuclear physics they are learning, but it will be derivations from pure physics research, which have been tried and tested and amended. ( )

   Kopp. (10)I, I am actually, I mean, I would like to see, I mean that's the everyday; I think what you are saying is our commonsense notion. (11)But the research

I have read just doesn't support that. (12)I mean, I would like to see where it is shown that you can see the development from the theoretical law, the physical principles, or whatever being used by mechanics. (13) Because the studies which I have looked at just don't find that. (14)I mean, if you can show me I will jump for joy. But... [laughs]

Leach. (15)Look, all I can do is point to, I mean, I don't know very much about civil engineering.

Kopp. (16)But I think, no, I think it is important..

Leach. (17)But my point is that there's people who do spend five years or whatever it is, and architects spend five years, being trained. (18)Now are they merely trained into the professional myth that there is a body of knowledge which they have got? (19)Is that the argument? (20)Or are they actually being given pieces of information? (21)Codes of practice? (22)And a code of practice in the last resort,becomes a theory (E15/16).

In her first speech Kopp suggests that bridge building is a craft skill, based on its own traditions (3-4), and therefore not a product of physical theory at all. In sentences 6 and 8, Leach responds strongly to this point and accuses Kopp of naivety. Cutting off Kopp's protest (7), Leach elaborates his point. Engineers, he tells the gathering of social psychologists, do not spend 5 years simply messing about with wind tunnels. They spend this time, he suggests, learning the theory of civil engineering (8). While Leach notes that this will not be theory from nuclear physics, he asserts strongly that civil engineers will be learning derivations from pure physics research (9).

Kopp's agitated response in sentence 10 eventually leads to the counter-assertion that it is Leach who has not got beyond the commonsense understanding of bridge-building (10). In contrast to his claim, she suggests that her own views are based on a body of research (11, 13). Furthermore, she asks Leach to show that his belief is based on more than common sense (12). She implies that she would like to believe in what Leach is proposing, but that the evidence precludes such a view (14). She thus formulates a distinction between Leach's 'common sensical' view and here own 'research-based' understanding of the relationship in question.

Leach prefaces his reply to Kopp by saying 'I don't know very much about civil engineering' (15). This statement leads us to ask where he obtained the rather specific information that appears in sentences 8 and 9; the information that civil engineers spend 5 years learning derivations from pure physics research. I suggest it is adopted as part of a generalised, taken-for-granted model of how application is related to theory; that is, Leach's statements about bridge-building derive from his reliance on the standard utility account. As in the case of the social skills trainer, this should not be viewed as poor reasoning. Leach has simply made a routine inference about civil engineering from a common-place system of terms for talking about and making sense of a wide range of areas of practical action. The basis for this interpretation of Leach's discourse becomes clearer as the interaction continues. Leach cuts off Kopp's reply (16) and formulates a series of semi-rhetorical questions which reconstruct the issue. Instead of basing his argument, as Kopp had sought to do, on putative evidence about the nature of civil engineering, Leach attempts to show that Kopp's position is absurd in the light of what everybody knows. He asks in sentence 18: how could it be that civil engineers spend five years learning a professional myth that they have a body of theoretical knowledge? This unlikely summary of the nature of civil engineering, which has in fact at no stage been an explicit part of Kopp's account, is contrasted with what is presented as the only other possibility, namely, that engineers are '<u>actually</u> being given pieces of information' (20, emphasis added)[24]. In this passage, Leach makes no appeal to <u>specific</u> information or knwledge of civil engineering; the appeal is to our common sense. It is this taken for granted knowledge which tells us that it is absurd to think that civil engineers are trained into a professional myth and, therefore, that Leach's view of the relationship between theory and practice is necessarily correct.

So far, through sentences 17-21, theory has not been mentioned. There has been a gradual shift from 'knowledge' (18), through 'information' (20), to 'codes of practice' (21). However, theory is imported to end this series of formulations. In sentence 22 Leach says that <u>in the last resort</u> a code of practice becomes a theory. This series of formulations appears relatively coherent, especially in the fast flow of ordinary interaction; but contrast the final formulation with that in sentence 9. The shift here is from a theory of civil engineering being derived from pure physics, to civil engineers learning a code of practice which in the last resort becomes a theory. We might speculate that the extent of this reformulation is in some part a response to Kopp's claim in 13 that 'studies don't find' Leach's initial formulation to be correct. Leach dilutes his formulation in the face of Kopp's claim to scientific legitimacy. Yet the diluted formulation still retains the notion that theory underlies practice and that it is self-evident that it could not be otherwise. Leach's apparent qualifications ofhis initial claim, introduced in response to Kopp's opposing thesis, are used simply to enable Leach to encompass Kopp's points as minor variations on his central assertion.

In this extract I have illustrated how participants may draw upon structured sets of presuppositions, or accounting systems, when discussing the application of scientific knowledge. Kopp's strategy is to question the common sense view of theory and application in the light of systematic evidence. She formulates an account consistent with the contingent version of utility, which suggests that the interplay between scientific theories and particular technological artifacts has been exaggerated. In contrast, Leach's strategy is not to question the evidence, for Leach acknowledges that he is unfamiliar with either engineering practice or with research on the topic, but to re-state what everybody knows must be the case.

Leach thus draws upon the standard utility account which expresses the customary view of scientific application: namely that modern technology is straighforwardly based upon scientific theorv. The strength of his allegiance to this account may be gauged by the way it is maintained without direct experience of the subject under discussion and in the face of claims that the available evidence clearly contradicts his view. Although Leach does reformulate his position in the light of Kopp's criticisms, the presuppositions of the standard account are maintained. Moreover, the strength of the standard utility account (henceforth SUA) is also evident in Kopp's statement that 'If you can show me (that I am wronq in rejecting the SUA) I will jump for joy. But... (laughs)' (14). In this statement she seems to be expressing a desire to be able to accept the standard utility account, which indicates that she recognises its persuasiveness, whilst showing to the audience that she is forced by the evidence to withhold aquiescence. These two approaches to theory application, then, closely parallel the secondary literature, where sociologists initially adopted the common sense position, which was subsequently challenged on the basis of its inconsistency with the results of systematic study. In the rest of this paper I will concentrate on social psychologists' use of the standard utility account (SUA).

Example (2): Media Violence and Broadcasting Policy

The example above was intended to illustrate the notion of application-discourses and to reinforce the claim that it is difficult to accept these sorts of accounts as accurate, literal descriptions of scientific activity. The two speakers gave contradictory versions of the role of theory in bridge-building. We saw that Leach's version drew upon a general, taken-for-granted model of knowledge-application and was not even presented by the speaker as being based on direct experience or systematic study. Thus Leach's description of the process of utilisation cannot be separated from his assumptions about utilisation. However, the implications of this instance are restricted because the speakers were not talking about their own research or even about utilisations within the realm of social psychology. In this example, and the next, participants are discussing research which they either conducted themselves or which is taken from fields where they are expert. In each example I will compare an extract from a formal paper, an extract from a scientist's initial overview of the paper in the conference, and extracts from the ensuing discussion. In these more complex situations we will see that participants make a variety of specific technical evaluations. However, I will be concerned with the scientific details of these evaluations only insofar as they help to highlight some of the formal properties of this kind of discourse.

The following set of extracts are all concerned with 'television violence research' and its application. The research consists of experimental simulations of television and cinema viewing. In particular it concerns the way that the effect of film violence on people's behaviour changes according to how they are watching and who they are watching with. The first of these extracts is taken from the conclusion of the paper that Biggs has prepared for the workshop. Having described a number of experiments carried out by himself and his research assistants, he addresses the general issue of the application of his resarch.

2. (1)We are not applied social psychologists; rather we consider ourselves as experimental social psychologists who choose to investigate empirically and theoretically problems having implications for the society; with the hope that such knowledge will some day be applied by others than ourselves(1).

   (Footnote 1.) (2)Fortunately, this happens sometimes in the area of mass-media and violence in Europe. (3)For instance, the BBC television recently circulated a new guidance policy booklet which clearly takes into account the results of most recent research. (4)The Swiss television also devoted one of its most favourite programs to the problem of filmed violence and to our approach to this topic. (Biggs, unedited manuscript, p. 15)

Biggs writes, in sentence 1, that he does not regard himself as directly engaged in applied social psychology, but as producing knowledge which can be applied by others. The knowledge is applicable because it concerns social problems which have particular implications for society. The first sentence of this extract employs the SUA in that the author presents his empirical and theoretical work as having been undertaken with application in mind and his results as being applicable in principle to the solution of social problems. The link between theory and practice is weakened somewhat by the qualification that the ensuing knowledge will only 'some day' be applied, suggesting that application has not yet taken place. However, in the footnote, two examples illustrating the application of such knowledge are given (sentences 3 and 4). These examples suggest how the research in the body of the paper might be applied and also that it is likely that it will be applied. They inform us that television companies have taken notice of such research in the past; in one case incorporating it into a policy booklet and in the other basing a programme around the authors' theoretical approach. These examples thus provide a warrant for the applicability of the reported research and justify its inclusion in a conference on the application rather than the theory of social psychology.

The next extract is taken from the transcript of the workshop. It is from Bigg's initial exposition of his research. Biggs has said that he will read out his conclusion. However, although sentences 2, 3 and 4 are identical to the manuscript reproduced above, sentence 1 is modified.

3. (1)Most of the experimental social psychologists rely on the fact that their basic work has applications, or at least implications, for the surrounding society. (2)Fortunately this happens sometimes in the area of mass-media and violence in Europe. (3)For instance, the BBC television recently circulated a new guidance policy booklet which Swiss television also devoted one of its most favourite programmes to the problem of film violence and to our approach to this topic. (Biggs, transcript, B7/8)

In the transcript, Sentence 1 refers to experimental social psychologists generally and not just to Bigg's particular research. Furthermore, sentences 2-4, which appear in the footnote in the manuscript, are here <u>directly</u> used to warrant the applicability claim. Apart from these minor differences, which perhaps make the utility claim slightly stronger in the spoken version, both accounts clearly draw attention to the practical relevance of the reported research.

The first discussant takes up a number of issues raised by Bigg's paper. One of these concerns the influence of the research in practical contexts.

4. (1)...your work [is a good example] of really the way to look at a topic in a slightly more discriminating way than, certainly in the aggression research, was typical in the 60's. (2)But I think this presents a great problem for researchers who actually want to convince officialdom. (3)Because it is bad enough trying to get people to understamd what a main effect means, but once you start trying to describe what an interaction is, and perhaps even a second order interaction, then officials say 'well, you are obviously not very confident about it'. (4)If a programme on television doesn't necessarily lead people to be aggressive, but it goes through a peer group, or it only affects some children, or it may have a delayed response, I mean; you have obviously done poor research or there isn't really a phenomenon there, and you are just some woolly-minded liberal standing on a soap box and we hear one of those every other day. (5)And so they can discard it. (6)And my feeling is that as one becomes - and this

is a general point - that as one becomes more discriminating on the research on these complex sort of multi-faceted phenomena, it becomes more and more difficult to make uneqivocal statements, the type of statements which decision makers in authority are likely to pay attention to and do something about. (Aldridge, transcript, B10/11)

The speaker starts by complimenting the author for work which is more discriminating than that done in the 1960's (1). This compliment may be double-edged, however. For Biggs's research is only 'slightly' more discriminating than work done 10 years earlier. The possibility is left open of even better work done recently. The speaker then suggests a practical difficulty in applying Biggs's research (2). The problem is that the sorts of results that Biggs has obtained, because they require some degree of statistical skill to understand them, will not be persuasive to officials who make policy decisions (3). In sentence 4 the speaker parodies the image which these unspecified officials will have of the social research. He describes the ways in which the results will be interpreted as erroneous: 'there isn't really a phenomenon there'; or a product of political interests: 'just some woolly-minded liberal standing on a soap box'; and can thus be discarded (5). In the final sentence the speaker produces a general account of the dilemma facing social researchers; as their research becomes more subtle, decision-makers will pay it less and less attention and it will cease to influence their action. Thus the discussant tends to undermine Biggs's application claim, suggesting that the relevant officials will ignore his research, along with other similar work.

Biggs replies as soon as this discussant has finished speaking.

5. I am not so sure about your intuition, or your conviction, lets say, that the officials would be less touched, that this kind of research with interactions, no main effects, would have less impact. (2)Er, I am not convinced, but at least I have some evidence that they don't like, that peoplein the TV, for example, would like that once there is violence in TV it is bad. (3)They don't like that and they are not naive enough to think that it has no influence. (4)So, in some sense they are very glad that someone says that, you know, it can have effects, but not for everybody, not in all circumstances. (5)Maybe they will say that so they have nothing to do. (6)That could very well be, (7)But at least in one recent journal that position was, you know, advanced, that position was apprec-

iated, and on the Swiss TV also. (8)But maybe it is in order so that they have every justification not to do anything. (9)That can be. (10)But if you say violence on TV is bad I think it, you block them also. (11)Well, that would be my reaction. (12)I am not sure that would have less impact. (Biggs, transcript, B15)

In sentence 1 Biggs identifies the point he is replying to, and takes issue with it. He characterises the discussant's claims about influencing officials as 'intuitions' and 'convictions'; whereas he has 'evidence' (2) that these claims are unfounded. Biggs suggests, contrary to the discussant, that the 'TV people' dislike claims that violence is bad per se (2), and that they are also experienced enough to know that violence has some effect on viewers (3). These officials are thus 'glad' of interaction effects which support neither extreme (4). In sentence 7 Biggs repeats, in support of this claim, the two examples that appear in the paper and in the initial verbal exposition (sentences 3 and 4 in extracts 2 and 3). However, in sentences 5 and 8 Biggs provides a further, and significantly different argument against the discussant's point. He suggests that the officials may use research which recognises the presence of complex interactions to legitimate their inactivity. Biggs is implying here that officials may well respond to social psychological research in accordance with their interests and that they may use its findings merely to justify policies which have their 'real origins' elsewhere. This conception of the practical impact of social psychology seems far removed from Biggs's initial formulation of the SUA.

We can see that the notion of 'impact' (12) is subtly attenuated as Biggs interacts with his critic. By the end of extract 5, 'impact' may mean that results are put into some sort of practice; but equally it may mean quite the opposite, that the results 'lead to' inactivity. In this extract Biggs has fashioned an account of the (potential) impact of his work specifically to repudiate the discussant's suggestion that it will have little impact. However, in doing so, Biggs has introduced an interpretation which was not used in either his paper or his initial verbal exposition. This interpretation is suited to the specific interactional task of undermining the discussant's central criticism, in that it enables the speaker to maintain some kind of utility claim even in a situation where his research may have no ostensible outcome. Thus this example illustrates the flexibility of the notion of application in informal discourse. The SUA can be employed both in instances where officials are depicted as using the research to guide policy and to educate the public about the problems of television violence, but also where officials use the research as a rhetorical justification for doing nothing. The notion of application is so open-ended that the speaker is able to draw two quite contradictory accounts of action together as documents of the utility of his research. As we will see in later chapters of this thesis, such interpretative flexibility is typical of informal accounting and is one of the principal reasons why general-purpose interpretative devices like the SUA are so widely adopted. With a little ingenuity, they can be applied to virtually any particular case.

Example (3): Theoretical Analysis and Energy Consumption

The third example concerns a discussion of research by Aldridge and Gough on the conservation of energy in private households. Again no appreciation of the technical details of their work is essential for understanding the analysis. The research involves comparison of energy savings in households which have been given feedback on their consumption and households given information about effective strategies for energy conservation. In the following extract from Aldridge and Gough's paper a strong case is made for the integration of pure and applied research.

6. (1)Following our previous argument [concerning shortcomings in purely empirical approaches to energy conservation], we would assert that the applied problem of energy conservation can only be effectively analysed and solved through theoretical analysis and related empirical research. (2)Of course, empirical research can produce a solution to a particular situation but any change in that situation which affects consumers' reactions can only be countered by further exploratory research to find a new solution. (3)The advantage of a theoretical analysis is that it allows for prediction of practical procedures to obtain optimal results in

> both stable and changing situations. (4)Moreover it gives rational direction to further research and allows for the identification of anomalies requiring consideration. (5)Theory must be an integral aspect of the research process not an optional extra. (6)It is in this context that distinctions between pure and applied research become somewhat meaningless: good research requires both. (Aldridge and Gough, unedited manuscript, p. 15-16)

In the first sentence the authors strongly assert that the solution to the specific problem of energy conservation is dependent upon the integration of theoretical analysis and emprical research (1). In the next three sentences grounds are given for this claim. Firstly it is suggested that emprical research alone is poorly suited for dealing with changing situations. Each change requires a fresh study to find a new solution (2). Theoretical analysis, in contrast, can predict how practical procedures should be changed to deal with fresh situations (3). Secondly the authors claim that theoretical analysis has the virtue of directing future research and identifying anomalous results (4). Although in sentence 1 the authors refer to the <u>specific</u> problem of energy conservation, their argument seems to be broadened through sentences 2-4 to deal with application in general. The central point is forefully repeated in general terms in sentences 5 and 6; theorv and application must be drawn closely together. Again we have an example ofan SUA which stresses the essential link between theoretically informed research and successful application, and which presents the speaker's actions as exemplifying that link.

These points are restated in Aldridge's initial overview of the paper, before any discussion has taken place.

7. (1)I really do consider that all empirical research should be theory related. (2)There are certainly occasions where a little crass empiricism doesn't do any harm at all, but I think in terms of the solutions to practical problems, and a development of an understanding of the psychological processes which may be involved, some kind of theoretical underpinning, theoretical framework, is absolutely vital. (3)Because it seems to me without theoretical underpinnings a practical solution which might emerge from a piece of empirical research is essentially static and is essentially only short term. (4)A purely emprical solution provides no <u>rational</u> way of making decisions about further research, provides no rational way of dealing with anomalies. (Aldridge, transcript, A2)

The first two sentences reassert the importance of theory for the solution of practical problems. As with the written version, the claims are strongly put: 'I really do think that all empirical research should be theory related' (1, emphasis added). Aldridge accepts that there are occasions when 'a little crass empiricism' does not actually harm the research. But he seems to suggest that this is less true when practical solutions are being sought. In these latter cases theoretical support is 'absolutely vital' (2, emphasis added). The written and spoken versions are thus very similar; with the latter being perhaps slightly stronger.

During the initial discussant's comments and ensuing general discussion, Aldridge and Gough's position is criticised a number of times. This leads Aldridge, at an early stage in the discussion, to state more explicitly how their work gives rise to practical action.

8. (1)The whole point of our research is not actually to persuade people to conserve energy, but to give people the wherewithall to use energy more efficiently if they so choose. ( ) (2)We are not in the business, it seems to me, of social influence by shaping the general public into certain ways of behaving. (3)It seems to me that it is so eminently rational for them to behave in the right fashion, given the information, and given that the price of energy will go up, etcetera, etcetera, we don't need to worry about motivation. (Aldridge, transcript, A12)

Aldridge compares his research to that on 'influence' (1-2). In contrast to that research, he suggests, he is concerned with providing the information to enable people to conserve efficiently (1). He argues for his own approach by the further claim that price increases will be enough to persuade people to conserve (3). All that is necessary to increase conservation is to give people information about how to do it effectively. This was criticised by Hearn, the initial discussant. He makes the point that the relationship between price risesand motivation to conserve may be weaker than Aldridge and Gough suggest. The following extracts are from Hearn's first three formulations of this criticism.

9a. (1)I'm just trying to think of how, em, generally viable is the model which you present which is, implicitly, that people know that energy conservation is a good thing, but they don't have the facts at their disposal to know how to go about saving energy. ( ) (2)I don't think it is just simply clear that nobody has taught them and now we musthave a theoretically based information campaign to tell them, and the, and things will somehow be better after that. (A7)

9b. (1)Can I just on, just query you on that rationality thing? (2)I mean, what would you say, in terms of saying this is clearly irrational, to somebody who says 'well look, the way things are going in ten years time I just won't be able to afford to run a car, so right at the moment I am just going to burn up and down in my TR6 or 7 or 8 or 9'? (A13)

9c. (1)All I am saying is that the message that prices for energy are high and likely to become higher, might have the effect of making you want to save or it might have the effect of lets use it as I, as, while we have got it. (Hearn, transcript, A14)

Hearn gives a gloss on Aldridge and Gough's model (a1) and suggests that one of its basic presuppositions - that increased information will lead to increased conservation - is possibily unwarranted (a2). Hearn does not accept Aldridge's reply to this point, and reformulates it with a specific example. He suggests that there is no <u>compelling</u> rationality to conserve. On the contrary it may be rational to use as much energy as possible while it is still possible to afford it. Aldridge's reply still does not satisfy Hearn and he gives a further reformulation, stating the point even more bluntly (c1).

We will now examine three responses to this criticism: two from Aldridge and one from Gough. The first is from Aldridge.

10. (1)One of the things that I think is relevant to that, I excluded it from the paper because, I think, we really can't think of a theoretical underpinning for it - it seemed to make this statement about theory and then produce a whole string of interesting results which didn't fit in... (2)Behaviours are located in group norms, and that one of the more effective ways of changing behaviour is to change those norms. (3) And so we are doing some research for example on the use of schoolchildren. (4)It's not terribly well structured but we see ( ) schoolchildren as potentially people who are carrying home new ideas, innovatory ideas, to their parents to change them. (Aldridge, transcript, A15)

Aldridge here replies to Hearn's point by outlining some research which he and Gough had conducted on social influence, more specifically on the influence of school children (2-4). This reply undermines the previous statement that they believe providing information to be sufficient on its own; it suggests instead that they have been looking at the effects of <u>both</u> information and persuasion (2), but that only data on the provision of information have been included in the present paper. What is most interesting from the present perspective is Aldridge's preamble to this statement. In this he accounts for the absence of the persuasion research from the paper and from the spoken presentation by noting that it is atheoretical, and thus could not be included in the paper as it would be inconsistent with the account of theory given there (1). Yet, in accounting for its exclusion, Aldridge comes close to recognising explicitly that there is a contradiction between this version of their work and that offered previously. This may be why sentence 1 remains uncompleted. Aldridge certainly stops at the point where the contradiction seems about to be made <u>fully</u> explicit. To summarise, Aldridge has produced an account of his research in reply to Hearn's specific criticism. In so doing, as he himself notes, he has departed from the rigorous utility account which is proposed in the paper and in the initial overview. Thus the SUA, with which Aldridge and Gough began the presentation of their work in the context of this conference on application, can be seen to be no more than one possible version of that work which the authors decide to present on this particular occasion. The contingency of this version becomes apparent only by means of close examination of participants' discourse, in the course of which alternative accounts are generated.

Aldridge's reply in passage 10 is not taken as sufficient by Hearn , who, after some discussion of related theoretical points, restates his criticism. This time Gough replies.

11. (1)I mean, there is a, there could be a completely different aspect on this research which is, perhaps, the way the research has developed out of the pilot research which was done, and the way in which the problem was defined, and so on. (2)(Hearn. Yes.) (3)Em, because it in a way, it is a fall back position from the original position, which was - although it was never probably formally stated - the original sort of ideal behind the research was promoting energy conservation, and persuading people to conserve, and very much bound up with social influence. (4)The fall back from that position to a position of trying to put people in a position where they can make better

decisions about energy, if they have to, was, in a way, a fairly defensive one because, since the energy crisis over whether there will be a shortage of energy or anything, or what it will be like in 20 years time, we haven't actually got a stable base to work on. (5)Because energy conservation, change of government, change of policy amongst the oil developing, er, oil producing countries and it is all wiped out and energy conservation is out the window. (6)So that to have a stable base for doing research, and so on, if you step back to not worrying about energy conservation and saying this is a good thing in itself, to educate people and inform them and if it comes we will know how to deal with it. (Gough, transcript, A20/21)

The structure of this account can be understood as dealing with the task of showing, much as Aldridge did in extract 10, that they accept the general force of Hearn's argument (that persuasive factors might be important), but that it does not apply to these specific results. Gough suggests that their <u>initial</u> research position was 'very much bound up with social influence', and he forestalls any scrutiny of this claim by noting that this was an 'ideal <u>behind</u> the research' which was never formally stated (3, emphasis added). He goes on to cite a pragmatic reason for changing to a completely different 'fall back position' (3). This is that, given the volatility of the political and international situation, they would not have a stable base on which to carry out their research (4). Thus Gough characterises this change of positions as enabling them to avoid having to deal with the changing political situtaion (5-6). However, in the paper and in the initial overview of the research, theoretically based research is depicted as and justified as a way of solving the problems that purely empirical research meets in dealing with dynamic situations.

It seems, then, that in the course of resisting Hearn's criticism, Gough departs radically from the written formulation; instead of reaffirming the version of their work in which <u>theory</u> is used to guide both research and practical application through various changing social situations, Gough now describes a methodological decision not to look at persuasive factors, which will be in flux, but to concentrate instead on informational factors precisely because they will be static (6). Furthermore, in describing their interest in persuasion as having been abandoned for pragmatic reasons, Gough produces an account which is inconsistent with Aldridge's in extract 10. In that account social influence was characterised as a continuing interest as yet in need of theoretical underpinning. Here, then, we have an instance of the joint authors of a paper giving, in the course ofinteraction, strikingly divergent accounts of the research activity depicted in the paper. Once again, the contingency of any particular account of their research and its dependence on the interactions occurring at the conference, is made strikingly evident.

At this point Aldridge gives a further reply to Hearn's criticism.

12. (1)I think also as a result of our research it is fairly clear that most people thought it was a good idea. (2)Most people admit that they do something but tend to do something that is rather ineffective without knowing. (3)(Hearn. Yes.) (4)So in a way talking to people persuaded us that that was really the place to start. (Aldridge, transcript, A21)

In this account Aldridge justifies the energy conservation research in purely pragmatic terms. He claims that 'most people thought it was a good idea' (1), and that talking to people persuaded them that it was a good place to start (4). In the light of the failure to satisfy Hearn as to the theoretical grounding of their research, Aldridge draws upon a purely pragmatic consideration to legitimate their approach. He accounts for their research in terms of people's lay evaluations, without making reference to theory. Thus, in the course of responding to Hearn's repeated criticism, the standard account of the relationship between theory and practice appears to be clearly abandoned and in its place a purely pragmatic justification is used. This in no way implies, of course, that the SUA of this research will not be employed again on different occasions or later in the same conference. Despite their production of several radically different accounts of the relationship between theory and application in this research, at no point in the discussion does Aldridge or Gough explicitly withdraw the initial utility account. Nor do any of these alternative versions of their research appear in the published version of their paper, even though it was explicitly suggested that people rewrite drafts in the light of the conference discussion.

Discussion: Utility Accounts and Interpretative Contexts

In the preceding analysis I have illustrated how social psychologists often use a particular interpretative device for depicting the relationship between theory and application. This I have termed the 'standard utiliity account'. It is a highly general and stereotypical accounting system for depicting this realm of social action and belief. By means of this system, application is typically described as 'derived from' or as 'enabled' or 'correctly guided' or 'rationally directed' by theory. Alternatively, theories are said necessarily to have 'implications' for society or to be a 'vital underpinning' of the solutions to practical problems[25]. In short, the standard utility account treats the existence of a strong relationship between theory and practice as proper, typical and generally unproblematic.

In each of the three examples examined above, the SUA was given interpretative primacy by at least one of the parties involved. In example one, we saw a speaker treating the SUA as obvious in the case of civil engineering. Interactional problems arose from this use of the SUA because the speaker was unable to claim any direct familiarity with engineering practice or with the relevant research literature. Nevertheless, the SUA was strongly reasserted in response to another speaker's more contingent description, on the grounds that the practical success of engineering <u>necessarily implies</u> that engineering practice ultimately derives from a sound body of scientific theory. The advocate of discontinuity between theory and practice was unable to elicit any withdrawal of the SUA, despite the apparently flimsy basis for her opponent's position. Indeed, her own alternative position was to some extent weakened by her acknowledgement of the interpretative potency of the SUA.

It seems likely that the SUA is most interactionally effective, and least likely to be subject to detailed qualifications, in situations like that occurring in the first example, where speakers are talking in generalities about areas of action in relation to which they have little first-hand experience. By contrast, in examples 2 and 3, speakers were dealing with research areas with which they were very familiar. Consequently, their interpretative work was more detailed and concrete; and a whole range of interpretative modifications were brought about in the course of interaction.

These interpretative modifications were analytically useful. For they made it possible to demonstrate the contingency of participants' specific attempts to portray and justify their own professional work by means of the SUA. Nevertheless, despite clear evidence that interactional difficulties followed from participants' use of this interpretative formulation, in none of the three cases did a speaker abandon an SUA. Rather, they engaged in supplementary interpretation which allowed them to retain the SUA as one legitimate characterisation of their work. For instance, the speaker in example two extended the meaning of 'application' to include cases where theoretical analysis produced no discernible practical outcome. Whilst the speakers in the third example generated a whole series of alternative and incompatible accounts, yet retained the SUA in the final text of their paper.

In examples two and three we observed social psychologists presenting their work in terms of the SUA when engaged in fairly formal discourse. It is only in the course of unpredictable informal interaction that these actors come to qualify, modify and contradict their initial, formal SUAs. There seems, therefore, to be some indication of broad contextual variation in the use of utility accounts, in a way which parallels scientists' use of empiricist and contingent repertoires[26]; that is, participants appear to use a relatively wide range of utility accounts in the course of informal interaction, but to select from this range a somewhat standardised and restricted kind of utility account for more formal contexts. This finding corroborates the conclusions of the previous chapter.

Clearly, the results of these analyses are highly tentative. They are based on an examination of only three examples from the transcript of one conference and one interview. Moreover, the conference was convened specifically to address issues in applied research and the interview to discuss application. Consequently, there will have been an exceptional stress on the applicability of social psychological research in these settings. Thus I am certainly not claiming that this sort of accounting will be pervasive in <u>all</u> areas of science on <u>every</u> occasion. However, let us accept as provisional possibilities that scientists recurrently characterise their professional actions in terms of the SUA; that such accounts cannot be treated analytically as literal descriptions, but must be regarded as contingent and context-dependent members' interpretations; that the SUA tends to be in certain respects primary in informal interaction and even more dominant in formal discourse. If these provisional results were firmly established by further more extensive and systematic research, would they have any interesting analytical or empirical implications? I will examine some possibilities.

In the first place, the question arises as to why the SUA should be primary and why it should be particularly prominent in relatively formal texts. As a provisional interpretation, I suggest that the notion of utility provides a powerful source of legitimation for a great range of actions in industrial societies, including that of knowledge-production. In our society, it is difficult to discredit any action which can be successfully depicted as facilitating control over the physical or social worlds. By characterising research in terms of the SUA scientists fashion a potent legitimation for the acquisition of funds and other scarce social resources.

Given this interpretation, it would be expected that the supposedly strong relationship between theory and practice would be particularly emphasised in interaction between researchers and non-specialists. For it is the non-specialist who must be persuaded of the practical results of theoretical work, if funding and other forms of support are to be acquired. It would also be expected that this general purpose legitimating device would be absorbed, perhaps increasingly in the current climate, into the informal discourse of the research community and employed in various appropriate circumstances. There is at present little systematic information available on scientists' informal interpretative procedures. It is, therefore impossible to document how frequently or when the SUA is used among researchers. There is, however, a considerable body of evidence showing that it is regularly employed by scientists in their dealings with laymen[27].

In the case of social psychology it is possible to identify publications which are aimed to show that the discipline makes important and frequent contributions to the society at large[28]. Without engaging in any detailed analysis, these articles appear to be constructed through giving brief overviews of research conclusions from different areas of social psychology and combining each conclusion with a SUA. In some respects these accounts are like examples 2 and 3 above, where SUAs are applied to specific pieces of research. However, these accounts also share some of the features of example 1; they give brief summary descriptions of work intended for readers who have no first-hand knowledge of the area. This kind of formulation, as I noted, appears to be interactionally very effective. Thus in the form of a written text it may present the reader with a version difficult to deconstruct. Indeed, in many ways such accounts are organised to appear self-evident: as we know X we must be able (in society at large) to do something about/to X.

Given the flexibility of the SUA, it seems well suited for legitimating research where other sorts of justification are unsatisfactory. Gilbert and Mulkay[29] have suggested that justifications based on empiricist accounts may be sufficient in a variety of informal and formal contexts in science, and particularly so when scientists are presenting their knowledge claims as being determined by the carefully controlled experimental explication of facts. Yet these accounts are likely to be opaque to lay people and inadequate on their own for funding agencies and political organisations. Here accounts of the practical returns of science may be far more persuasive. It is not that the SUA replaces the straightforward empiricist account on these occasions, but it supplements the account and joins the arcane world of science to the everyday world of non-scientists. The SUA can thus be viewed as part of a 'vocabulary of justification'[30] or of an 'ideology of application'[31]. It may protect the collective interests of scientists by portraying their sectional interests as universal and by suggesting that the support of science in particular will be to the general good. For instance, by supporting research on television violence, we can be seen to be helping to cut down crime on the streets. I have shown in the above analysis that this sort of practical characterisation of research activity can be more or less effectively maintained because of the generality and flexibility of the utility account. This argument suggests that it would be fruitful to examine the organisation of application accounts in more detail as well as the ways in which accounts are devised in accordance with variations in interpretative context. The preliminary conclusions suggest that SUAs should be more common in contexts where other legitimations of theoretical work are interactionally inappropriate.

Overall, then, it is possible to develop a tentative explanation of why SUAs are often treated by participants as primary in informal settings and why they are adopted in formal, generally accessible, discourse about knowledge-production. In addition, these findings have implications for research into theory-application. It is evident, of course, that the kind of research being suggested here will not enable us to answer traditional questions on this topic. In particular the investigation will not help to reveal how far practical action does actually depend on theory. At the start of this chapter I mentioned the contradictory answers to this question presented in the secondary literature on science. In this respect, the secondary literature simply reflects the variability of participants' own interpretative work. As I have shown, there was considerable variability in the accounts of scientific action and research given at the conference. This can be seen in each of the three examples discussed. This variability appears to pose a dilemma for the researcher who tries to use such accounts as data on the relationship between theory and practice. Given that different accounts of the 'same' action are divergent and, in some cases, contradictory, the analyst who wishes to answer the traditional question must decide which accounts are correct and which erroneous, in order to construct his or her own analysts' account of theory-application. As I have argued in chapter one, however, there are no satisfactory procedures for building up analytical versions of action in this way[32].

The perspective on utility accounting adopted here suggests an explanation for the variability in the literature on scientific application. This variability may be unrelated to actual utilisation of scientific theorising, but may instead be an artifact of the contexts in which the discourse about theory-application used by the analyst was produced. For example, discourse taken from contexts where empiricist legitimations are insufficient and where SUA's predominate may lead the analyst to conclude that interchange between theory and practice is pervasive. Alternatively, discourse taken from contexts where empiricist legitimations are sufficient, for instance, in informal interviews where scientists are commenting on each other's work may lead to the reverse conclusion. Indeed, in the latter situations both empiricist descriptions <u>and</u> SUAs may be abandoned at certain moments in favour of contingent versions of scientific activity. It may be, therefore, that the 'new model' of the science-technology relationship formulated by Barnes and others is a direct result of changing research practices which have placed increasing emphasis on informal sources of information.

If we accept that participants' accounts cannot be used as inert data about their actions outside the context in which those accounts are produced, but instead must be treated as the means through which participants accomplish contingent meanings for their actions, we can start to develop a more viable analytic approach. This point can be illustrated using example 3 above. Here Aldridge and Gough give a variety of different accounts of their research. This variability becomes intelligible if each account is itself taken to be a type of action rather than merely a representation of action. For instance, in extract 12, after sustained criticism from Hearn of theoretical aspects of their research (extract 9), Aldridge formulates a purely pragmatic case for the utility of the research. Although inconsistent with earlier accounts (notably the stress on theory as all-important), it is highly suited to the task of justifying the research as useful independently of the specific theoretical underpinning which is being challenged by the discussant. In this type of analysis we have advanced beyond the preliminary formulations of the previous chapter in which the type of account used was related only to broad shifts in social context. Here the participants themselves can be seen to be constructing and modifying accounts in line with the specific interpretative contexts which they also help to reproduce[33]. Within broad social contexts - the formal literature, the conference, the interview, and so on - there can be many changes in the specific interpretative context. That is to say, the meaning of the interpretative context is not determined by the nature of the social context, but is a social accomplishment. Indeed, social contexts themselves can be seen to be constituted by the recurrent use of particular systems of accounting.

CHAPTER FIVE

TESTABILITY, FLEXIBILITY

In the previous two chapters I examined the organisation of accounting in the context of the utility of scientific theories. The issues raised in this analysis will be developed in the next two chapters through an analysis of Kuhnian notions about scientific development and structure. In both of these chapters I will be concerned with the flexibility inherent in seemingly precise and clearcut linguistic categories when they are drawn upon in different interpretative contexts, although it is not until chapter six that I will start to look in detail at the ways in which such flexibility is achieved. For the moment I will concentrate on the notion, developed by Kuhn, that certain broad values constrain theory choices and thereby allow scientific progress. Kuhn's general argument will be discussed and then compared with an analysis of value accounts produced at a scientific conference. As a resolution of some of the difficulties raised by this analysis, I will suggest that values should be viewed as a flexible repertoire of interpretative resources which scientists' selectively draw upon when warranting their own theory choices and undermining their opponents'.

Values and Kuhn's Model of Progress

In 1973 Kuhn[1] responded in some detail to accusations that his views led to the inevitable conclusion that scientific progress was irrational[2], and ultimately a matter of mob psychology[3]. Kuhn's denial of these accusations rested on a discussion of the crucial role played by scientific values in the choice between competing theories. He suggested that scientists are socialised into the use of a number of broad scientific values, such as accuracy, consistency or scope and that, because they are largely independent of particular scientific theories or frameworks, they provide a rational basis for theory selection. Ultimately they ensure the rational progress of science, which to some had seemed so fragile following the publication of _The Structure of Scientific Revolutions_[4].

There is considerable subtlety in Kuhn's notion of values. He is unequivocal about the importance of values; 'they provide _the_ shared basis for theory choice'[5]. Yet the actual process by which values effectively constrain scientific development is complicated. He does not suggest that scientific values _determine_ theory choice. On the contrary, he notes that 'two men deeply committed to the same list of criteria for choice may nevertheless reach different conclusions'[6]. This situation arises because of unavoidable difficulties in relating broadly formulated criteria to multi-faceted practical situations. For example, Kuhn notes the difficulty in applying a criterion of accuracy to the choice between oxygen theory and phlogiston theory. Although oxygen theory could account for the weight relations in chemical reactions, phlogiston could account for the fact that metals are more similar to each other than to their ores. The criterion of accuracy is thus, on its own, insufficient. Either theory can be held to be the more accurate, depending on the way in which accuracy is interpreted in this context.

The situation becomes even more complicated when we consider more than one value. Kuhn illustrates this with the tension between the values of simplicity and consistency in the choice between heliocentric and geocentric astronomical theory. He notes that Ptolemy's geocentric system was both internally consistent and consistent with much more of the broader physical theory of the time than Copernicus's heliocentric system. Consistency alone would thus unambiguously favour the geocentric system. However, Copernicus's heliocentric system was simpler in the sense that certain broad features of planetary motion could be calculated with the aid of fewer mathematical assumptions. Each theory therefore satisfied one value but not the other. Thus values on their own could provide no unambiguous criteria for the selection of one or other of these theories. To understand why a particular theory choice is made in the way it is, Kuhn argues, we must go beyond the list of shared values, to examine the characteristics of individual scientists and aspects of their social context.

We should not, however, imagine that Kuhn is embracing some sort of relativism, some sort of strong sociological explanation of scientific activity. This is exactly the accusation he is trying to refute. While he attacks the idea of producing a well articulated objective algorithm of theory choice, he claims that values have considerable effectiveness in practical situations. He contrasts his claims about the functioning of scientific values with philsophers' discussions of the role of crucial experiments in arbitrating between scientific disputes. His position, he claims, is descriptive of scientific practice, while philosophers' discussions are simply post hoc rationalisations. According to Kuhn, crucial experiments are not a factor in the actual choice between competing theories, they are performed or at least recognised as 'crucial' after the choice, to provide illustration and legitimation. Values, on the other hand, are actually used by scientific participants in the choice between theories[7]. How, then, can values be effective in practical situations, where their application to specific theories is ambiguous and where they may conflict with each other?

Kuhn suggests that this indeterminacy of values is actually functional for the development of science. Without it the creative processes of dispute and theory testing would be lost. But how exactly do values ensure this creative orderliness? Kuhn suggests analogies from other spheres of social life to clarify the issue. He notes that proverbs, such as "Many hands make light work" and "Too many cooks spoil the broth", are frustratingly vague, and also in apparent conflict with one another. Nevertheless these maxims:

> alter the nature of the decisions to be made, highlight the essential issue which it presents, and point to those remaining aspects of the decision for which each individual must take responsibility himself. (8)

In the same way, he suggests, values and norms provide effective guidance in situations of choice and uncertainty. For example, the values of freedom of speech and the preservation of life and property may conflict, such that freedom of speech may have to be curtailed in certain situations, when it leads to a riot, for instance. Yet we do not suggest the abandonment of such values. We are acutely conscious:

> that there are other societies with other values and that these value differences result in other ways of life, other decisions about what may and what may not be done. (9)

Despite being superficially convincing, both of these examples of Kuhn's soft determinism are problematic. He draws upon our unexplicated common-sense knowledge of the functioning of proverbs and social values. Proverbs alter the nature of the decision and thus its outcome, claims Kuhn. But do they actually do this? Are they really more than post hoc legitimations of action? Again, with social values: is it actually the possession of certain values that leads to differences between one society and another? Sociologists have been far less ready to characterise the causal chain in this way. It is just as plausible to treat the different actions which occur in different cultures as products of the way in which participants use values and other cultural resources to give meaning to their actions. Thus merely to point to the prevalence of different actions in separate cultures in no way demonstrates that actions are the result of actors' conformity to cultural values, rather than their interpretative use of such values.

Kuhn's discussion of the operation of values in science itself does little to clarify their putative function. Kuhn says tantalisingly little on this issue. Referring to the problem of scientists' sharing the same values but making different choices, he writes:

> differences in outcome ought not to suggest that the values scientists share are less than critically important either to their decisions or to the development of the enterprise in which they participate. Values like accuracy, consistency, and scope may prove ambiguous in application, both individually and collectively; they may, that is, be an insufficient basis for a <u>shared</u> algorithm of choice. But they do specify a great deal: what each scientist must consider in reaching a decision, what he may and may not consider relevant, and what he can legitimately be required to report as the basis for the choice he has made. (10)

Kuhn suggests that values identify certain features of scientific theories as important, and relevant to the choice between theories. But this contention is either empirically empty or it is inconsistent with what he has argued before. In suggesting that values specify the factors which must be considered in reaching a decision, Kuhn seems to be doing no more than saying that values specify themselves: a value of accuracy, say, merely specifies that accuracy is important. However, this leaves totally unresolved how the value is to be interpreted in, and thereby guide or constrain, any specific choice. If he wishes to regard values as constraining scientists' choices, as 'specifying what each scientist must consider', Kuhn must describe what these values specify for each scientist. Yet Kuhn provides no such specific analysis; and, indeed, he cannot do so whilst he continues to emphasise the essential indeterminacy of scientific values.

Perhaps it is not surprising that Kuhn does no more than provide this almost tautological description, for his is the difficult task of describing in general terms the effect of broad formulations on idiosyncratic practical instances. Kuhn has produced a number of strong arguments as to why values do not determine theory choice. But his case for values nonetheless, in some unspecifiable way, constraining choice appears much weaker. It rests largely on our common-sense assumption that generalised values are in fact important in directing our activity down particular avenues. This seems to be fundamentally an empirical issue. We need to know exactly how a value or set of values constrains choice in any particular scientific debate. The rest of this chapter will be concerned with the empirical analysis of the functioning of scientific values.

Values in Psychologists' Conference Discourse

It is particularly difficult to make an empirical evaluation of Kuhn's model of theory choice because of the tensions within his argument. On the one hand, he stresses that values are largely consensual and longlasting attributes of science, with a meaning which is independent of any specific theory or perspective; and on the other, he notes that they change with time and according to theory selection[11]. These claims make his position vague and possibly inconsistent. If values change over time and with the choice of certain theories, how can they constrain those choices? Kuhn claims that they do not change enough to make a difference, but does not elaborate this point[12]. The actual process of constraint is never made explicit. The only examples he gives are simplified panoramas of historical episodes that occurred over many years. It is clear that this degree of ambiguity makes any evaluation of the model very difficult. As it stands it is not clear at all what sorts of findings would cast doubt on it.

It is crucial to any examination of the functioning of scientific values that we note that they are considered to be participants' concepts. Values are not intended to be simply descriptive of scientific activity on aggregate; they are seen as categories that scientists actually use in the decision making process. Thus, according to Kuhn, Copernicus responded to the value of accuracy when converting heliocentric astronomy from a global conceptual scheme to mathematical machinery for the prediction of planetary position[13]. Kuhn makes reference to scientists' interpretation of simplicity, etc., not those of philosophers or historians. In the empricial situation it will not be sufficient to say that certain choices appear to conform to particular values from an analyst's perspective; we must look for evidence that the participants themselves are drawing upon values.

In the analysis which follows, the data are drawn from periods of semi-formal talk at a scientific conference. This is just the sort of situation of direct theoretical debate that we ought to see participants identifying the values which, supposedly, guide and legitimate their theoretical commitments. For in this situation scientists are continually describing and justifying their acts of theory choice under the critical scrutiny of experts within their own field, who may respond by supporting, modifying or challenging the propriety of their choices.

To recap on the details of the conference (described more fully on pages 75-76); it was attended by psychologists who, for the most part, were from the United Kingdom. Just over 100 attended, drawn from various areas of the discipline, to discuss 'fundamental theoretical issues'. Roughly half of the conference time was allocated to the formal presentation of papers which had been previously circulated, and half to the discussion of papers and the issues which arose out of them. The general discussion periods were led by selected participants although in many cases topics were raised from the floor. The papers were exclusively orientated to theoretical or conceptual issues; no speaker used the conference to present original data. All the sessions were tape recorded and the entire discussion was transcribed verbatim.

The analysis that follows will concentrate on the value of testability. Although it is not one of the examples that Kuhn explicitly characterises in the 1973 paper, there are good reasons for concentrating attention specifically on this value. Not only is it strongly emphasised by other philosophers, such as Popper[14] and Quine[15], but also Crane[16] has suggested in a Kuhnian inspired analysis of particle physics that it is the value most fundamental to scientific activity. Popper, of course, takes it to be similarly fundamental. Furthermore, in the conference transcript references to testability, or to the related notions of falsifiability and refutability, occurred very frequently. On average one page in five of the transcript contained such references, which were produced by a total of 34 of the participants. Even using such a gross measure of occurrence as this it is clear that testability entered the discussion more than any other Kuhnian value. With so many accounts of testability it is obviously not possible to examine them all in a single chapter. Instead I will document some of the different forms in which testability accounts appear and the implications of this for Kuhn's model. No attempt will be made to separate the notions of testability and refutability. Throughout the transcript the participants tended to use these concepts interchangably, or to use refutability simply as a stronger form of testability. And as I have emphasised above, and in chapter one, it is the participants use of categories that is important.

Participants' Versions of Testability

In this section I wish to document the most common ways in which the conference participants characterise testability and the constraint it places on theory choice and scientific development[17]. A number of participants emphasise the central importance of testability. The following example is taken from the opening minutes of the very first discussion period, and may well have helped to plant testability firmly in the conference agenda. The speaker produces a linked set of complaints about a paper which has just been read by a psychologist who I will call Carlisle[18]. One of the complaints concerns whether Carlisle's position is testable:

1. if you are offering any explanation, you don't offer any means of testing your explanation - and of course that is the absolute, cardinal feature of scientific work, scientific explanation. And that's not just pointing to something that you say is important. It has the following critical value: that it allows us to make progress; we can discard theories which have proved useless (Norton, 3).

This is a very strong account of the role of testability. Norton implies that a theory which is not testable cannot be scientific. For testability is not merely important but is a central and necessary feature of science. It is only by testing explanations, and rejecting those that fail such tests, that scientific beliefs will progress.

In a further dispute between these two scientists, later in the same day, Carlisle himself gives a strong account of the centrality of testability:

2. anybody who, er, wants to propose any sort of theory in psychology, they have got to put that theory to the test. I clearly think that. Anybody would be utterly foolish to say here are some proposals but there are no consequences that follow from them (Carlisle, 72)

In this account the speaker suggests that the requirement that a theory be testable is obviously important, so much so that anybody would be utterly foolish to deny it. By implication, he includes himself within the category of 'sensible scientists who recognise the importance of testability' and, in this way, challenges Norton's assertion that his theory does not satisfy this criterion. He is thus able to characterise as misplaced Norton's suggestion that his theory might not be testable.

During the conference, numerous references were made to philosophers of science and the emphasis that they place on testability. Popper was most often mentioned, but Lakatos and Kuhn were both referred to on several occasions. In the next extract Popper is drawn upon to legitimate a general stress on the importance of testability in the evaluation of theories:

3. as Popper said, and I think quite rightly, the merit of something which is called a scientific theory, or a scientific hypothesis, is that it is clearly enough stated and clearly enough linked to empirical predictions so that we can go around finding out whether it is true or false (Hurst, 113).

In this quotation the property of being open to empirical test, through being clearly stated and making precise empirical predictions, is taken as a defining feature of scientific theories. It is strongly implied that if a theory is not testable it will not be scientific.

Popper is also used in the following extract, where the idea that scientists should attempt to refute their own theories is criticised.

4. I have never yet seen a scientist engaged in the refutation of his own theory. [] And I understood the Popperian idea not to ask for the action of refutation in the working scientist, but to be concerned with the object that the scientist has created being so formulated that it has the character of refutability. But you see the psychology of scientific work doesn't care two hoots about refutation; and it shouldn't. You would never get any places if you really, with your own ideas, set out to search how can I possibly be wrong. But <u>refutability in the formulation</u>; that's the major problem (Bowen, 274).

The speaker stresses that refutability is an important property of scientific theories, but that we misunderstand Popper if we maintain he is claiming scientists should refute their own theories. It is not that particular scientists should act in a certain fashion towards their theories, but that they should take care to formulate them in the proper, testable way. The speaker does not elaborate, but seems to suggest that by formulating theories such that they are testable other scientists may undermine them through testing experiments, or they might even be inadvertantly undermined through the continuous production of data.

These first four extracts put a strong emphasis on testability as an important or perhaps crucial feature of scientific theories. They show scientists themselves making use of the notion of testability in the evaluation of both scientific theories in general (3 and 4) and specific theories (1 and 2). There is some disagreement over whether a particular theory should be seen as testable: Carlisle claims that his own theory is testable while Norton disagrees. Yet Kuhn suggests exactly that two scientists sharing the same value may interpret its use differently. Thus Carlisle and Norton's disagreement is in no way opposed to Kuhn's model. However, these extracts seem to suggest an even stronger role for testability than might be implied from this model of theory choice. Kuhn claims that values work together to make theory choice progressive. Yet these speakers seem to imply that this feature <u>alone</u> should be criterial in theory selection; i.e. that <u>whatever</u> its other features no untestable theory should be selected. From these accounts, then, we might suppose that testability is not a Kuhnian value at all, but that it acts more like a straightforward selection criterion. Thus whatever their simplicity, scope, etc., all acceptable theories must be testable.

Other extracts from the conference transcript present a very different picture of the significance and function of testability. The following accounts suggest that testability should not necessarily be a central aspect of scientific theories. Just as philosophers, particularly Popper, are drawn upon to justify an emphasis on testability, so they are drawn on to undermine the importance of testability. For example, in the following passage the speaker characterises philosophers' as rejecting the notion that testability is essential for scientific progress:

5. it seems to me that a considerable body of informed opinion in philosophy of science, now, which would argue that there is a great deal, that there are a great many other ways in which scientific theories and so forth (understand[ing]) <u>progress</u>; <u>other</u> than by falsifying particular hypotheses (Rugg, 88).

In contrast to Norton's earlier claim that testability is vital to ensure scientific progress, the speaker suggests (without specifying any of them) that there are a great many other ways in which scientific understanding progresses. The implication seems to be that progress through repeated exclusion of hypotheses which are found wanting is just one of a number of means by which science can develop.

A stronger conclusion is drawn in the next extract.

6. [Although psychologists have absorbed a little bit of philosophy] they obviously havn't heard about sophisticated falsificationism. And this - I am not going to go into this now - but it can really be summed up by the statement which I think is generally agreed by all philosophers now; and that is that it is not just that scientists, or certain scientists, don't go about refuting their theories, but the simple statement that no theory was, is, or ever will be actually refuted by the facts (Black, 275).

The speaker draws a contrast between those 'little bits of philsophy' that psychologists have absorbed and what is 'generally agreed' by 'modern' philosophers. It is implied that talk of scientists' refuting theories originates in these out-dated and superficial philosophical ideas that psychologists have adopted. However, what all philosophers now agree is that theories are not refuted by the facts at all: a claim the speaker specifies as a summary of 'sophisticated falsificationism'.

Participants at this conference thus use versions of philsophers' claims and arguments to support both an emphasis on testability (extract 3 and 4) and a view that theories are not straightforwardly open to testing (extracts 5 and 6). Furthermore, despite these opposing claims, the speaker in extract 6 suggests that there is agreement amongst philosophers. We can perhaps view this claim of philosophical consensus as an interpretative resource for legitimating the position on testability he espouses; if all philosophers are agreed, then what they agree on must be correct.

In other parts of the transcript, more explicitly social accounts of theory selection are offered. For example, the following passage draws on the social studies of science literature.

7. If logic plays no part in the matter, if in fact we haven't managed to refute anything yet; and yet there are a large number of theories - I put the word in inverted commas - that have been proposed and with which we no longerhave any truck; if logic has played no part in that, then what has? It seems to me that what's happened is some combination of individual and

> social matters, um, as Ziman said: science is public knowledge, that science takes place in a social context, and is, in the last analysis, a consensual matter (Inness, 279-280).

Here the speaker starts by suggesting that the progress of science poses a problem: if theories have not been abandoned as a result of the 'logical' procedures of refutation, why is it that, nonetheless, many theories are no longer supported? The answer, he suggests, is that it is not refutation at all but social processes and individual psychologies which lead scientists to abandon theories. The refutability of theories is thus seen as unimportant, it 'plays no part in the matter'. For the real processes of development are social

A further social account of testability or refutability proposes an almost complete dislocation between theory and data. Because of the way data are open to reinterpretation, a theory can be sustained by continually recategorising any data which seem to undermine it.

8. And I would want to make the argument that most theories are, as an abstract system for which there are no obvious exemplars. [] We have sets of constructions - theoretical constructions, multiple constructions - for which we can point to no obvious exemplar. And we can negotiate the meaning of any particular observation in virtually any direction. So that, for example, we talk about aggression, and yet it's not clear when we ever have an instance of aggression. [] Virtually any activity, I suspect, that I engage in any day, could be looked at as an instance of aggression in some form. It is aggression or it is not aggression. And it depends on the set of social agreements of what things are called. And those agreements, it seems to me, are negotiable over time, and they can be disagreed upon and writhed round in any case. So that, in effect, any theory can be sustained so long as you have a capable negotiator of reality (Leary, 278-279).

In this passage a radically social view of the testing process is proposed. Instead of theories being undermined by recalcitrant observations it is suggested that the meaning of any observation is dependent on a set of social conventions and, furthermore, that those conventions can be reinterpreted in very different ways. In fact the possibilities of reinterpretation are so great that a 'capable negotiator of reality' can continue to reinterpret observations indefinitely so that they fall into line with any particular theory. The value of testability thus becomes irrelevant from this perspective, because what is seen as a test will depend entirely on the negotiated conventions for interpreting observations.

These last extracts (5-8) present a picture of testability and theory choice markedly different from that documented in extracts 1-4. Instead of testability being described as a necessary requirement for progressive theory choice, its significance for scientific development is radically downgraded. In extracts 5 and 6 the speakers draw upon recent philosophical thinking to suggest that refutability is unimportant or even irrelevant; its role is certainly not seen to be crucial in guiding scientific progress. Extract 5 seems to characterise testability in a way much closer to the Kuhnian value than the earlier extracts. Progress <u>may</u> result from the testing and eliminating of theories, but it may also result in many other ways. These are not specified by the speaker, so we cannot be sure that reference would be made to other values. However, taking testability as just one of a number of possible considerations seems quite close to Kuhn's position. Extract 6 implies that refutability ought to be abandoned as a criterion altogether, although again the speaker does not exclude the possibility that it should be replaced by further criteria.

In extracts 7 and 8 more social accounts of theory rejection are offered. These also suggest that the process of testing plays little or no part in scientific development. Theories are rejected because they do not coincide with the consensus of scientific opinion. Moreover, testability cannot constrain development because with skillful interpretation of data any theory can be sustained. In these accounts the basis of selection is seen to lie in the social organisation and action of scientists rather than in criteria such as testability.

It seems, then, that we can document the existence of different versions of the constraining role of testability in the talk of different participants. One version approximates to the model that Kuhn suggests. Yet others, as we have seen, treat testability as a rigid criterion or, alternatively, suggest it plays little part in theory selection. Most of the accounts discussed above deal with the general significance of testability in theory choice and scientific progress (3-8). This raises the possibility that these psychologists are merely reiterating standard philosophical positions about science. For there has, of course, been considerable philosophical dispute over the question of the refutation of theories[19]. This leads to the question of whether a more straightforward, consensual picture would emerge in the discussion of the testability of specific theories. That is, do the psychologists utilise a broad quasi-philosophical discourse for arguing about these theories in general terms, but approach specific scientific positions with more clearcut expectations.

Disputing the Testability of Specific Theories

In the 7 extracts that follow a number of psychologists argue about the testability of specific theories. In the first passage Norton notes that Carlisle has written approvingly about a particular theory, taken from the Russian psychologist Vygotsky, concerned with the development of consciousness. He asks Carlisle how he would decide whether this theory or a particular opposing theory is the correct one.

9. Vygotsky also, if I could make a guess, didn't suggest an experimental or other way by which one could distinguish between that view and any other view. I am still trying to get you to tell me - you admit that there is no point in proposing a theory if you cannot test it; no point, you said, in just standing up and making proposals unless consequences follow [see extract 2]. I want to know how you would actually rule one of these points of view out. They are diametrically opposed hypotheses. They can't both be right (Norton, 73).

However, it is Thomas, not Carlisle, who responds to Norton.

10. Thomas. But there are such things as coherence theories of truth as well as correspondence theories.

    Norton. Then how would you apply it in this or any comparable instance?

    Thomas. Well, it seems to me that, I may be doing Vygotsky a gross injustice, but his view has the advantage for him of being coherent with Marxism. Um, now my view has the advantage, for me, of being coherent with a body of findings of experimental psychology which enables me to provide what seems to me to be a coherent account of a range of phenomena. And I would hope to convince Carlisle in due course, or he might hope to convince me on the same ground, that one view or the other was preferable because it enabled us, if we were prepared to agree in adopting that view, it permitted a coherent set of views, covering a range of phenomena, which was larger, or preferable for some other reason, from the one which followed from the view at present adopted (73-74).

In this extract Thomas, instead of proposing a specific way of testing Vygotsky's theory as Norton asks, proposes an alternative view of the process of theory selection. Instead of a theory being selected because it survives a test, or being rejected for failing one, theories may be selected because they are coherent with various other views that are held. The theory which allows most coherence, and deals with the largest range of phenomena, should be accepted. Of course, to make coherent sense of a body of findings might itself be seen as a test and to lay the theory open to refutation, because an inconsistency may be discovered in some part of the findings. However, this is not how Thomas is using the notion of coherence. In this context he takes coherence specifically to oppose the emphasis that Norton places on testing. He thus relegates the notion of testability to being just one of two possible criteria that can be used.

In the next extract, which follows directly from the last, the idea of a coherence theory of truth is taken up and used to introduce a further disagreement with Norton's claims.

11. Well, yes. I would like to support that. And I think I would like to accuse Norton of some inconsistency on this point. Because several times today he has quoted us the example of the theory of natural selection as, above all, the sort of theory that we ought to aspire to in psychology. But, notoriously, all this Popper business doesn't hold with the theory of natural selection. If somebody doubts it you can't drag them along and show them a critical experiment. You simply have to jawbone with them for hours. And if at the end of the day they don't agree with you you just have to conclude that they are some sort of fool [laughter]. That's pretty much the only way to do it. I suggest that's what Carlisle's trying to do with you [much laughter](74).

The speaker suggests that Norton is being inconsistent, because the criterion he wants to apply to Carlisle's theory would not be satisfied by one of the theories he himself espouses, namely evolutionary theory. He introduces the passage by expressing support for what Thomas has said in in extract 11. His point, however, is rather different. Although Thomas includes social elements in his exposition of the 'coherence theory', the coherence is seen to be the property of a range of phenomena; that is, the scientist is introduced as an agent, but his role is taken to be mainly passive. In extract 11, on the other hand, a much stronger social account is given. There are no critical experiments that can demonstrate the truth of evolutionary theory. Scientists come to accept it through social processes of persuasion. They argue with each other about the value of their theories and no mechanical decision procedure is available for deciding their truth and falsity.

Norton replies to these claims about evolutionary theory with an apparently sarcastic characterisation of the idea of a coherence theory.

12. Perhaps I could tell Quest why I think the theory of evolution is not the same kind ofthing as whether or not you agree with Marxism. The difference is this. It is perfectly correct that you cannot put the theory of natural selection directly to experimental test. It is a set, it provides you with a set of axioms which you must take as the starting point for theory construction. But if that's all the theory of evolution had been we would not have taken it seriously. What has happened, of course, is that within the time that Darwin proposed the theory of natural selection a very large number of <u>specific</u> theories, and hypotheses derived from those theories, have been developed and <u>put</u> to experimental test. There are in fact a very large number of experimental tests of theories constructed with the axioms of natural selection in the background. And had any of those individual theories come up with hypotheses which had clearly proved to be falsified then we would not accord the theory of evolution the respect that we do in fact accord it today (74-75).

Norton here draws a powerful picture of evolutionary theory supported by a large number of successful experimental tests. He suggests that Quest is right (extract 11) that the theory cannot be put <u>directly</u> to the test. However, he claims that because of its axiomatic structure it is indirectly testable: hypotheses are derived from theories, which are in turn derived from the axioms of natural selection. If any of these hypotheses had been refuted, then less respect would accrue to the axioms of the theory. Norton thus characterises evolutionary theory using classical logical empiricist categories and thereby maintains the notion that evolutionary theoryis accepted due to its satisfying some mechanical decision procedure, rather than because of its coherence or persuasiveness.

Thomas and Quest respond to this description of evolutionary theory in significantly different ways. Thomas suggests that the picture is not so clear cut as Norton would have it. There are researchers who disagree with the theory and have data to support their disagreement.

13. Can I say, surely the theory of evolution is also controversial. There are people who don't accept natural selection and advance counter evidence (75).

This account is used to undermine the claim, implied in Norton's account (extract 12), that it is consensually agreed that evolutionary theory has survived the testing process. unscathed.

Norton's reply is to claim that all biologists accept evolutionary theory.

14. To my knowledge there are no biologists who do that [do not accept natural selection] but there are others (75).

In this extract Norton can be seen to draw on a similar device to that used in extract 6. A position is made more credible by characterising it as one espoused by all competent or expert members of a field. In this case, therefore, there may indeed be some scientists who do not accept natural selection, but those who are properly qualified to make assessments of the evidence (biologists) are all agreed on its correctness. Norton thus uses a social account of scientific belief to undermine the suggestion that evolutionary theory is controversial[20].

Quest responds to Norton's description of natural selection (extract 12) by asking for an example of a hypothesis derived from evolutionary theory which has survived attempts to falsify it. In reply Norton mentions that there have been 'innumerable behavioural selection experiments' (75). However, Quest in the following passage, undermines the relationship between these sorts of experiments and evolutionary theory.

15. But the counter argument to that, as you will know, is that the sort of mutations that have cropped up in the laboratory situations, which allow the possibility of selection experiments, have virtually nothing to do with the kinds that have been postulated to be advantageous and have led to the progress of evolution that has actually occurred. Um, I mean they have to do with the change in colour of the flies

> of drosophila and they have to do with various cytogenic things. But none of the millions of gradual improvements in the optical structure of the eye which have to be postulated have ever been observed, still less selected for. Unless there should be any misunderstanding, of course, I believe the official doctrine [much laughter]. But I can't for one moment admit that there is any experimental evidence that argues in favour of it (Quest, 75-76).

Quest describes the experimental evidence that Norton mentions as irrelevant to the theory; it is only to do with trivial mutations and certain delimited changes that are amenable to laboratory control. In contrast, Quest claims that important changes that the theory does predict, such as changes in the structure of the eye, have never been observed. Nevertheless, he does not argue that the theory should be rejected. Instead he describes his belief in the theory as 'entirely due' to its coherence with his other beliefs. He thus implies that the more complex model of testing that Norton produces (extract 12) is a fiction, because the sorts of specific hypotheses through which he claims evolutionary theory as a whole may be tested have no bearing on the central tenets of the theory. He re-emphasises the more social account of the adoption of the theory.

In the course of the interaction (extracts 9-15) the participants dispute the role which the criterion of testability plays in the adoption of specific theories. These accounts display a variability in characterising testability which is very similar to that documented in the previous section. Norton asserts the crucial importance of testability. He views it as an essential property of any scientific theory and the process of testing is taken to provide an unambiguous criterion for theory selection. The reason why evolutionary theory is accepted, then, is its survival of empirical tests. On the other hand, Norton criticises Vygotsky's theory and Carlisle's theory for being untestable; such theories should not be adopted by scientists.

It seems therefore, that Norton takes a stronger view of the role of criteria than Kuhn, much like that taken in extracts 1-4. Kuhn argues that shared criteria for selection exert a powerful influence on scientific judgments, and indeed enable progressive theory choices to be made. However, he does not want to claim, as Norton seems to, that criteria determine theory choice. Instead he emphasises the problems scientists have in interpreting values and weighing their relative importance for any particular choice. This appears closer to the view of theory selection proferred by Thomas (extract 11), who suggests that although theories can be selected because they are testable, they may also be selected because they are coherent with a large body of findings. The notion of coherence is used here in very much the way that Kuhn uses scientific values, as one of a number of positive features that theories may possess. Vygotsky's theory and Carlisle's theory may be adopted because of their consistency with other bodies of data. Their (possible) lack of testability should not lead to their immediate rejection.

Although Quest's account of the adoption of evolutionary theory in extract 16 is similar to Thomas's in its stress on coherence, in extract 11 he produces an account which places more emphasis on social processes. In this extract he makes no reference to criteria or values for theory selection. In fact he produces no formalised account of the selection process at all. Emphasis is instead placed rather vaguely on social processes of discussion and persuasion; although to what degree this process is seen as rational is not made clear. Nevertheless, this view appears rather different from Kuhn's stress on the role of a few broad values. As with the previous section, therefore, some of these accounts appear to be similar to Kuhn's, while others propose either a stronger or a weaker role for testability. Moreover, this variability represents more than merely inconsequential reiteration of philosophers' perspectives on testability. For it shows that these psychologists are using different versions of testability to make detailed evaluations of specific theories, not just in their general discourse concerning theory choice.

It is worth noting finally in this section that these extracts present a further problem for Kuhn's model of theory selection. For they show that judgments of testability may be based on additional technical evaluations of scientific fields and that these technical evaluations may be highly contentious. Thus Quest uses a distinction between the limited findings of laboratory experiments and predictions which are central to evolutionary theory in order to undermine the close positive relationship that Norton claims between the theory and experimental findings. The tests that Norton sees evolutionary theory as having survived are only taken to be tests from a particular interpretation of the theory, its predictions and the meaning of the experimental findings[21]. Yet Kuhn argues that one of the things that supporters of rival theories can display to each other (not always easily) is exactly their concrete technical results[22]. This seems to be a modification of his earlier view which placed considerable emphasis on the way observations and findings are understood in the context of specific theoretical and conceptual frameworks[23]. And these present findings seem more in line with this earlier view.

Testability, Variability

In this section I wish to show that the criterion of testability is not only used in highly variable ways by different speakers, but that there is also considerable variability within particular speaker's accounts. I will illustrate this point by examining individual scientist's accounts of the testability of a number of specific theories. In the following passage the speaker accounts for the role of testability in the selection of his own theory and those of other speakers at the conference.

16. I would like to return to this game of I am more refutable than you. I said refutable, not reputable, although you can be forgiven for misunderstanding that. [laughter] Um, I don't believe that refutability is the only criterion that we should be judging our models by but, and it is particularly difficult to use it very systematically because we actually don't understand the nature of refutation at all well. Nevertheless, I think it is perfectly reasonable that people should have made such a big thing of it earlier on. [] Um, they all, everyone pretty well has paid lip service to refutation. It is interesting to notice the strat., the different strategies that people, er, have used. Um, since Norton spoke such a lot it is a pity he is not here. It's, I would like to draw attention to his, which is very clearly to say 'leave me alone, I am doing very well in my own small corner', you know, 'go and die in your own'. [laughter] Leave me alone I, although he says that everybody has a duty

> to be an intellectual imperialist, he is not trying to produce, um, larger, more ambitious models which are, of course, much easier to, to, er, refute, er, than the less ambig., ambitious ones. [] Now the good guys are the ones who are leaving hostages to fortune, who really are laying themselves open to being knocked down in all possible ways. The good guys are the people who are putting up the biggest possible theory that they can imagine. And, um, hoping that, um, it won't be knocked down too quickly. Among these people I would put Squire, um, Chester - I apologise if I have missed anybody else out, I can't remember if all the things that have happened in the, in this meeting. But I can, of course, remember what I said, and I am one of the good guys [laughter] (Young, 272-272).

In this passage the speaker, Young, fashions an account ofhis own and other scientists' theories in terms of whether they can be tested, presumably by means of experimental evidence. The passage starts by characterising the evaluation of testability as a game in which the goal is to establish that the player's own theory is refutable, and therefore reputable. He then claims that some earlier speakers used a variety of strategies to reduce the possibility that their theories will be undermined through testing. He singles out Norton for particular comment. As Norton was not present Young could provide an account of his work without fear of contradiction by the person being criticised[24].

Young criticises Norton for using double standards: on the one hand he advocates an intellectual imperialism, yet at the same time he researches a very limited area of psychology, using small-scale, unambitious theories. These sorts of theories are not, Young claims, so open to refutation as more ambitious models. Although Young stresses that Norton is using testability in a strategic fashion, and that testability is not well understood, he does not maintain that it is irrelevant as a constraint on psychologists' acts of theory choice. He states that it is quite reasonable that people should have emphasised it earlier in the conference. This enables him to use the notion of testability himself, later in the passage, to establish that his own and some similar positions are 'reputable'; that they are the 'good guys' and, by implication, that their professional actions follow from proper conformity to the rules governing scientific theory choice.

This extract can thus be seen to draw upon two rather different versions of theory choice, similar to the different versions of theory choice I documented earlier in different speakers' accounts. The first, which Young uses to characterise his opponent's theory, I will call the contingent version, using the term contingent as in the previous chapter. The criterion of testability using this version is said to be used 'strategically'; it is depicted as being inconsistently invoked in support of Norton's theory at the expense of others'. In contrast, Young gives a much more empiricist, asocial account of the adoption of his own theory. In this case the criterion is not presented as open to strategic interpretations, but as an effective constraint on the speaker's actions. The action of 'refutation' is removed from the speaker's sphere of social control; all he can do is 'hope' that his theory will not be undermined by the evidence. Young presents himself, unlike Norton, as unable to influence whether or not his theory will be found wanting. As a result of Young's interpretative work the criterion of testability appears to ensure that his theory is a 'hostage to fortune' and that it will be rejected or retained by virtue of the data alone[25].

These sorts of interpretative asymmetries are a recurrent feature of psychologists' accounting for the choice of their own and others' theories. Let us examine some other examples to further illustrate this claim. We can see this type of asymmetry in Norton's accounts quoted earlier in the paper. For instance, in extract 11 he gives an account of evolutionary theory, which he has been espousing as an exemplary model for psychology, as having survived thorough attempts to test it, none ofwhich have showed it to be wrong or in need of revision. Furthermore, evolutionary theory is depicted as testable through the clearcut consistency of experimental data with specific, formalised hypotheses. Reference is thus made to a determinate evaluation procedure which constrains individual scientists to endorse particular beliefs about the natural world. In contrast, he criticises Carlisle's theory for being untestable in both extracts 1 and 8. Carlisle, Norton claims, provides no clearcut procedure for testing his theory and therefore it ought not to be adopted. The pattern here is thus the same as that revealed through analysis of extract 16; Norton's own theory is described as having the virtue of testability, while an opponent's is seen as untestable.

We saw in extract 2 Carlisle give a general account which emphasised the importance of testing theories. Indeed, he stresses that any scientist who ignored the requirement that a theory be testable would be 'utterly foolish' and he implies that he is certainly not one of these scientists. In the following passage, which is directed at Norton, Carlisle gives an account which undermines the testability of Norton's position.

17. I mean [] that 'mechanism' is also a metaphysical system within psychology, such that there are no <u>facts</u> so to speak which can <u>prove</u> the mechanist approach wrong, just as there are no facts which can prove the Kelly approach wrong - as you want to say (Carlisle, 144).

Carlisle suggests that the approach which Norton has been espousing is not open to refutation by the facts. It is not a directly empirical approach but a metaphysical system. Its selection could not, therefore, be adjudicated by direct reference to empricial events.

So far, then, we have documented three scientists' use of asymmetrical accounting. Each variably characterises the role of testability; it is a central determinant in the choice of the speaker's own theory, while being unimportant or irrelevant in the choices of certain other scientists. The speaker's own theory or approach may be undermined by the facts, while no such clearcut process may happen with other's theories and approaches. Moreover, it is clear that these accounts cannot all be right; i.e. they cannot all be accepted as straightforward descriptions of the testability of different theories. For there are significant disagreements between them. We can see that Carlisle's theory is viewed as testable by Carlisle but not by Norton; while Norton's theory is viewed as testable by Norton, but not by Young. Furthermore, although Carlisle stresses that it is the <u>broad approach</u> which Norton takes which is not testable, Young argues that Norton's theory is too <u>small scale</u> to be fully testable. Each of these scientists, then, has variably characterised the role of testability to present radically conflicting representations of the nature of theories within their scientific field.

In the accounts which we have examined so far in this section, an asymmetrical pattern of accounting is regularly used to depict each speaker's own espoused theory as more testable than some other theory. In the next passage, however, the speaker gives an account which stresses that his own theory is actually <u>less</u> testable, less open to refutation, than various other theories. Nevertheless, the account is organised in such a way that it provides legitimation for the speaker's theory.

18. Could I just say a quick word about falsification, because I think it is really a rather important topic. And I think part of the troubles that arise in trying to get a handle on it, as a practical working scientist, is that it is quite different, it has quite different character depending upon the type of theory that you are operating with. If you are operating with a theory which has a well established deductive structure then the, er, the refutatory process is relatively speaking logical; i.e. you use modus tollens to show that a false consequence tells you that some element in the deductive structure is, is false. [] Now that's OK, and that's I think the structure of what we have been calling mini-theories, i.e. theories which are sufficiently small-scale to be articulatable in that deductive form. They are really rather rare in the natural sciences. And I dare say they are just as rare in the psychological sciences. But I think the case that's much more interesting is the sort of theorising at the level of which I was talking of this morning: molecular theory of gasses; evolutionary theory; [Squire's own theory]; those kind of theories which contain the double analogy structure. [] And it may be that a really powerful theory contains within itself enough, as it were, potential material, as indeed the molecular theory of gases did, to go on with, through a great deal of traditional refutatory procedures and still survive as the theory in the field. And Darwinian theory has also been through the same kind of game. Now the theory I was trying to outline this morning has just that character; i.e. it is a theory for providing a conceptual system, but it does have - although Young, of course [see extract 16] sees it as having pseudopodia which put out a fair way into the, into the refutable world - at least I would also claim for it that it has a certain measure of elasticity and that <u>it is not going to be too darn easy to refute</u> (Squire, 273-275).

In this passage the speaker makes a distinction between two broad form of theory which are testable in very different ways. One of these has a deductive structure, and thus its refutation is based on fairly simple logical procedures; a refuted prediction indicates that there is an error somewhere in the deductive structure. The speaker equates these kinds of theory with the 'small-scale' theories that have been discussed by Young (extract 16) - that is, he equates them with theories such as that espoused by Norton. The other form of theory is depicted as having a very different structure which is based on analogies rather than formal logical deductions. It is thus not open to refutation in the same way. Indeed, Squire emphasises that theories of this kind have a measure of elasticity which allows them to <u>resist</u> traditional procedures of refutation and to remain the central theory in a particular field.

Squire fashions a wideranging and historically based account of the nature of theories in science to display lack of testability as a <u>positive</u> feature of certain theories. He does this by characterising the class of theories which are not straightforwardly refutable as including classic theories from the natural sciences, such as the molecular theory of gasses and evolutionary theory. The charge that all theories ought to be refutable is thereby undermined. For it is made equivalent to the very implausible charge that certain classic and profound theories are not properly scientific. And the charge is further undermined by the suggestion that those theories which do have a deductive, refutable structure are rather rare. Moreover, by characterising his own theory as being based on a structure of analogies rather than deductions, Squire equates it with those theories which are usually seen as the very pinacle of scientific achievement and, at the same time, indicates that its lack of refutability is an asset rather than a fault.

Overall , then, this account is like Young's (extract 16) in that it embodies two very different images of the way the criterion of testability functions in theory selection. On the one hand there is refutation, that is, a clearcut, logical process which effectively constrains any theory choice. While on the other, there is a much more flexible process of theory testing in which empirical findings are not crucial for selecting or abandoning theories. However, Squire uses these two versions in a way which is unique in the transcript and almost the exact opposite of the way Young uses them. Thus Squire characterises Norton's theory as open to clear testing, while his own theory takes a form which actually resists empirical refutation. Young claims the reverse. He characterises Norton's theory as not open to empirical testing and refutation, and Squire's theory as much more exposed to refutation. In extract 18, Squire comes close to drawing attention to this conflict between his and Young's accounts, when he explicitly formulates Young's view that his theory is clearly refutable. Yet he draws away from endorsing Young's stronger version of testability (which is already somewhat attenuated in Squire's broad gloss on Young's point - extract 18) because it would contradict Squire's own warrant for his theory.

Despite the fact that Squire in this passage goes to considerable lengths to show that lack of openness to empirical testing is a positive attribute of his own theory, only a few minutes later he criticises another theory for exhibiting exactly this feature:

19. the survival by a thousand qualifications ah, is, er is what goes on. And certainly cognitive dissonance theory is a beautiful example of that. The more the theory got refuted the more distinctions that were made. Now of course that is a technique which you can use. But it has, as it were, nothing to do with the logic of the case. And one would then query the motives of the people engaged in the [inaudible] (Squire, 277).

In this passage Squire accounts for the lack of falsifiability of a certain theory in the same way that Young accounts for the lack of falsifiability of Norton's theory. Thus retaining support of 'cognitive dissonance theory' in the face of contradictory evidence is characterised as a technique used by scientists whose motives are open to question. When his own theory flexibly avoids refutation that is a good thing; it is only doing what great theories have done in the past. But when a particular theory which Squire opposes is treated in this way, Squire suggests that this is not something rational but to be explained by reference to the psychological makeup of certain scientists.

Discussion: Testability as a Flexible Symbolic Resource

I will now look more closely at the implications of this analysis for Kuhn's model and the social study of scientific theory choice. In the first section we saw that although certain participants gave an account of the constraining role of testability very similar to Kuhn's, allowing it as just one of a number of important attributes of theories, others gave rather different accounts of testability. On the one hand, some of the scientists implied that testability acts like a determinate criterion for theory selection and that no untestable theory should be adopted, regardless of any other virtues that it might have. On the other, some participants' accounts stressed the central role of social phenomena, such as scientists' consensual belief systems and their interpretative skills, in theory selection, and suggested that the criterion of testability would therefore be unimportant or irrelevant.

This variability in accounting was repeated in the second section of analysis, where scientists disputed the role of testability in the selection of specific theories. Again, certain participants' accounts seemed to depict testability as a Kuhnian value. In these instances testability is characterised as one possible condition for selection, but not a necessary condition. For example, Vygotsky's theory may be selected because of its coherence with a body of data rather than because it is necessarily testable[26]. For other participants, however, testability is considered to be a necessary condition for selection. Thus evolutionary theory is said to be adopted specifically because it is testable and has been able to withstand rigorous empirical testing; and it would not have been adopted if this had not been the case. While Vygotsky's theory is seen to be untestable, and therefore it ought not to be adopted. Further participants, in contrast, emphasised the centrality of unformalisable argument and social processes in theory selection. They suggest, for instance, that evolutionary theory is not widely supported because of its ability to withstand empirical tests but because scientists have been persuaded of its correctness. Here no explicit criteria are offered for the choice; but continued discussion and dispute are seen to be crucial in the process. I have thus documented variability between different scientists in both their evaluations of the general importance of testability and its significance for choosing particular theories.

What are we to make of this variability? As I have noted at the start of this chapter, Kuhn accepts that there will be a degree of variability between individual scientists in their emphasis on different values and their meaning. He also suggests that the significance placed on particular values will change when new theories are adopted; perhaps a new theory with broad scope will be chosen and then more emphasis will be subsequently placed on the value of scope. This, of course, opens up the possibility of circularity: scientists adopt a new theory, values change to correspond to features of that theory, and then the adoption of the theory is subsequently explained by reference to the very constellation of values it produced. However, Kuhn claims that any circularity will not be vicious, because the 'magnitude' of value change is regularly smaller than that of theory choice[27]. Yet this claims seems to presuppose exactly what Kuhn makes problematic, namely an understanding of the specific processes by which values constrain theory choice. For it is only if we have this sort of understanding that we can judge the magnitude of value change to be insufficient to account for theory choice. Kuhn nowhere produces such a specific account of the practical effectiveness of scientific values.

It is difficult, therefore, to see how we can judge the implications for Kuhn's model of the variability between scientists' accounts of testability I have documented above. Variability is to be predicted by Kuhn's model; yet if variability is too great values will cease to be useful in explaining theory choice. Or, put another way, Kuhn wants to use the functioning of values to explain scientific progress (i.e. values through their constraint on choice ensure progress) and at the same time maintain that they do not determine theory choice. However, this raises the question: at exactly what level does variability become so great that theory choice ceases to be progressive?

Of course, one of the central difficulties in elucidating this question is that it involves clarifying what counts as scientific progress. In his discussion of scientific values Kuhn treats progress as relatively unproblematic. Indeed, his paper is organised to show how the 'facts of progress' may be explained by reference to scientific values; while the assumption that progress is a fact goes unexamined[28]. It seems, therefore, that Kuhn's model does not provide the resources for its own evaluation. It is not specific enough to enable us to determine the degree of variability in the interpretation of values at which they will cease to act as a constraint.

In these circumstances, perhaps the best that can be hoped for is a more negative critique. Because Kuhn's model does not specify the degree of variability between scientists which will ensure progressive theory choice, and because such a specification depends on the further specification of such imponderables as what counts as scientific progress in a scientific field[29], I cannot clearly demonstrate that the variability documented is too great to allow progress. It is only possible to make the weaker claim; that there is no apparent way in which such extreme variability in values (from testability as a necessary condition to a total irrelevance) can effectively and productively guide scientific development. Of course, a mechanism which is driven by such highly variable values might be formulated, or the model might be rescued by claiming that there is a true consensus over values, but it is not apparent at scientific conferences. Nevertheless, no resources for a mechanism of this sort are apparent in Kuhn's model as it stands, nor does it indicate where a truer consensus might be found.

In the third section of analysis I documented the variability within individual scientists' accounts of the role of testability in theory choice. I described two broad perspectives on testability which are used in these accounts. On the one hand, testability is characterised as an effective constraint on choice which is not significantly influenced by social processes. It serves purely to select those theories which may be related to bodies of data in a clean cut fashion. This can be called the empiricist version of testability. On the other hand, testability is described as ineffective in constraining scientific activity. For it is open to different interpretations which may be strategically made by scientists to give a spurious aura of legitimation to their theories. This I have called the contingent version of testability. Furthermore, I noted that there is a regular pattern in the use of these different versions. Apart from one interesting exception (which I will discuss shortly) the empiricist version is only used by participants to characterise their <u>own</u> theories. In contrast, the contingent version is generally used in the description of theories which the speaker does <u>not</u> support or are supported by scientists the speaker disagrees with.

Before we can go on to properly examine the implications this sort of variability has for Kuhn's model, we must look in more detail at the relationship he advances between values and acts of theory choice. The major source of variability in criteria that Kuhn identifies is <u>between</u> different scientists. He suggests three origins of this variability[30]. Firstly, it may result from each participant's unique experience as a scientist; being particularly successful with a certain sort of theory, say. Secondly, extra-scientific belief systems may encourage scientists to accept certain sorts of theory; for instance, Victorian notions of population selection influencing the adoption of Darwin's theory. Thirdly, scientists' individual personalities may lead them to make certain sorts of choices; for example, encouraging a scientist to choose a more narrow, conservative theory.

Kuhn's identification of the main sources of variability in this way allows a further explication of the relationship he proposes between values and scientists' acts of choice. Kuhn implies that scientists internalise a particular version or constellation of scientific values through their scientific and broader social experience which is, in turn, mediated via their specific personalities. In Kuhn's terms, these features of each scientist's socialisation lead to the emergence of individual interpretations of the criteria for theory choice which are generally available. Each scientist will thus take a value such as testability to have a particular importance for choosing theories and will understand the meaning of testability slightly differently from other scientists. Consequently, an historian or sociologist of science who understands both these shared criteria and the idiosyncratic ways in which individual scientists are socialised into them can properly explain why 'particular men made particular choices at a particular time'[31].

There are, however, some important difficulties with this. The central problem is that, when Kuhn deals with the relationship between scientists' individual interpretations of criteria and their acts of choice, he treats this relationship as determinate; i.e. he assumes that the particular constellation of values held by a particular scientist will determine choice. Although he emphasises that the criteria do not provide a <u>shared</u> algorithm of theory choice, he seems to accept that each individual's acts of theory choice can be, in principle, unequivocally derived from that individual's particular interpretations. His argument, then, is not that shared criteria do not determine general scientific choices; that there is a generalised indeterminacy between criteria and choice. Rather it is because individual criteria are <u>different</u> for each scientist that what is shared is insufficient to fully predict the theory which will eventually be adopted.

However, in treating individual scientist's criteria in this way, as enduring, psychologically encoded values which actually determine their actions, he ignores important arguments and evidence that demonstrate the great flexibility of criteria when interpreted in practical contexts[32]. Moreover, although flexibility is a pervasive feature of all natural language use[33] it is particularly evident in the case of broad, generalised criteria such as testability, scope, etc.. Because Kuhn ignores the possibility of variability within an individual scientist's interpretations of criteria 'at one point in time'[34] he fails to consider the possibility that the <u>same</u> scientist, using the <u>same</u> value, will interpret it differently on different occasions. It seems, therefore, that Kuhn's model cannot account for the sorts of variability I have documented within the talk of some individual scientists. This raises the issue of what can account for this variability, and leads on to a further difficulty with the model.

In treating values as psychologically encoded templates for generating action which are socially conditioned into each individual scientist, Kuhn directs attention away from an examination of what is achieved by scientists' talk of theory choice. This might not matter if Kuhn were concerned only with philosophical rationalisations of theory choices. However, as I have noted, Kuhn makes it clear that he is interested in the actual categories used in choosing theories; i.e. he wants to produce an historian's account of the way science actually progresses, not an a posteriori justification of that progress. Thus he is faced with the social analyst's task of identifying the values held by particular scientists and elucidating their actual use. He cannot, of course, infer these values by looking at the properties of theories which have been adopted. For such a procedure would inevitably lead in a vicious circle. In fact Kuhn avoids facing up to this problem by not making the analytic basis of his model explicit. Nevertheless, if he is going to avoid circularity, at some point his model must be based on evidence of the kinds of criteria offered and arguments proposed by scientists themselves when deciding between opposing theories; in short, on their discourse concerning theory choice.

Following the approach developed throughout this thesis, the variability within individuals' accounts can be taken as an analytic point of departure. If the contents of accounts on a given topic are highly variable, it seems reasonable to explore the idea that accounts are not literal descriptions of participants' given actions nor expressions of their given beliefs, but rather flexible interpretations devised in accordance with the requirements of social situations which are themselves constantly changing[35]. That is, we can start to specify the procedures whereby scientists actively use accounts of values in the construction of their own and their colleagues actions. Another example of Mulkay and Gilbert's work on biochemists can clarify this point and serves to elucidate the particular pattern of accounting analysed above[36].

These authors examined the way in which a certain group of biochemists gave accounts of the success and failure, or correct and incorrect beliefs, of other scientists in the research network. They found a regular pattern of interpretation in the participants' talk. Each scientist would take his own scientific view (correct belief) as unproblematic. This belief was warranted in the great majority of cases by means of reference to the constraining role of experimental evidence and the correct application of a limited number of procedural rules. This, however, leads to a problem for participants in devising accounts of erroneous belief; if beliefs typically arise unproblematically from experimental evidence why are some scientists mistaken? This difficulty is resolved by the biochemists through the adoption of an alternative repertoire for characterising action and belief. In this case, supposedly distorting social, psychological or political factors are introduced into the accounts to make sense of errors. Thus an erroneous belief is not characterised as arising directly from the experimental evidence but as a product of incompetence, political bias, self interest, or one of a multitude of other socio-political factors. It is impossible for the analyst to accept literally any collection of such accounts from a specific field, because the accounts of different scientists are often incompatible, because each scientist says different things in different passages of talk, and because one direct implication is that virtually every member of the field is scientifically incompetent. Nevertheless, although the specific content of these accounts varies from one occasion to the next, they display a common underlying interpretative structure. It is, therefore, possible to use the accounts, not to explain how scientists develop views which are taken to be incorrect, but as revealing a recurrent interpretative technique which scientists employ to construct versions of social action, in this case versions of scientific error[37].

I have already noted that the accounts of values I discussed cannot be read as literal description of their role in various acts of theory choice. There are further parallels with Mulkay and Gilbert's study. The asymmetry in psychologists' accounts of values is very similar to that which occurs in biochemists' accounts of correct and incorrect belief. Thus social factors and interests are entirely absent from the empiricist version of testability which psychologists use to characterise their own theories. In contrast, they use a contingent version of testability when accounting for the selection of certain other scientists' (incorrect) theories. Here testability is characterised as open to strategic manipulation and is not seen as an impersonal constraint on action. This asymmetrical accounting has much the same consequences as error accounting. Psychologists' draw flexibly on the notion of testability to display their opponents' view (incorrect belief) as problematic, i.e. not directly and necessarily constrained by the criteria but influenced by contingent social factors[38]. And they also draw upon the notion of testability to characterise their own views (correct belief) as impersonal and determined by the criteria. Thus, by way of these accounting techniques, they legitimate their own position and undermine alternative, competing positions.

In the third analytic section I documented a passage which appears to be an exception to this regular pattern of accounting for theory choice. For in this extract (number 18) there is an inversion of the asymmetrical structure found elsewhere in the psychologists discourse. In this case the contingent version of testability is used to support the speaker's own theory, while the empiricist version is used to downgrade the value of an opponent's theory. However, this inversion is introduced in the context of a wideranging historical account which undermines the standard view of testability and divides theories into two distinct classes. By associating the contingent version of testability with certain classic and successful theories from the natural sciences the speaker recharacterises it as a positive feature of theories, while implying that the empiricist version is perhaps only a feature of a less important class of theories.

Although in one particular discursive context the speaker apparently produces an internally coherent warrant for his idiosyncratic perspective on theory choice, contingent accounts of theory choice tend to lead to interpretative problems. Difficulties arise, for example, if other scientists are said to support an incorrect theory in the face of contradictory evidence. It is not easy for a given speaker both to criticise their continued support of their theory and to espouse a contingent version of testability in relation to his or her own theory. In fact, as we see in extract 19, the speaker who proposed the inversion of the regular pattern of justification adopts the more standard empiricist justification when faced with this kind of situation. In this new context he suggests that the motives of the scientists who continue to support the theory may be disreputable. He is enabled to adopt this more traditional view by using his division of theories into two classes. For one class of theories (which includes his own) does not have to be straightforwardly testable. While this example further demonstrates the flexibility of the notion of testability it also illustrates the difficulty in trying to sustain across different contexts a contingent, social account of testability as a formal justification of theory choice. Although he uses the contingent version of testability for legitimation in extract 18, this speaker, like the others discussed, goes on to characterise it as a negative feature of his opponent's theory (extract 19).

These psychologists may therefore be seen as flexibly drawing on the notion of testability in the construction of an unproblematic version of the way their own and other's theories are chosen. Testability from this perspective may be viewed as a symbolic resource which participants use in the performance of specific interactional tasks. Participants characterise it as a constraint, or as providing no constraint, only in the context of further, situationally specific interactional work. It is the very generality of the notion of testability which enables participants to flexibly interpret it in these different ways. It must be emphasised here that it would be quite wrong to think of these scientists as confused about testability. For they use the notion in a regular and clearcut fashion, carefully fitting particular versions of the criterion to specific interpretative contexts.

In conclusion, then, I have not offered a straightforward refutation of Kuhn's model. For I have argued that the model is both too vague and inconsistent to allow clearcut appraisal, and that there is no way of getting directly at the kinds of evidence which might be relevant, namely definitive versions of the role of values in specific theory choices. However, I have illustrated some of the ways in which scientists themselves use values in the construction of versions of their acts of choice, and how the same value may be used by the same scientist to perform quite different interpretative tasks. It is possible, of course, that testability is an unusual value or that psychologists are not typical of the scientific community. However, there seems to be no obvious reason why notions such as 'scope' and 'simplicity' should be any more clearcut than testability; and Mulkay and Gilbert have documented the variable usage of these other Kuhnian values in the discourse of biochemists[39].

It seems likely - although he does not make it clear - that Kuhn's model is ultimately based on just the sorts of participants' interpretative work that has been explicated here. As I suggested in chapter one, it is difficult to see how models of this kind can be developed in any other way. Insofar as it is to be assessed in relation to the available empirical evidence, it must be compared with the kinds of accounts of theory choice examined above. Furthermore, it is probable that Kuhn, because of his interest in scientific progress, concentrates on accounts of values in the choice of 'correct' theories; indeed, just the sort of retrospective accounts that participants and historians use to show why significant theories were chosen. Moreover, as I have argued, it is in exactly these sorts of accounts that values are depicted as constraining theory choice. It seems likely, therefore, that it is by treating at least some of these sorts of accounts as literal descriptions, and by ignoring the specific interpretative contexts for which they were fashioned, that Kuhn is led to, or expects to gain empirical support for, the model of values constraining choices[40]. In the end, Kuhn's attempt to show that progress is ensured through the action of values may be merely a reification of scientists' self-legitimating talk.

CHAPTER SIX

SCIENTISTS' SOCIAL CATEGORISATIONS

I wish now to extend the discussion concerning the flexible application of criteria for theory choice to an examination of flexibility in the use of social categorisations. The general form of analysis will be similar to that of the previous chapter. I will show that there is considerable variability in the meaning and application of supposedly straightforward descriptive categories. This will reinforce the findings of earlier chapters and provide further evidence of the necessity for a systematic analysis of scientists' discourse[1]. However, in this chapter I intend to look in more detail at the interpretative practices through which the flexibility in category use is achieved. In what ways is the meaning of categories altered? And what devices enable such change? These questions will be addressed through a study of psychologists' use of the categories 'humanist' and 'mechanist' at the Theoretical Perspectives Conference'[2]. Firstly, however, I will discuss some attempts to deal with social categorisations of scientists analytically, starting with Kuhn's classic notion of the scientific paradigm.

All Together Now

The notion that scientists can be divided into categories of various kinds underlies virtually all social studies of science. Sometimes the notion is tacit, but often it is explicitly formulated. As Woolgar notes, the identification and definition of scientific collectivities is the 'basic preliminary task in many studies of science'[3]. The very idea of examining 'science' implies at least the existence of some sort of community of participants. Often analysts take scientific categorisations to be so obvious and unproblematic that they devote little attention to showing how they were arrived at. One categorisation, however, has generated a considerable amount of discussion and a body of empirical studies. This is, of course, T.S. Kuhn's concept of the 'paradigm'. It is unusual because it is taken to show more than merely participation in some broad scientific collectivity such as 'physicists' or 'social psychologists'. Membership of a paradigm group is taken to indicate a high degree of cognitive consensus. Moreover, this consensus will cover a diverse array of items: 'values for theory selection'; 'metaphysical models'; 'symbolic generalisations' and 'exemplars'[4]. According to Kuhn, these are 'the objects of group commitment... and as such they form a whole and function together'[5].

It is not my goal in this chapter to produce a detailed textual exegesis of exactly what degree of consensus is implied in Kuhn's notion. Indeed, the highly varied literature on paradigms and scientific development indicates that this sort of definitive reading might well be impossible[6]. Rather I wish to draw attention to the way that the notion of a paradigm has been used to organise items as different as 'values' and 'exemplars' so that they can be treated as a unit. In other words, I wish to emphasise the way it has provided a rationale for making, as it were, a neat set of slices through a multi-layered cake. For simplicity, and to avoid the large baggage of connotations which have become attached to the term paradigm, I will use this cake metaphor through the rest of this chapter. In the analysis which follows I will show that a very similar construction appears in member's discourse at certain junctures, although it is by no means the only form of participants' categorisation. Before moving on to the analysis, though, it is worth looking at some of the ways in which researchers have traditionally attempted to identify and explicate paradigms.

Various analytic techniques have been used with the goal of identifying the consensual matrix of items which supposedly make up paradigms and their boundaries. Quantitative work has concentrated on indices of communication and the explication of social networks[7]. Citations and co-citations along with other records have been analysed to reveal any regular structuring into discrete networks. Insofar as they aim to identify paradigms or 'invisible colleges'[8], these sorts of study depend on the equation of communication systems of some sort or other with shared systems of concepts and techniques. Yet there is no necessity for the existence of such a one to one relationship between communication (citation), belief and practice[9]. Thus to show that a group communicate is not sufficient to show that they share activities and beliefs. It might, of course, be argued that if communication is taking place then at least the parties are 'speaking the same (scientific) language'; that is, they share a set of categories in which to formulate agreement and disagreement. However, even this is assuming too much. Neither citation nor participants' specification of communication shows that communication is <u>actually</u> taking place, that there is shared reciprocal understanding. Going back to the criticisms of quantitative approaches in chapter one, we can see that there is an unacknowledged inference being made when citations are used as measures of communication; for citations only indicate the possibility of communication, they are not a sufficient condition for demonstrating its existence.

Other research has not tried to stick to purely quantitative indicators, but has tried to directly elicit the belief systems of scientists. These are got at through interview and questionnaire techniques. For instance, workers in a particular area have been asked to rank recent research contributions and the importance of certain future research questions[10]. Their ranking can be used to calculate a measure of similarity - the more similar the ratings the greater the degree of consensus which is taken to exist within the group. As a less structured alternative, replies to open-ended questions posed in interviews have been inspected to see whether scientists say 'the same thing' about their particular subfield, and, by inference, how far they believe the same thing and do the same thing[11].

There are a number of difficulties with these latter kinds of approach. To start with, they are subject to all the usual problems which beset these sorts of research instruments. These have been documented repeatedly and at great length in sociology and social psychology over the past 15 years[12]. Furthermore, as Kuhn himself notes, scientists may not have perfect or even very good insight into their own circumstances.

> Though many scientists talk easily and well about the particular individual hypotheses that underlie a concrete piece of current research, they are little better than laymen at characterising the established bases of their field, its legitimate problems and methods. (13)

This sort of criticism presupposes scientists' talk to be a fairly straightforward and homogeneous medium for communication, characterised here and there by certain unfortunate failures of insight. However, from the standpoint on scientists' discourse which has been developed in this thesis and elsewhere much more serious doubts must be expressed about this way of analysing consensus. Just because scientists appear to say the 'same thing' in an interview, say, does not mean that they will continue to do so in other contexts or that such accounts constitute a literal description of their scientific subfield. In fact accounts of the consensual nature of a field may vary radically, even within the discourse of a single scientist[14]. And even the issue of what should count as 'saying the same thing' is, of course, highly problematic[15].

An alternative to the analysis of citation and communication networks, and to the elicitation of beliefs using interviews and questionnaires, is to become a participant in the area of science being studied. For instance, in Collins and Pinch's work on research with children who claimed to be able to bend metal spoons by paranormal means, their analytic practice is to develop a working 'participants'' competence[16] by way of a varied range of procedures: doing work in the field; reading written materials; interviewing members; visiting conferences and doing much informal talking[17]. The suggestion is that by using this strategy the analyst is enabled to draw upon his or her member's competence to explicate the frames of meaning or paradigms shared by different scientific participants .

However, this approach appears to be vulnerable to the very same set of problems as arise with more traditional interview and questionnaire studies. Even if the social researcher is able to develop a working members' competence, there is no reason why he or she should not be subject to exactly the same difficulties in 'accurately' formulating the boundaries of social categories and the content of consensus as beset scientific members themselves. Part of the problem may lie in the idea that there is a single, definitive categorisation which will be revealed if all sources of bias and distortion are removed. The researcher may indeed learn to successfully make certain sorts of social categorisations appropriately and in such a way that they are accepted by members. Yet this does not solve the analytic problem of trying to formulate a technical version of consensual belief. For this will still involve resolving inconsistencies and ambiguities in the acceptable practice of categorisation. Furthermore, this approach assumes that there exists a unitary competence which will enable the researcher to assign scientists to membership of particular paradigm groups and explicate what sort of beliefs will count as rational in a given group. Alternative models of competence, which place more emphasis on the way it is occasioned in particular contexts, might not be so easy to reconcile with such a definitive account of paradigm membership[18].

It seems, then, that there are shortcomings and difficulties with each of the three main approaches for determining the existence of paradigms in particular scientific areas. Having explicated some of the methodological difficulties, let me mention some of the research which has examined the question of whether there are paradigms in psychology.

Categories of Psychologists

The early 1970's saw a flurry of activity on the issue of whether psychology is divided into paradigms or not. The presence of this analytic literature makes a study of psychologists' own categorisations particularly apposite. Most of this work has concentrated on the question of whether 'behaviourism', 'introspectionism' and possibly psycholinguistically based 'cognitive psychology' are paradigmatic or not. Palermo and Weimer[19] have argued that these terms refer to fully paradigmatic traditions of research. In contrast Mackenzie[20] and Warren[21] have argued against this claim on a number of grounds. Briskman[22] has suggested yet another possibility by arguing that although behaviourism does not meet the criteria for full paradigmhood it should nevertheless be considered to be a Lakatosian research programme.

Commenting on this disputatious literature, Weimer[23] has suggested that the debate is apparently irresolvable in the terms it is couched because of the complex interrelationship between theories of scientific development and the historical reconstructions they underpin. To decide whether psychology has paradigms and periods of revolution reference must be made to the 'historical record'. Yet this is not a neutral description of events but is an active construction which itself cannot be divorced from theories of scientific development. Weimer suggests that most of the major historians of psychology, and in particular E.G. Boring[23], worked with a simple empiricist historiography which stressed the orderly accumulation of scientific knowledge. No wonder, therefore, that the presence of paradigms in psychology has often been questioned; for they have been 'written out' of the historical record[24]. More interestingly for the present discussion, Weimer also claims to be able to detect the sound of committed theoretical axes grinding behind a number of these disputes about paradigms. Certain historical reconstructions of events, he suggests, may actually have the function of increasing the legitimacy of certain positions in psychology.

This issue of how analyses in terms of paradigms can be used for purposes over and above 'mere' description is discussed by Peterson[26] in a comprehensive review of the literature on paradigms in psychology. His general approach is to contrast various claims made in the analytic literature and by psychologists themselves with 'what Kuhn intended'; which is outlined in a confident definitive reading of Kuhn's various works. Despite this strangely innocent (even pre-Kuhnian) approach to Kuhn's work, Peterson highlights the way that these studies can use apparently descriptive or historical analyses as a vehicle for making highly selective evaluative claims. Thus he suggests that certain assertions about the existence of paradigms have been used to 'facilitate consensus and reduce debate'[27]. Describing a theory as paradigmatic in this way is to further imply that it is legitimate. On the other hand, if a debate can be characterised as a disagreement between two paradigms the need for rational discussion is reduced, as is the necessity to explain why traditional ways of thinking are difficult to overcome.

> Seeing a debate as a paradigm clash whose results are seen as depending on persuasion and conversion, may become self-serving and inhibit fruitful exchange. Those offering new paradigms have seen debate over their proposals as signs of a clash, which serves, of course, to affirm their approach as paradigmatic. (28)

In effect, therefore, Peterson is suggesting that what was originally a descriptive approach is vulnerable to being used in a normative fashion. Where it originally merely described situations in which rational debate had become problematic, it now is taken to legitimate the abandoning of certain kinds of disputes. Peterson's suggestion, then, is that the idea of paradigms and revolutions can be used, at least on certain occasions, for evaluative purposes by scientists. In the analysis which follows this notion will be examined in specific instances.

The Practice of Social Categorisation

It is interesting to note that virtually the only discussion of scientific conferences in the analytic literature concerns consensus and categorisation. Ziman provides a strong rationale for taking scientific conferences as a fruitful site for the study of categorisation. In fact, he implies that the scientist's main purpose for attending conferences is to situate themselves with respect to important consensual groupings:

> the primary motive [for attending conferences] is to be noticed as a serious contributor to the consensus, to be seen by the leaders, to act the part of membership in the [Invisible] College.
>
> A scientific conference, as the venue of the face-to-face social interaction that governs an Invisible College, is thus a fascinating phenomenon, full of hidden meaning and symbolic ritual... The actual papers themselves may not be of great importance, but the informal discussions, the talk over lunch or at the bar, questions from the floor and remarks of the chairman of the session, are the means by which the current consensus is dramatized to the participants. (30)

The analysis which follows will suggest serious difficulties for Ziman' characterisation of the role of social categorisation at conferences. Nevertheless, it is true that categorisation and consensus are a recurrent issue in the transcript. Some sort of social categorisation or consensus claim is made, implicitly or explicitly, in the vast majority of utterances by conference participants. Even a speaker's characterisation of their own beliefs and allegiances can be seen to presuppose assumptions about consistency and category membership.

As in the previous chapters, the analytic approach adopted here will be different from that common in most of the literature on consensus and paradigms. In particular I will not be attempting to formulate a more accurate method for describing the paradigmatic landscape of scientific specialties. With respect to psychology, at least, it seems unlikely that any further such attempts would resolve the debate about the presence of paradigms. Instead the way participants divide up their fields into social categories in the course of varied interactions will be the central topic for analysis; that is, I will attempt to elucidate members' practices of social categorisation rather than trying to produce a definitive set of analysts' social categorisations.

The analysis will concentrate specifically on the use of the categories 'humanist' and 'mechanist'. These were chosen as the central topic in part because they are characterised, on a number of occasions, in a way similar to Kuhnian paradigms. That is, they are depicted as made up of a diverse array of items - methods, forms of explanation, etc. - rather than merely being labels referring to general institutional specifications (psychology) or research specialties (artificial intelligence, say). These categories, therefore, are not simply reflections of institutional definitions and conventional problem areas, but puport to be picking out genuine groups with different practices and beliefs. The second reason for this choice is that the categories 'humanist' and 'mechanist' occurred very frequently in the transcript, particularly at some of the periods of most heated debate. They appear on 17% of the transcribed pages of discussion and are found in the talk of 28 of the of the participants. Moreover, as nearly one in three participants did not contribute to the discussion at all, this means that over half of those who did contribute used these categories at some point. Of course, not all these accounts can be discussed in this chapter. My approach will be to concentrate on cases where more than one account of the same topic is available. For it is only in these cases that the variability or consistency of the categories can be properly assessed. Even so, there is space to discuss only a part of the diversity in the use of these terms. Finally, it should be strongly emphasised that these are participants' not analysts' concepts - except in the extracts themselves they should be read in inverted commas throughout.

Psychologists' Formal Cakes

In this first section I will concentrate on the most detailed formal accounts of the categories 'humanist' and 'mechanist'. Two very elaborate descriptions of a distinction between opposing 'camps' of psychologists were produced in the formal conference papers and appear also in the published proceedings. These are virtually identical, and to save repetition I will discuss only one of them - the other is reproduced in Appendix B. The passage that follows appears at the beginning of the paper which puports to situate contemporary theoretical issues in a broader historical context. At the conference this was the second paper to be presented; however the order was changed for the published version in which this passage starts the very first page.

1. (1)It may be said that at present the psychological world is divided into two camps; on the one side are the champions of mechanism, on the other side the champions of the person. (2)The first camp makes its headquarters in biological psychology; animal behaviour provides the key to man's more complex functions, and objective experiment is the preferred method. (3)The second camp is more loosely organised and more varied in its opinions; its adherents are more likely to be found in clinical, differential and social psychology; they employ self-report and conceptual analysis, and prefer purposive to mechanistic explanation.
(4)Any such division is certainly a great oversimplification. (5)There are many shades of opinion in each camp; some theorists combine aspects of both;

yet others are hard to place in these terms at all. (6)But it provides a useful first approximation to the different kinds of [metatheoretical frameworks] that we are likely to encounter in psychology today, and reminds us that this division is part of a much wider debate in philosophy and the social sciences generally, which [has been termed] the rivalry between 'plastic' and 'autonomous' man. (Fish, published paper)

The extract begins by proposing a broad division that splits the 'psychological world' into 'two camps' (1). By characterising each camp as comprised of supporters of a certain kind - 'champions' - the writer emphasises that this is a social division which represents a categorisation of scientists, rather than a clear conceptual or theoretical distinction. This sort of social categorisation into 'camps' with particular 'adherents' who are 'organised' and have 'opinions' is continued into sentences 2 and 3. In addition, some features which distinguish the membership of the two camps are identified. Thus the camps are viewed as variously distributed across the different subgroupings in psychology: 'champions of mechanism' are more likely to be found in 'biological psychology' (2); 'champions of the person' are more often to be found in 'clinical, differential and social psychology' (3). The camps are also distinguished in terms of methods ('objective experiment' versus 'self report/conceptual analysis'), subject matter ('animal behaviour' is essential or not) and forms of explanation ('mechanistic' verus 'purposive').

In sentences 4 and 5 some qualifications are placed on the picture so far produced. The writer suggests that the notion of two camps is an over-simplification of the situation because it ignores different 'shades of opinion in each camp' and some theorists who 'combine aspects of both camps' or cannot be categorised by this sort of division. However, in sentence 6, two warrants are provided for using it. Firstly, the writer emphasises its heuristic value in approximately characterising the different theoretical frameworks in psychology. Secondly the categorisation is depicted as emphasising the continuity of the division in psychology with a more general 'debate in philosophy' and other social sciences. There is a subtle but significant shift in terminology in sentence 6. It seems that the social categorisation, a division between camps, becomes a part of a more abstract debate between two positions concerning the nature of human action. The divide between individual psychologists is thus recharacterised as part of a debate between philosophical ideas. In Kuhnian terms, the account proposes that different metaphysical assumptions underly each category of psychologists.

An extended abstract of this paper was precirculated to all participants before the conference. In it the first three sentences are identical to those in the published version. However, the sentences 4,5 and the first part of 6 are ommitted.

2. It may be said that at present the psychological world is divided into two camps; on the one side are the champions of Mechanism, on the other the champions of the Person. (2)The first camp makes its headquarters in biological psychology; animal behaviour provides the key to man's more complex functions, and objective experiment is the preferred method. (3)The second camp is more loosely organised and more varied in its opinions; its adherents are more likely to be found in clinical, differential and social psychology; they employ self-report and conceptual analysis, and prefer purposive to mechanistic explanations. (4)The contrast is part of a much wider debate in philosophy and the social sciences generally, which [has been termed] the rivalry between 'plastic' and 'autonous' man. (Fish, extended abstract)

The effect of these deletions is to remove the qualification on the accuracy of the writer's social categorisation. Furthermore, the relation of the social division to a wider philosophical debate is here merely stated, rather than being depicted as an inference from the use of this categorisation. This version thus seems slightly stronger (less hedged by qualifications) although much of the material is shared[31].

To summarise, then, the basic feature of these accounts is that they divide psychology according to the way its members participate in one of two camps. These camps are further distinguished according to their emphasis on particular research areas, methods used, general subject matter studied, and form of explanation adopted. Without in any way wishing to suggest that an analyst would be likely to identify these camps as paradigms, or would be right to do so, the formal similarity is readily apparent. Membership of one of the camps is not taken merely to be nominal but is seen to allow deductions to be made about a whole range of the member's activities and beliefs. This account is a classic cake account. It groups together diverse features of scientific practice and belief into coordinated entities. It seems, therefore, that at least on certain occasions psychologists' draw upon categories which have a very similar structure to 'paradigm groups' when they are making sense of their own discipline. However, as we examine further participants' accounts it will become clear that these are not the only ways that psychologists' use these categories.

Crucial Evaluative Distinctions

In this section I will discuss two extracts from the transcribed discussion which also formulate the difference between 'humanist' and 'mechanist' psychologists. In these cases the cake structure is abandoned in favour of a single distinction, which is used to mark a crucial difference between the putative camps and to show one is inferior to, or superior to, the other. As the discussion proceeds I will also document some of the detailed interpretative procedures through which these 'crucial evaluative distinctions' are achieved.

The following extract is a response to the paper by Fish described above (extracts 1 and 2). The speaker adopts the distinction between two camps of psychologists. However, he disagrees with what he takes as the premise of Fish's paper.

3. (1)I find the premise extraordinary: that there has been no advance in the laboratory and experimental investigation of psychology from what you call the mechanist camp since William James wrote his textbook. (2)I think of Pavlov having produced a generalisation at something, it must have been about 1903, about the conditioned reflex. (3)And that has not been thrown aside in favour of other things. (4)It has been precisely refined over the years... [states generalisation] (5)Another, I could give you hundreds of examples from the study of learning. (6)And you will probably object to me that I will qualify each of them by saying that they apply to specified experimental conditions. (7)But if you make that objection then you have misunderstood the nature of physical science to which our science is attempting to approximate. (8)The generalisations of physics and chemistry <u>also</u> relate to specified experimental conditions. (9)No physicist makes a statement that

applies at the level of generalisation, empirical generalisation, across the universe... [indicates two further generalisations from psychology] (10)I have no need to say more, I am sure. (11)So the premise seems to me to be quite wrong. (12)On the other side of the fence, looking at what you call the humanist camp, I would like to know, conversely, where there has been any progress? (13)I will there go back to the point I made in reply to, er, Dr Carlisle's preentation, that I don't think that camp has any way of testing between rival generalisations and making progress. (14)So does the humanist camp have anything more specific to offer today than it had in 1890? (Norton, transcript, 12-13)

The speaker, Norton, starts by identifying as 'extraordinary' what he describes as the 'premise' of the preceding paper (extracts 1 and 2). This premise concerns progress in the 'mechanistic camp'. However, he distances himself from the term 'mechanistic'; it is identified not as his own term but that of Fish, the paper's presenter. Thus Norton talks of progress 'in what you [Fish] call the mechanistic camp' (1, emphasis added), indicating that he might not wish to use this term. However, Norton apparently takes the reference of this term as unproblematic. That is, he seems to accept that there is a group of people, a camp, but 'mechanistic' is not the appropriate term for it. This enables him to warrant his suggestion that Fish's claims about progress are 'extraordinary' by mentioning specific researchers and their work (2). For it would, of course, be difficult to question the reference of the term and at the same time apply it to particular members of a camp.

Norton contrasts Fish's 'premise' that there has been no progress in the 'mechanistic camp' with the example of Pavlov's 'generalisation' about conditioned reflexes, which he describes as having been 'refined over the years', rather than abandoned (2,3 and 4). And he goes on to suggest that he could provide 'hundreds of other examples' from research on learning (5). This use of contrasting pictures depicting on the one hand 'no advance' and on the other 'hundreds of examples' of generalisations which, like Pavlov's, have stood the test of time, meshes with Norton's characterisation of the first picture as extraordinary. It is displayed not as merely an ordinary disagreement; Fish's claim conflicts with so many counter examples that it is almost anachronistic. One counter example might be over-

overlooked but it would be an extraordinary achievement to overlook hundreds[32].

Immediately after this the speaker formulates what he claims will be a 'probable objection' that Fish will make to his points (6). This is that his examples are only generalisations within 'specified experimental conditions' (6). However, Norton claims that Fish's supposed future objection is based on a 'misunderstanding of the nature of physical science' (7). For generalisations in physical science *also* have this property of relating only to 'specified experimental conditions' (8). Norton thus characterises any criticisms of the generality of his examples as equally criticisms of work in physics and chemistry. Physicists, he claims, consensually avoid generalising '*across* the universe' (9). Thus in the course of sentences 6, 7 and 8 he draws a parallel between the generalisations in an area of psychology which he wishes to cite as examples of progress and generalisations in the natural sciences. This parallel is used to reinforce the bizarreness of Fish's suggestion that there has been no advance in this area of psychology. For it now becomes equivalent to the suggestion that physics and chemistry have not advanced.

Norton here uses a technique which can be called preformulation. That is, he formulates what he claims will be a future objection and then undermines it. Such a technique may not only defuse future criticism but also implies that critics hold false (and predictable) views. Moreover, by formulating his opponents putative views within his own discourse they can be characterised in a way which *prepares* them for rejection. Another way of thinking of this accounting technique is as a formulation of a contrast between a real and apparent state of affairs. One view of science is depicted as plausible, but as only a misguided *apparent* version; the other is treated as the *real* state of affairs, which may be concealed behind false appearances. Of course, much of scientific debate has this kind of underlying structure. Yet the difference here is that the contrast is formulated by a single speaker. Some of the accounting possibilities offered by this technique will be discussed in chapter seven.

After providing descriptions of two further generalisations Norton indicates that these, along with earlier ones, will be sufficient to warrant his case. He is sure that he will have to give no more examples (10). This comment seems to present his instances of progressive generalisations in 'mechanistic' psychology as so self evidently persuasive that anyone asking for additional examples must be abnormal in some way, or even irrational. And here the initial criticism is forcefully reiterated: that Fish's premise (of no advance in 'mechanistic' psychology) is 'quite wrong' (11).

At this point Norton changes the focus of his talk to the 'humanist camp'. Again he distances himself from the term by talking about 'what <u>you</u> [Fish] <u>call</u> the humanist camp' (12). But again the reference of the term is taken to be unproblematic; the existence of such a group of researchers is not queried. The speaker identifies what he claims is a central feature of the 'humanist camp'. This is it has no 'way of testing between rival generalisations and making progress' (13). He asks if the humanists 'have anything more specific to offer in the way of generalisations ' than in 1890 (the date of William James's text). Yet this apparently straightforward question seems to have a rhetorical emphasis, sharpening the contrast between the many generalisations of 'mechanistic psychology' which he has referred to and the lack of any such generalisations in 'humanistic psychology'; 'humanists' have produced vague talk while 'mechanists' have produced empirically supported laws.

In the course of his reply to Fish, therefore, Norton adopts the general image of the two camps of psychologists, whilst at the same time indicating suspicion of Fish's terminology for referring to the camps. He formulates the difference in a way quite different to Fish. Instead of proposing a series of distinctions and building up a cake account from them he concentrates on the single issue of progress and suggests that the crucial difference between the camps is the lack of progress in the 'humanist camp'. He thus uses the notion of a division to make a critical evaluative claim: only the 'mechanist camp' of psychologists exhibits the progress characteristic of the natural sciences.

The next extract has a very similar basic structure.

This time, however, the viability and coherence of a 'humanist' approach is emphasised.

4. (1)The fact is that mechanism and humanism, as we were discussing earlier, [] are incompatible. (2)It seems to me that any humanism requires as a minimum condition that the subject should have freedom in some sense and that he should have consciousness which is effective in some sense, not merely epiphenomenal. (3)And that these two points are utterly unacceptable to any mechanist, who has to account for these phenomena in some other way. (4)Now, harking back once again to a discussant in a previous paper who talked about the nature of philosophy, it seems to me that philosophy has always worked by identifying problems which were capable of answer and hiving them off into separate disciplines. (5)And it seems to me that we have now reached the stage where philosophy has done enough to show that the problems of an effective consciousness, the problems of free will, can be reconciled with a naturalistic view of the phenomenon of man, quite compatible with science, as long as we understand what science is really about and don't go in as we traditionally have in psychology assuming that science is what the 19th Century philosophers of science said it was. (6)They were simply wrong. (7)And modern philosophers of science present a view of science which makes it perfectly possible to have a central model which will permit all the phenomena in which humanists are interested in and all the phenomena in which natural scientists are interested to coexist. (Thomas, transcript, 23)

The speaker starts by emphasising the incompatibility of 'human<u>ism</u>' and 'mechan<u>ism</u>' rather than emphasising a division between 'human<u>ists</u>' and 'mechan<u>ists</u>' (1). It might seem that this introduces a separate kind of discussion, which emphasises <u>conceptual</u> differences rather than differences between identified groups of scientists. Yet the speaker, Thomas, quickly blends this with talk of social categories ('any mechanist' - 3, 'humanists' -7)[33]. Having emphasised the incompatibility of 'humanism' and 'mechanism' Thomas goes on to state a 'minimum condition' for 'any humanism', which is that people ('the subject') should have 'freedom' and an 'effective consciousness' (2). Furthermore, this is depicted not only as a necessary condition for 'humanism' but as 'utterly unacceptable' to 'any mechanist' (3). The 'mechanist' must account in some other way for phenomena which the 'humanist' explains as a product of free will (3). This issue is thus made crucial for distinguishing the two groups of psychologists.

At this point Thomas relates the discussion back to a previous issue concerning the nature of philosophy and suggests that philosophers work by identifying answerable questions and 'hiving them off' into other disciplines (4). The reason for this apparent digression into the workings of philosophy becomes clear in the next sentence. There the problem of 'free will' is described as just such a problem which can be 'hived off' into science itself. For philosophers have shown that free will does not conflict with a 'naturalistic' approach to the 'phenomenon of man' (5). However, this point is qualified. He claims that this compatibility will only be visible if 'we understand what science is really about' (5). The problem is, he explains, that psychologists have traditionally adopted a view of science derived from 19th Century philosophers. It is this which has prevented psychologists from seeing the compatibility of notions of 'effective consciousness' and 'science'. These 19th Century philosophers 'were simply wrong' (6). Modern philosophers, in contrast, show science to be able to study phenomena of interest to 'humanists' and 'naturalist scientists' (7)[34].

Thomas characterises the contrast between 'mechanism' and 'humanism' very differently from Norton. Instead of addressing the issue of progress in the two camps, he emphasises a sharp distinction in their approach to such phenomena as 'effective consciousness'. 'Humanism' is depicted as able to deal with these from a 'naturalistic', 'scientific' viewpoint; 'mechanism', in contrast, must find some other way of dealing with them. To take this approach would be 'utterly unacceptable'. This extract, then, like the last, can be seen to make an evaluative claim based around a single crucial distinction. In this case the claim is that 'humanists' can deal with central phenomena such as 'free will' from a scientific perspective. 'Mechanists', however, are unable to deal with these phenomena in this way. While Norton emphasised the progress of 'mechanism' with respect to 'humanism', Thomas emphasises the greater ability of 'humanist psychology' to deal with certain kinds of difficult subject matter. Despite their substantive contradictions both of these extracts share the same underlying structure. Each draws upon a crucial evaluative distinction for displaying the adequacy of one group of psychologists' work with respect to the other's.

A further similarity between these extracts is that each draws on a general model of science to warrant specific claims and preformulates an incorrect model. Norton depicts the generalisations produced by scientists in the 'mechanist camp' as being of the same kind as those of physicists or chemists. Thomas depicts the essential features of 'humanism' to be fully amenable to scientific analysis. Each speaker preformulates a plausible alternative to their own 'correct' view and implies that those scientists who hold such misguided beliefs may be led to equally misguided criticisms. In each paper, therefore, criticisms are dismissed in advance. In extract 3 the 'mistaken' view of science is contrasted with the 'correct' view in which the 'generalisations of physics and chemistry... relate to specific experimental conditions' (3.8). This view is baldly stated, without warrants or hedges. In extract 4 Thomas contrasts the 'traditional' psychologists' view of science with the 'real' view. However, in this case the speaker more fully explicates the contrast. The traditional psychologists' view is depicted as arising from an identification of science with what '19th Century philosophers of science said' about it (4.5). However, Thomas's own beliefs are based on the improved ideas of modern philosophers. Unlike Norton, Thomas implies that science is not directly understood but available through the analyses of philosophers. In each case the general technique is to show that one camp is more scientific than the other. That is, that the distinction 'mechanist'/'humanist' will collapse into the more basic distinction 'scientific'/'non-scientific' (or vice versa).

In constructing these accounts other social categories are taken as unproblematic. For instance 'physicists' (3.8) and '19th Century philosophers of science' (4.5). These categories are treated as straightforwardly consensual; no hint is given that problems might arise in saying who is a physicist or not, or even which philosophers properly belong in the 19th Century. Thus Norton confidently asserts that 'no physicist' would make generalisations beyond specific experimental conditions[35]. Thomas does take the catogory of philosophers to be divided; however it is split only into those of the 19th Century and those of the 20th. The point of the division is that it enables Thomas to account for psychologists' (preformulated) misunderstandings of science. As philosophers have progressed from the 'simply wrong' to the modern consensually correct view psychologists have yet to catch up.

To summarise this section: both speakers do interpretative work on the nature of the categories 'humanist' and 'mechanist'. Each highlights a certain sort of distinction and emphasises the properly scientific nature of one of the categories with respect to the other. Furthermore, the speakers warrant their claims by way of specific interpretations of the genuine nature of science which are contrasted with a popular but mistaken alternative. Nevertheless, although the two speakers evaluate 'mechanism' and 'humanism' very differently, there is no explicit disagreement in their specific claims about what divides the two camps. Norton does not address the issue of how 'mechanism' deals with purposive behaviour; Thomas does not comment on the respective production of empirical generalisations from the camps. However, as we examine further accounts in the subsequent analysis it is possible to document highly discrepant versions of each of these issues. We will see that not only the formal cake accounts but also these accounts invoking a crucial evaluative distiction are reinterpreted and undermined.

Accounts of Testability and Progress

In this section I will concentrate on the issue of theory testing and progress raised in extract 3. In that extract Norton suggested that a crucial difference between the 'mechanist' and 'humanist' camps is that only psychologists working within a 'mechanist' framework can properly test theories and thereby produce progressive science. In the three extracts which follow, we will see this claim repeated in even stronger terms, rejected and then reaffirmed. As well as providing evidence of the flexible meaning of these categories, these extracts provide further examples of the accounting techniques through which these variable categorisations are achieved.

The following passage shows Norton reiterating the importance of testability in response to an earlier account by a participant called Reese which outlined four central areas of difference between 'mechanists' and 'humanists'. Indeed, this was a classic cake account similar in many ways to that seen in extracts 1 and 2. Norton responds to it by denying what he takes to be Reese's suggestion, that 'mechanist' psychologists cannot deal with 'experience' but only with 'behaviour'.

5. Norton. (1)You [Reese] suggested that the mechanist studies behaviour and that the humanist studies experience. (2)There is no need for that to be so. (3)Let me tell you of a beautiful little experiment which studies experience, brought it under experimental control, and proved that the reports of experience were veridical.

Reese. (4)But I was not arguing that it need be so; I was trying to point to the facts of the case and the facts are that/[Norton cuts Reese off]

Norton. (5)I take your point, but I was trying to use what you had said as a way of answering the question, by saying that anything is capable of scientific investigation if you think about it hard enough. (6)Now that, to me therefore [] the key issue is not what the subject matter is. (7)You can apply scientific method to anything. (8)Nor is it what the theory is that is under test. (9)The hypothesis can come from anywhere. (10)The trouble with Freud is not that his theory postulates very curious entities like ids and egos, but that they cannot be tested. (11) If somebody could propose a way of testing Freud, Freud would become a scientific theory. (12)If somebody could propose, including Peter [Carlise] himself, a way of testing Peter's views about social behaviour, that would become a useful theory. (13)And until that point is reached it is not useful. (14)I don't think it is useful to the humanists any more than it is to the so called mechanists. (transcript, 83-84).

Norton starts his account by formulating one of the points which the earlier speaker, Reese, described as distinguishing 'humanism' from 'mechanism'. This is that 'mechanists' study behaviour and humanists study experience (1). He suggests that this need not be the case (2) and goes on to refer to a 'beautiful little experiment' which took a certain sort of experience, studied it 'under experimental control', and showed that the subject was reporting it truthfully (3). This is proffered as an example of 'mechanistic' research on experience. Thus it seems that Norton is here identifying the 'mechanistic' approach with an experimental approach.

Reese, who originally suggested the division in terms of subject matter, responds to Norton by claiming that although there is no necessity for this distinction the facts of the matter are that 'mechanists' deal with behaviour and 'humanists' with experience (4). Norton appears to agree with Reese; yet he does not seem to modify his account in any way. In his reply he gives a further formulation of his point that 'mechanists' can study experience. However, here the claim is that '<u>anything</u> is capable of scientific investigation' (5). In sentences 3 and 5, therefore, Norton uses firstly the term 'experimental' and then the term 'scientific' as interchangable with the term 'mechanistic'.

Norton carries on to suggest that because of the fact that 'anything' is amenable to 'mechanistic' investigation what subject matter is investigated is not the key issue (7). What is crucial, Norton argues, is whether the theories can be tested (11-14); for instance, it is the lack of testability of Freud's theory which stops it from being scientific (11-12). Again here, Norton takes 'mechanistic' to be equivalent to 'scientific'. And finally Norton refers to Carlisle's ideas as 'views' and suggests that only when a way of testing them can be proposed will they become 'a useful theory' (13-14).

Following Halliday, I will use the term 'relexicalisation' to refer to this situation in which there is a systematic replacement of terms[36]. For Halliday and linguists working within the same tradition the substitution of lexical items in this way is a functional process; it enables the speaker to avoid certain sorts of troubles which potentially arise when 'managing reality' and to coherently achieve certain accounts. An example from Trew's work on newspapers can illustrate this[37].

Trew carried out a detailed examination of the way certain happenings were reported in newspapers[38]. In particular he examined the way the terms used to describe events and processes were modified as stories were reiterated on subsequent days in the same newspapers. Trew suggests that the stories are not merely being condensed or more briefly summarised but are actually transformed in this process. For instance, in a story concerning a number of people killed by police during a riot in Rhodesia, Trew notes a systematic sequence of modifications from day to day. In the first report it is claimed that '11 Africans were shot dead... when police opened fire on a rioting crowd'. This expresses in an only weakly causal form the link between the actions of the police and the deaths, as is clear when it is compared with the potential: 'police shoot dead 11 Africans in riots'. However, in the following day's report both the agents and the manner of death are deleted altogether leaving: '11 Africans were killed in riots'. Crucially the term 'shot dead' is replaced by 'killed'. Trew argues that processes ofthis kind (this is only a small part of his analysis) allow participants to construct explanations which would be untenable when applied to the initial, untransformed version. The relexicalisation of 'killed' with 'died', or its reverse changing 'killed' with 'murdered', allows the construction of highly variable and conflicting accounts of the original events.

Coming back to the present analysis, two sorts of relexicalisation can be identified in extract 5. On the one hand, certain terms such as 'experimental' and 'scientific' are substituted for the term 'mechanist'. On the other, 'humanist' is made equivalent to 'non-scientific' and 'non-useful'. Overall Norton contrasts 'useful, testable, experimental, scientific, mechanistic, theories' with 'useless, untestable, non-experimental, unscientific, humanistic, views'. The division between 'humanists' and 'mechanists' comes to mean the distinction between 'scientific' and 'non-scientific'. This process transforms a boundary between two camps of scientists into an opposition between two discourses, one scientific and the other not. Essentially this is the same process as we saw occurring in extracts 3 and 4; the opposition 'mechanist'/'humanist' is collapsed through a sequential replacement of terms into the (tacitly evaluative) opposition 'scientific'/'non-scientific'.

The following speaker responds immediately to Norton's claims. Carlise responds to Norton by stressing that testability does not necessarily arise from taking his kind of experimental approach but depends on the way theories are formulated.

6. Carlisle. (1)I would like to say something quickly, because it does seem to me that this question of testability is not simply a question of scientific experiment itself, but a question of the type of way in which a theory is formulated. (2)And, er, when I am concerned with, er, whether a mother has a particular intention in relation to a baby, say, that can actually be tested out, with evidence, in terms of what, as a consequence of having that particular intention, I would suggest a mother goes on to do. (3)And if she in fact does it I take that as confirming evidence. [] (4)So what I am, er, baulking at is the great apparatus of experimental design and control and that sort of thing. (5)But as soon as you mention the example of the eidetic imager - a single subject and a crucial test - I would think that I could match that with some of the things with which I am interested.

   Norton. (6)Most of psychophysics was done with the single subject.

   Carlisle. (7)Um, sure, right. (8)Well, I am more than happy to accept that. (transcript, 85-86)

In this response, Carlisle argues that testability is not merely dependent on the kind of experimental approach adopted but also on the type of theory one has. The researcher can test whether a mother has a certain intention by checking what the mother does (2). If the mother acts in line with a hypothesised intention then that is 'confirming evidence' for that hypothesis (3). Carlisle thus argues that it is not testability per se that he is baulking at (for this can be provided if the theory is correctly formulated) but the 'great apparatus of experimental design and control' which he sees Norton as advocating for all psychologists (4). However, Carlisle contrasts this with the particular research that Norton mentioned on mental imagery. The latter research, Carlisle claims, is very similar to things in which he is interested (5).

At this point, instead of producing any critical comments, Norton notes that 'most of psychophysics' was done in this way (6). He thus seems to support Carlisle's endorsement of the value of this kind of study[39]. Carlisle in turn expresses agreement with Norton ('sure, right' - 7) and emphasises that he is 'happy to accept' this kind of work (8). Nevertheless, through this account Carlisle undermines the contrast in Norton's attempted relexicalisation. He does not adopt the offered terminology of 'unscientific, non-experimental, untestable, humanistic views' for depicting his own work. Instead he argues that it produces 'theories' which are amenable to 'crucial tests' although they do not correspond to the 'great apparatus of experimental design and control and that sort of thing'. He thus counteracts Norton's relexicalised version, splitting up the particular pattern of distinctions and associtions which has been generated, and substituting his own replacement. In this the opposition is not 'science'/'non-science', or even 'testable'/'untestable'; rather it is concentrated on Norton's methodological approach. Carlisle formulates this as an unwieldy and unresponsive structure for which his own approach substitutes precise tests with single subjects.

Returning now to the general issue of variability between social categorisations, it seems that Norton and Carlisle are able to agree, at least on this occasion, on a certain subset of possible research methods which are acceptable and which allow theories to be properly tested. In extract 3 the crucial importance of testability for distinguishing 'mechanistic' work from 'humanistic' was emphasised by Norton. Yet it now seems that 'humanistic' research <u>can</u> be testable. Furthermore, it is clear that Carlisle takes his own work as testable[40]. The crucial evaluative distinction between the camps thus appears to be an occasioned matter. It is supported by Norton in extracts 3 and 5 and undermined by Carlisle and Norton in extract 6. It should not, however, be thought that this variation merely results from a change of opinion or a lack of understanding which has been rectified. There is no evidence that the disagreement has been sorted out and the participants now understand the testable nature of 'humanism'. Indeed, it is not long before the crucial evaluative distinction between the camps is reaffirmed.

At a later point in the conference a discussant returns to the issue of the testability of 'humanism'. He brings the point up in the course of a description of his own position.

7. <u>Keefe</u>. (1)I think that a behaviouristic view - I don't particularly like the term; lets say a scientific view, or a bioscient., a biological view of man/ [Squire cuts Keefe off]

Squire. (2)Not 'scientific', please! (3)That's the one thing it isn't, so lets not use moral terms like/ [Keefe cuts Squire off]

Keefe. (4)But you haven't heard it yet! [laughter] (5)Well, OK. (6)This is the view, then, that man's behaviour is governed by natural laws, that natural laws can be understood, and it is the task of science to describe or represent these natural laws. (7) This means that a scientific view of man must make propositions the truth value of which can be assessed. (8)OK. [] (9)Now, the objections, the discrepancy I think, the conflict between the so called biological or behaviouristic or scientific - if you will pardon me - view of man and the alternative, whatever you want to call it - there are so many other terms that are applied to the alternative - from the scient., from the biological or behaviouristic point of view, we, I do not understand how one tests the truth value of assertions that are generated by, for example, Peter Carlisle's theory. (10)And that, and several things have been said here about Jim Norton's contribution to this symposium. (11)I think that he was completely consistent in asking that question over and over again. (transcript, 288-289)

Keefe starts his description of his work by indicating a difficulty with what to call it. He suggests possible alternatives to 'behaviouristic', such as 'scientific', 'biological' and 'bioscientific' (1). However, Squire interrupts him to complain that the term 'scientific' would be inappropriate (2), because Keefe's work is not that ('that's one thing it isn't' - 3). Keefe in turn interrupts Squire and reproaches him for making this judgement before his work has been properly described (4). This is another attempt at relexicalisation, again with the goal of substituting 'scientific' for 'mechanistic'. In this case the process is questioned by another participant who suggests that it is misleading, and indeed immoral.

Keefe goes on to outline his view as being that 'man's behaviour is governed by natural laws' and that the task of science is 'to describe' these laws (6). He draws a further implication from this, which is that scientific work must produce testable propositions (7). This he relates back to earlier discussions of the testability of 'humanism' and 'mechanism' (10-11). He repeats the earlier attempt at relexicalisation, formulating a series of different descriptions of his work: 'biological', 'behaviouristic', 'scientific', ironically apologising for 'scientific' as he does so (9). This can be viewed as an instance of 'overlexicalisation'. Trew suggests that repetitions of terms with slightly different meanings in this way can have an almost tautological effect and work to foreclose any discussion and disagreement[41].

Keefe does not name the alternative to his approach, merely commenting that there are 'so many terms' for it (9). Yet it is clear the use of Carlisle's 'theory' as an example and from the reference to Norton's criticisms that the earlier dispute between 'mechanism' and 'humanism' is being referred to. The crucial feature of the extract, however, is that Keefe characterises his account as a reassertion of Norton's position, which is that no way has been put forward for testing Carlisle's theory in particular and 'humanist' theories in general. For Keefe, clearly, the question of whether theories produced by the 'humanist' camp could be tested was never answered and the distinction still stands.

To summarise this sequence of extracts, then; Norton initially distinguished between the 'humanist' camp and the 'mechanist' camp by suggesting that 'humanists' could propose no way of testing their theories and thus generate progressive science (extract 3). Later Norton reasserts this distinction, and indeed implies that testability is the <u>only</u> feature that necessarily separates the camps (extract 5). In contrast Carlisle argues, using a specific research example produced by Norton as an illustration, that 'humanists' can and do formulate testable theories and go on to test them. At this point Norton seems to accept Carlisle's claims. Thus, on this occasion at least, the two participants take testability to be a feature of both 'mechanist' and 'humanist' work (extract 6). It should not, however, be thought that the differences have been resolved once and for all. For later in the conference Keefe reiterates Norton's point and treats it as unanswered (extract 7). The distinction of 'humanists' from 'mechanists' according to their production of testable theories seems therefore to be an occasioned matter. In extracts 3, 5 and 7 Norton and Keefe treat testability as a crucial difference between camps. On the other hand, in extract 6 Carlisle and Norton accept that the 'humanist' camp is able to produce testable theories.

Accounts of Purpose and Agency

In the previous section I discussed the variability in the way theories from the 'humanist' and 'mechanist' camps are seen as testable or not. In this section I will return to the other crucial evaluative distinction concerning agency. In extract 4 the single crucial feature dividing the camps was taken to be the ability to deal with agency and human purposes. It was suggested that only 'humanists' have managed to adopt a proper scientific approach to purposive behaviour. For 'mechanists' cannot deal with purposes as such; they can only be dealt with as an epiphenomenon or as a result of some more fundamental causal process. In this section I will show that this distinction too is open to variable reinterpretation.

In the accounts discussed above there are a number of instances in which Carlisle is characterised as a member of the 'humanist' camp; indeed, on occasion he seems to be depicted as a typical or paradigm member of this camp with his work taken as synonymous with 'humanism' (5.12-14, 7.9-11). However, in the following extract Thomas criticises Carlisle's work specifically because of its (putative) emphasis on automatic rather than purposive behaviour.

8. (1)...it seemed to me that, um, Peter Carlisle's analysis of the humanistic tradition had got things upside down. [] (2)it seemed to me that while in theory <u>much</u> of our everyday interaction is at an unthinking level, at a level of acceptance - or we wouldn't be able to say precisely why we behave as we do or how we learned to interpret other people's social signals in the way that we do, quite automatically - that he underestimated the extent to which conflict existed in this social situation, and the extent to which once conflict becomes apparent [] you have then to fall back on the problem or not as the case may be. (3)And it seemed to me more appropriate to regard the mode that Peter [Carlisle] regards as important as being the residue of numerous acts of active problem solving on the part of individuals in contemporary society. (Thomas, transcript, 68)

Thomas suggests that Carlisle's analysis of the 'humanistic tradition' has got things the wrong way up (1). For whereas Carlisle is presented as emphasising that automatic activity is primary, the speaker maintains that this primacy is illusory. Thus, although 'in theory <u>much</u> of our everyday interaction' is 'automatic' and 'unthinking', in situations where 'conflict becomes apparent' people have to think about these problems and try to solve them (2). Thomas here fleshes out why he thinks Carlisle has 'inverted' the 'humanist tradition'. He suggests that the 'model' of activity which Carlisle treats as central is really only the residue of 'numerous acts of active problem solving' (3).

In this extract, then, Thomas, who argues in favour of 'humanism' in extract 4, asserts that Carlisle is in a specific respect not properly within the 'humanist tradition'; even though he argues in favour of 'humanism' (extract 6) and is depicted as a 'humanist' by Norton (extract 5) and Keefe (extract 7). Thomas's criticism concentrates on the issue which he has claimed is the essential criterion for research to be 'humanistic'; i.e. it must scientifically examine purposive behaviour. He suggests that Carlisle is treating purposive behaviour, or the products of purposive behaviour, as automatic and thereby 'inverting the humanist tradition'. Here is an example, therefore, of a psychologist who has been identified by certain other speakers as a 'humanist', and identifies himself in this way, having his work criticised for not satisfying the essential requirement of humanism; for Norton and Keefe Carlise is a typical and indeed paradigm 'humanist'; for Thomas, Carlisle does not meet the essential criterion of 'humanism'.

The next two extracts are from Coleman's contributions to the discussion. In the second, Coleman gives an account of the way his research deals with purposive behaviour. The first is a reply to Fish's historical paper (extracts 1 and 2) and shows Coleman identifying himself with the 'mechanist camp'. It is worth examining this passage in some detail, along with the reply from Fish, because it provides further illustration of some of the detailed accounting techniques used in the formulation of social categorisations.

9. <u>Coleman</u>. (1)I am rather worried by the prevalence of your war-like metaphors. [](2)you use these terms like 'warring faction', 'champions', 'continuous conflict' and talk about 'winning an outright victory' and so on. (3)And this really isn't my picture of most psychology; they don't seem to me to exist in warring

camps at all. (4)Speaking for myself, I am one of the vast majority of unaggressive psychologists who see nothing much wrong with other people's points of view. [] (5)Now, for the purposes of the conference at least I am going to be playing the role of a radical behaviourist. (6)And one finds that when one is in this role, either voluntarily, or because others push one into it, they seem to have a conception of the role which I don't; it doesn't correspond with mine, not at all. (7)But they push me into this role of being a kind of psychological black, and it seems to me almost the victim of prejudice. [] (8)But I suggest then, to revert to my original topic, it is not so much that there is a war but there are a large number of people throwing things at what you call the mechanist camp as it exists at the present day. (9)And I would suggest, really, that most of the ammunition, the missiles - if I can indulge in the warlike metaphor - are really going in one direction.

Fish. (10)Well, you did have to come back in the end, though admittedly [inaudible]. (11)I don't take that too seriously but it is a convenient way of making it rather vivid and perhaps by talking in those terms one tries really to persuade people not to take it too seriously. (12)Because it is not, actually, a shooting war. (13)I don't really agree when you say that the missiles are only flying from one side. (14)That is a piece of propaganda for war which is not at all, well, there are plenty coming in the other way I must say. (transcript, 9-10).

At the begining of this extract Coleman expresses disagreement with Fish's characterisation of psychology as being split into 'warring camps', and he details the list of terms which he sees as sustaining the metaphor and thus as pernicious (1-3). Like Squire in extract 7, Coleman contests an attempt at relexicalisation. He claims that the 'vast majority' of psychologists see 'nothing much wrong' with those positions taken up by other psychologists (4). Fish's 'camps' metaphor is replaced by a picture of (mainly) harmonious tolerance; Coleman includes himself with this tolerant majority. He goes on to note that the role he will be adopting 'for the purposes of the conference' is that of a 'radical behaviourist' (5). However, he complains that often he is 'pushed into' this role, and even when he adopts it 'voluntarily' other people have a very different conception of the role to his own (6). Coleman's adoption of the category radical behaviourist is thus hedged with distancers in much the same way as Norton's use of the term 'mechanist' (extract 3 and 5) and Keefe's use of that term (extract 7). Furthermore, this different conception of the role of radical behaviourist approaches prejudice; he is treated, he complains, as a 'psychological black' (7). He thus characterises himself as a victim of unfair or misguided criticism which people apply because they have a distorted understanding of the meaning of 'radical behaviorism'.

Coleman then broadens his discussion from 'behaviourism' in particular to 'mechanism' in general. The attack is depicted as not merely on 'radical behaviourism' but on the 'mechanist camp' as a whole. He replaces Fish's metaphor of war with one of persecution: 'there are a large number of people throwing things at what you would call the mechanist camp' (8) and 'the missiles are really going in one direction'. (9). In this passage, therefore, Coleman adopts two radically contrasting criticisms of Fish's categorisation and analysis. He suggests, firstly, that psychologists are not organised into warring camps and so metaphors which depict conflict between such camps are inappropriate. Secondly, he claims that the 'mechanist camp', which includes 'radical behaviourists', are victims of 'prejudice' and have been attacked by 'missiles'. He thus attempts to undermine the descriptive form adopted by Fish <u>and</u> draw upon that descriptive form to deny certain of Fish's claims. Moreover, there is a further inconsistency with his attempt to suggest that the label 'radical behaviourist' is not his own and does not fit. For this is exactly the category he adopts when he tries to display Fish's intellectual prejudice.

In his reply, Fish initially appears to comment on the tension between these two sorts of criticism (10). Yet he too draws on the same competing ways of criticising Coleman. Fish suggests that the use of the warring metaphor should encourage people not to take the disputes too seriously - Fish seems to imply here that there is something obviously wrong or exaggerated about his military metaphor (11). And he also characterises Coleman's claims that 'mechanists' are victims of prejudice as 'propaganda for war' (14). Again, there is a move, in a very short time, between treating a certain form of words as rhetorical and taking them as literal.

I will return to this issue of mixing criticism and use of categories in the discussion. For the moment, the main point to note is that in this context Coleman treats his particular research area as a part of the 'mechanist camp'.

Later on in the conference he continues the theme of other people misunderstanding his research area while at the same time he addresses the problem of purpose and agency.

10. Coleman. (1)I think that still the sort of position I take is grievously misunderstood, and has to be put in front of people over and over again. (2)But also in the end I have discovered I have some sort of model so I want to come out and positively [inaudible]. (3)The things that I would want to say about what I would call operant psychology - I think behaviourism is a bit out of date - but operant psychology and the attitudes of operant psychologists are something like this. (4)That first of all most behaviour is operant behaviour. [](5)And most of the area I am interested in is operant. (6)Now, the first characteristic of this behaviour is that it is emitted; it is not jerked out of the organism; it is emitted. (7)And so it is voluntary behaviour.

Thomas. (8)Not necessarily.

Coleman. (9)Well, it is something like voluntary behaviour. [laughter] (10)It is not, we are certainly not talking about muscle twitches or reflexes in the psychologists' heads. (11)And the second point about it is this. (12)That the behaviour is defined in terms of its consequences. (13)In other words it is acts we are talking about. (14)Not muscle twitches. (15)Now the notion of muscle twitches came in not from any behaviourist at all, but from the critic McDougal: McDougal used this phrase, directed against Watson. (16)Watson used the term reflex, yes, and perhaps he shouldn't. (17)But you have to remember that the term reflex has been used in very many senses since it first came into psychology. (18)It is a very wide, a very wide term indeed. (19)So Watson perhaps shouldn't have used that, and perhaps he shouldn't have talked about stimulus and response. (20)But he did make it clear that when he said response he meant a bit of behaviour; he said, a bit of behaviour, something you can give a name to. (21)And then he gives examples like picking up a pencil from the desk, getting married, and so on. [much laughter] (22)Yes, that's one of his examples. [](23)They are [inaudible] defined or anything; in other words they are acts: the same sort of thing that all psychology is about and must be about. (24)A rat pressing a bar in a Skinner box is performing an act, which is defined by its consequences. (25)There is no doubt about it at all, about that. (26)The rat does something and we haven't prescribed exactly what is is at all. (27)And we don't care exactly what the behaviour is; the rat is, in a sense, is left free to do that.

(28)So we are all talking about the same thing, it seems to me. (transcript, 281-282)

Coleman starts this passage by returning to the theme of extract 9, that his position is '<u>grievously</u> misunderstood' (1). In what follows he tries to address some of these misunderstandings. This is prefaced by Coleman suggesting that the term 'behaviourism' is a bit out of date' (3). Instead he wishes to be known as an operant psychologist (3). The point of emphasising this terminological change becomes clear as Coleman directs his account to showing that his specific approach to psychology <u>can</u> deal with voluntary behaviour, and indeed is <u>based on</u> the analysis of voluntary behaviour.

Coleman claims that 'most behaviour is operant behaviour' (4) and most of his own interest is in this sort of behaviour (5). He characterises this sort of behaviour as 'emitted', which is contrasted with the idea of behaviour being 'jerked out of the organism' (6). Coleman then claims that this behaviour is 'voluntary' (7). Therefore, in sentences 4 to 7 he has equated 'operant behaviour' with 'emitted behaviour' and in turn with 'voluntary behaviour'. It seems that the change from 'behaviourism' to 'operant psychology' is meant to provide an indication of an increased emphasis on purposes at the expense of causes.

At this point Thomas (who as we saw in extract 4 suggests that the crucial difference between 'humanists' and 'mechanists' in psychology is that 'mechanists' cannot deal with purposes) intervenes and suggests that emitted behaviour is 'not necessarily' voluntary (8). Coleman's retort is to suggest that 'it is something like voluntary behaviour' (9) and he emphasises the contrast between the sort of behaviour he is talking about and 'muscle twitches' or 'reflexes'. He then returns to his theme and asserts that as the behaviour under study is 'defined in terms of its consequences' (12) the operant psychologist is studying 'acts' (13) not 'muscle twitches' (14).

Through the initial part of this extract Coleman has contrasted first 'operant behaviour', then 'emitted behaviour', 'voluntary behaviour', 'something like voluntary behaviour' and finally 'acts' with the terms 'muscle twitches' and 'reflexes'. Coleman now gives a further gloss on the two opposing notions: 'muscle twitches' and 'reflexes'. It seems that these terms have been applied to 'behaviourism' in the past, but incorrectly or innaccurately. It is clear, therefore, that he is engaged in reconstructing the categories used for depicting the subject matter of 'operant psychology'. He is attempting to eliminate the use of certain previously applied terms and to demonstrate the relevance of certain new terms.

In this sequence Coleman fashions a complex account to show that Thomas's picture of 'mechanistic' psychologists being unable to and unwilling to deal with voluntary behaviour is merely a rhetorical version. For he suggests that the <u>real</u> subject matter of (his area of) 'mechanist' psychology <u>is</u> voluntary behaviour and action. To achieve this account Coleman engages in a comprehensive attempt at relexicalisation. He replaces terms sequentially throughout the account, thus softening the contrast between the initial terms and their final replacements. The progressive sequence here is: 'behaviourism', 'operant psychology', 'emitted behaviour', 'voluntary behaviour' and finally 'acts'. The end product of this process appears to contradict the suggestion that 'mechnaists' do not deal with voluntary behaviour.

Coleman goes on to construct an historical account to show that while certain key terms which seem to conflict with the emphasis on agency have indeed been used, they should not be treated as literally applicable to Coleman's work. The notion of 'muscle twitches', he claims, was introduced by McDougal, who is not a 'behaviourist' at all, but a 'critic' (15). Coleman describes the notion of 'muscle twitches' as having been 'directed at Watson' (the 'founder' of behaviourism) by McDougal (15). Although 'muscle twitches' appears to be a descriptive phrase, Coleman's form of words treats it as abusive; syntactically, descriptions are 'about' while abuse or insult is 'directed at' as the present extract would have it. Moreover, displaying McDougal as a critic rather than a member allows Coleman to treat his terms as abusive in this way rather than merely descriptive; for it shows McDougal to have a specific <u>interest</u> in producing a non-neutral terminology[42].

Although the term 'muscle twitches' can be dealt with in this way the term 'reflexes' is a rather different problem because it was introduced by Watson (16). The difficulty here for Coleman is that he is involved in depicting modern 'behaviourism' as dealing with 'purposive behaviour' rather than 'reflexes'; yet the latter is exactly the term used by the founder of 'behaviourism'. Coleman resolves this interpretative problem by emphasising the vagueness of the term as it has been traditionally used in psychology. Although he himself has drawn upon it earlier (10), Coleman stresses 'it has been used in very *many*' ways in psychology (17) and that it is a 'very wide term indeed' (18). Because of these many possible senses, Coleman implies, Watson's use of the term 'reflex' must not be taken too literally. Despite some of its uses, the term can cover purposive behaviour.

Coleman indicates that it was also unfortunate that Watson used the terms 'stimulus' and 'response' (19). However, he suggests that what Watson really meant by the term 'response' was 'a bit of behaviour' (20). And such things can be named (20); for instance, 'picking up a pencil' and 'getting married' (21). This means that they are 'acts' (22). Coleman thus draws an equation between Watson's term 'response', the term 'behaviour', 'behaviour which can be named' and finally the notion of 'acts'. The force of this seems to be that although Watson used the term 'response', which according to Coleman he 'shouldn't have', what he was *really* talking about was 'acts'. And 'acts', as Coleman goes on to state, are 'the sort of thing that all psychology is about and must be about' (23). It is thus Watson's *language* which is at fault, not the *nature* of behaviourism. As in the earlier sections of the extract, Coleman's account is constructed using a progressive series of relexicalisations which smoothly shift it between causal and voluntaristic conceptions.

Coleman finally returns more explicitly to modern psychology and notes that when the rat in a 'Skinner Box' presses a bar (to get food, say) it performs an act (24). 'There is no doubt about it' (25) says Coleman, the rat 'does something' (26) which it is 'free to do' (27). And Coleman ends this sequence by concluding that in psychology 'we are all talking about the same thing' (27). In the end, therefore, the implication is that differences between psychologists have been exaggerated by other speakers and that all psychologists conceptualise their subject matter in fundamentally the same way.

In this account Coleman has stressed that his work takes purposive action as its central topic. Indeed, Coleman denies any interest in non-purposive topics. However, in extract 9, he has also indicated that his approach is broadly aligned with the 'mechanist camp', although he would not necessarily refer to it by that term. Yet, in extract 4, Thomas strongly argues that the essential feature of 'humanism' is its study of purposive behaviour and that this is 'utterly unacceptable' to 'any mechanist'. As before, then, with accounts of the testability of the camps, we see there is a significant degree of variability in the claims of different scientists[43].

Discussion: Categorisation, Evaluation and Reference

Let me return now to the metaphor of the sliced, multilayered cake which I introduced at the start of this chapter. While Kuhn's thesis of scientific paradigms is the classic cake account, it is clear that such accounts are not exclusive to the writings of meta-scientists. For instance extract 1 is one of two cake accounts which appear in the formal conference proceedings and are presented in papers at the conference. From a close examination of these accounts it is possible to formulate the basic features possessed by such social categorisations.

1) They refer to groups of people who share a particular set of beliefs and assumptions and engage in a similar set of scientific practices.
2) The beliefs and practices of the members form a coordinated whole.
3) This whole is constructed by combining a particular subject matter, methodological approach, form of explanation and set of basic theoretical assumptions.

When we come to examine in detail the passages of conference discussion in which 'cake categories' are used a rather different picture emerges. I will illustrate this by summarising in turn the analytic findings concerning the four distinctions between the 'mechanist camp' and the 'humanist camp' introduced in the initial cake account (extract 1 and 2).

1) Subject Matter. In extract 1 the speaker proposes that one of the differences between the 'mechanist camp' and the 'humanists' is that the 'mechanists' emphasise the study of animal behaviour. Yet in extract 5 another speaker suggests that this is not necessarily so. This speaker maintains that what subject matter is studied is not important for distinguishing the two forms of psychology.

2) Methods of Study. In extract 1 the speaker makes a distinction between objective experiments, which are used by members of the 'mechanist camp', and conceptual analysis and self report, which are used by the 'humanists'. In extract 3 a stronger account is given which suggests that only the 'mechanists' have methods which enable them to produce progressive science. This distinction is elaborated in extract 5 where 'mechanist psychology' is equated with the use of experimental methods, and it is also stressed that it is these particular methods which lead to scientific progress. While neither of these formulations are identical with that in extract 1, the equation of experimental methods with 'mechanistic psychology' in extract 5 comes close to it (the difference is only that the initial cake account does not tie progress to this distinction).

Extract 6 also draws upon a distinction between different methods, in this case distinguishing large scale experimental approaches from intensive studies with single subjects. Here, however, it is claimed that a process of theory testing, and thereby scientific progress, is possible for the 'humanist camp'. Furthermore, it is not experimental methods per se which are criticised, rather large scale, conventionally designed studies. Extracts 5 and 6, then, contradict each other: while extract 5 treats progress as given only by the methods of the 'mechanistic camp', extract 6 stresses that progress is produced by the alternative methods used by 'humanists'. Moreover, extract 6, in accepting the importance of some kinds of experimental methods, undermines the experimental/non-experimental distinction erected in extract 1.

3) Metaphysical Assumptions and Explanatory Form. In the formal cake accounts (extracts 1 and 2) the two camps are distinguished by the different sorts of explanations which they use: mechanistic in the 'mechanistic camp' and purposive in the 'humanist camp', and by the underlying metaphysical assumptions of 'plasticity' and 'autonomy' that go with them. In extract 4 this is treated as the essential feature which distinguishes the camps. For only 'humanists' adopt a scientific approach to human purposes. Yet, in extract 10, a scientist who adopts the label 'mechanist' suggests that his area of psychology is concerned specifically with purposive behaviour and that its explanatory form reflects this rather than trying to replace purposes with a more causally based form of explanation. This speaker finally suggests that the subject matter of all psychology is purposive behaviour or acts, thereby erasing the basic distinction proposed in extract 4.

To sum up: each of the distinctions between camps constructed firstly in the formal cake accounts and subsequently in accounts stressing crucial evaluative distinctions, are undermined or revised at some point in the conference discussion. The distinctions in terms of subject matter and metaphysical assumptions are straightforwardly contradicted. The distinction in terms of methods is broadly compatible with two further distinctions of this kind in the discussion. Yet these further distinctions contradict one another. Thus it seems that neither the formal cake accounts nor the specific accounts in the discussion can be taken as unproblematic literal descriptions of what it means to be a member of the 'mechanist' or 'humanist' camps. Although the categories of 'mechanism' and 'humanism' are frequently used by psychologists at this conference as they construct their accounts of their professional social world, the meaning of the categories appears to vary significantly from one occasion to another.

A similar problem is apparent in the way membership is ascribed to the camps. For instance, Carlisle is treated as a 'humanist' in extracts 5 and 7. And he himself at no point in the transcript queries this categorisation. Yet

Thomas suggests that his work does not satisfy the essential criterion of 'humanism', namely, that it should deal with purposive behaviour (extract 8). Indeed, Thomas claims that Carlisle has inverted the 'humanist tradition' by claiming that automatic behaviour is prior to purposive behaviour. Furthermore, to take a second example, Coleman aligns himself with the 'mechanist camp' (extract 9) and at the same time argues that his particular approach to research concentrates almost exclusively on voluntary behaviour (extract 10). Yet Thomas has claimed previously that it would be utterly unacceptable for any 'mechanist' to account for behaviour in this way. In each case the designation of membership conflicts with certain versions of the meaning of these categories. It seems, therefore, that neither accounts of the meaning of these categories nor particular participants' ascriptions of membership can be taken as straightforward descriptions of the state of action and belief in the discipline as a whole or even the specific conference.

One response to these findings might be that the variability in meaning and reference of these categories is not surprising and that analysts would certainly not adopt such categories when providing technically adequate divisions of the field. However, the method of comparing and examining in detail social categorisations produced on different occasions makes any variability present highly visible. When categorisations are used as a basis for erecting certain analytic claims variability is likely to be much less apparent. Using the work of Wynne[44] and Harwood[45] let me briefly show that there can be important similarities between analytic categories and the kinds of cake account discussed at the start of this chapter.

If we remember the discussion of Wynne's work in chapter 1, it is clear that a fundamental part of his analysis of the social determination of ether theory is the unambiguous identification of two categories of scientist: the Cambridge Physicists and the Scientific Naturalists. These sides are said not only to have different views about the physical world but also to differ in their broad philosophy of science, methods of study and general beliefs about society. In most respects, therefore, Wynne's categories are like standard cake accounts. The major difference from the psychologists' accounts is the inclusion of views about society into the coordinated set of beliefs and practices. Yet, as I demonstrated, a number of the specific accounts cited by Wynne conflict markedly with this discrete division. Some of the accounts stressed only theoretical differences between the groups, while others showed that certain key participants had beliefs compatible with both categories (see pages 34-36 above). Indeed the division seems to be sustained largely through the use of a number of 'homogenising devices', the most important of which is the repeated equation of institutional affiliation with shared belief. In this case, then, there appears to be a considerable degree of formal similarity between the participants' categories 'humanist' and 'mechanist' and Wynne's technical division into 'Cambridge Physicists' and 'Scientific Naturalists'[46]. Each specifies the existence of a coherent group of scientists possessing coordinated sets of beliefs and practices; but when discourse is examined closely it becomes apparent that these categories may be modified or even abandoned on specific occasions.

Harwood's work on the debate over the innateness of IQ differences is interesting here because the two sides in this debate are characterised in a way almost identical (in content as well as form) to the 'humanists' and 'mechanists' in the formal cake accounts. On the one hand are supporters of a hereditary position which is 'characteristically rationalist, quantitative, abstract, atomistic, and static' while on the other are critics and environmentalists whose position is 'intuitive, qualitative, concrete, holistic and dynamic'[47]. Harwood even refers to the hereditarian position as 'mechanist', although he reserves the term 'organismic' for the alternative. His central analytic goal is to account for the (social) origin of these two styles of thought[48]. As in the case of Wynne, Harwood's analysis depends on the unambiguous identification of two sides. There is not sufficient information available in his article to properly show the inadequacy of these catogories, or their origins, but it seems very likely that they are subject to the same flexible accounting as has been documented in the present study. It also seems likely that his technical categories originate in the discourse of participants. It is noticable that the terms 'environmentalists' and 'hereditarians' are introduced as participants' categories, but as the analysis proceeds they become increasingly incorporated as taken-for-granted analytic resources.

These two examples suggest, therefore, that analysts have adopted as adequate for technical purposes categories very similar to the ones discussed here. Indeed, these seem to be exactly the kinds of categories that are drawn on by researchers whose work is based on the qualitative reconstruction of historical debates (Wynne, Harwood) or participant observation (Collins and Pinch[49]). Perhaps this similarity is not surprising as cake accounts are very like Kuhnian paradigms, and Kuhn's work has had a powerful influence on contemporary social studies of science. Nevertheless, the present study suggests such categories should be used with considerably more caution than in the work of these authors.

One feature shared by these analytic approaches is that their categorisations are viewed as essentially descriptive rather than evaluative. Put another way, the identification of collectivities is treated as entirely separate from judgments about the relative merits of different positions. The latter activity is unacceptable to those who accept the Strong Programme's tenets of impartiality and symmetry, or who espouse relativist principles; whilst the former activity is a crucial and often largely unproblematic preliminary to conducting analyses. However, there is no possibility of producing a clear cut division of this sort in the present analysis. In many instances the distinction 'humanist'/'mechanist' is intimately associated with the construction of accounts which assign different scientific values to these categories of psychologist.

It is important to be clear about what is being suggested here. The aim is not to replace one interpretation of these categories - that they are literal description - with its converse - that they are simply rhetorical glosses. Perhaps the best way of putting it is to say that scientists construct the social categories in their field in accordance with their other scientific claims and occasioned to fit the requirements of specific interpretative contexts. To see them in terms of a polarisation between strategic and literal discourse would be to adopt an unwarranted participants' classification.

This emphasis on the enmeshing of description and evaluation is clearly illustrated by the contrast between the formal cake accounts in the published proceedings (extract 1) and accounts 3 and 4 from the transcript. While formal cake accounts formulate a number of ways in which 'mechanism' is distinct from 'humanism', accounts generated during the conference tended to concentrate attention on single issues: the methods of theory testing used and the progress they provide; the ability to deal with purposive behaviour. Each speaker claims one of these issues to be crucial for the division into camps. Although different in content, these issues are each used in the same way: to highlight what one camp is taken to lack with respect to the other. They are descriptions used in the process of evaluation and constructed accordingly. Thus Norton (extract 3) depicts the 'humanist camp' as lacking any way of clearly testing theories and therefore as unable to produce a progressive science; while Thomas (extract 4) depicts the 'mechanist camp' as lacking any way of dealing with purposive behaviour scientifically. By focussing on particular 'crucial' issues, rather than an array of different issues which distinguish the groups, a stronger evaluative form can be achieved. To claim that 'X is crucial and this camp lacks X' is more damaging than claiming 'X,Y and Z are all important, and this camp lacks X'. In the latter case the force of the account can be undermined by emphasising 'Y and Z' which the camp does not lack; while in the former case this response is made more difficult.

In each of extracts 3 and 4 the evaluative force is increased by displaying one of the camps as scientific in a way the other is not. Thus the 'mechanist camp' is depicted as able to produce long lasting empirical generalisations like those of the natural sciences; whilst the 'humanist camp' is unable to do this (extract 3). In contrast, extract 4 shows the 'humanist camp' as able to take a 'naturalistic', 'scientific' view of human purposes which the 'mechanists' are unable to do. It appears that if the speakers able to make the distinction 'mechanist'/ 'humanist' collapse into the distinction scientific/non-scientific (or non-scientific/scientific) this will provide a powerful legitimation of one camp with respect to the other. The category 'science' is used as a basic legitimating term. Once these speakers have shown their camp to be scientific no further interpretative work is seen to be required.

So far, then, I have shown that although in the formal papers accounts appear which divide psychologists into two coherent camps, and that this camp metaphor is at times reiterated in the discussion, the meaning and membership of these camps is variably accounted for. I have also shown that in the discussion these accounts are particularly involved with displaying the relative success or coherence of one camp with respect to another. Furthermore, I identified two accounting techniques for legitimating the work of one or other camp. These were to stress a single evaluative distinction as the crucial difference between them and to identify one camp as properly scientific with respect to the other. In the analytic sections of this chapter I discussed two further accounting techniques. One of these involved the 'preformulation' of competing positions to undermine them in advance of their use. This will be discussed in greater detail in the next chapter. The other involved the process of 'relexicalisation', where certain kinds of contrasts and distinctions are modified and transformed by the progressive substitution of terms. This process was apparent in a number of the extracts, although on some occasions attempted relexicalisations were contested (see extract 7, sentences 2-3, extract 10, sentence 8).

Before moving on to address the issue of social categories in more general terms, one final accounting technique should be noted. In each of the passages which stressed the inadequacy of the 'humanist' with respect to the 'mechanist camp' (extract 3, 5, 7 and 9) the term 'mechanist is displayed as problematic in some way. This is typically done either by prefacing the term with a 'distancer', such as 'the so called mechanist camp' or 'what you would call the mechanist camp', or by relexicalising: replacing 'mechanist' with 'scientific', 'experimental' or 'biological'. Across the entire corpus of categorisation accounts there is no construction which positively values the 'mechanist camp' without treating the term as problematic in one way or another. In extracts arguing for the superiority of the 'humanist camp', however, the use of such techniques is very rare. What could explain this asymmetry?

One plausible explanation is suggested if we return to extract 4, the account which identifies the crucial distinction between camps as the inability of 'mechanism' to deal with purposes. For here the term 'mechanistic' appears to be treated as synonymous with non-purposive. On the other hand, in extract three there is no suggestion that the term 'humanist' means non-testable, it is simply a label for scientists whose work is (it is claimed) non-testable. It thus appears that the terms themselves more effectively mark the positive value of the 'humanist camp' than the reverse. Adopting these terms, then, may be to begin to adopt the evaluation they mark; and as a consequence, in certain cases they must be made problematic by the use of distancers or by relexicalisation[50].

Returning now to the general issue of social categorisation and consensus, we can see that there is a crucial by product of the form of talk which has been discussed. The participants continually formulate and reformulate the meaning of the categories, and what they should be called, in the context of producing specific evaluations. This process continually presupposes the successful reference of the categories even while their meaning is disputed; that is, it is presupposed that this talk is about certain specific groups of scientists and that the terms 'humanist' and 'mechanist', even if not always appropriate, are names for unproblematic groupings of psychologists. One reason for this appears to be that it is necessary to assume the categories when fashioning an evaluation. If the membership of the categories cannot be distinguished there is no basis for making an evaluative distinction. The very structure of this talk, therefore, leads to the social categories being reified.

Evaluative categorisations could be undermined simply by questioning the existence of the categories being discussed. But for any particular scientist's work this would not offer any response to the content of the criticism; it would only suggest its form was inappropriate. In may thus be more straightforward for participants to (tacitly) accept the idea that there exist two homogeneous groups of psychologists so that the specific evaluative criticism can be addressed. This sort of difficulty may explain the form of accounting seen in extract 9. There the 'camps' metaphor is characterised as an inappropriate way of describing psychology. Yet very shortly afterwards, when trying to show that a specific criticism of his own position is not justified, the speaker treats the 'camps' metaphor as indicating a genuine difference between groups of psychologists. Thus even where a speaker explicitly undermines the basis for making the division into camps, it is reintroduced to respond to the content of criticism.

While these social categories tend to be reified in this way, as literal descriptions of psychological subgroupings, their actual reference is left extremely vague. Very few contemporary participants are identified as members of one camp or another. Furthermore, there is often ambiguity over whether the accounts are meant to be descriptions of the actual states of (contingently existing) affairs or illustrations of the necessity for, say, certain camps to use certain methods. For instance, in extract 5, while one speaker emphasises that the categories describe 'how things are' the other claims that his concern is with what 'must be the case'. On the one hand, it is implied that the constellations of beliefs and practices adopted in the camps of psychologists are just one of many possible constellations; on the other, it is implied that these constellations have properties such that certain methods, say, will necessarily combine with certain metaphysical assumptions. A third area of vagueness is over whether the camps consist of groups of members or systems of beliefs and concepts. This difference is marked by the difference between 'X<u>ism</u>' and 'X<u>ist</u>'. In many cases speakers moved fluently between these two constructions as if there were no important differences between them[51].

As a product of this vagueness there is only a very low likelihood of severe interpretative difficulties arising over the reference of these category terms. The vagueness introduces a flexibility which facilitates the repair of any apparent conflict over reference. It seems likely, therefore, that these categories will continue to be available as viable resources used in the construction of evaluations despite the sorts of problems over their meaning and membership outlined at the start of this discussion.

Finally, let me briefly return to the issue of formal cake accounts. I have already argued that they cannot be treated as literal description of consensual scientific categories. But this raises two further questions. How are they constructed? And what is their function? Neither of these questions can be answered definitively through the present analysis. However, it is possible to make some suggestions for further, more detailed study.

It seems likely that formal cakes are produced by combining together various evaluative distinctions made in different kinds of participants' discourse. The presuppositions which are made in each particular dispute - that there exist certain coherent, orderly subgroups in psychology - are taken as literal and used as grounds for treating them as markers of a boundary between two broad social categories. At the same time, many of the evaluative overtones are stripped away so that the final cake account treats, or apparently treats, each cake grouping symmetrically. Formal cakes are thus constructed through a process of reifying categories that are used by participants' themselves as practical evaluative resources in certain varied forms of discourse.

Why should such cakes be produced? Both cakes presented in formal papers appeared to depict 'mechanism' and 'humanism' symmetrically, as both equally valued. Nevertheless, the categories were involved in evaluative accounts. For in each paper both cakes were contrasted with a third alternative. In one the products of 'mechanism' and 'humanism' on their own were depicted as inferior to the product of their interaction. Only by an exchange of ideas and methods, the paper suggests, will psychology realise its progressive potential. In the other paper differences between the camps were depicted as relatively unimportant because they have little influence on the practical utilisation of psychology. Arguing about the nature of camps is here a scholastic irrelevance. In both cases the two camps are together evaluatively contrasted with a third option. So there is some tentative indication that formal cake accounts may have specific stipulative functions, although not necessarily the same ones as the informal evaluative distinctions.

It seems, overall, that the claims of Peterson and Weimer to be able to detect partisan commitments underlying the discussion of paradigms in psychology should be extended to other kinds of social categorisation as well. In formal cake accounts there is some evidence of stipulative use, while many informal classifications accompanied strongly evaluative claims. Because of the intimate relationship between evaluation, beliefs and social categorisation it is likely that it will prove difficult if not impossible to produce a truly neutral social categorisation which attempts to classify scientists according to their beliefs and actions. For in every case that content is given to particular consensual groupings certain achievements and limits are implied: theory A can explain Y but not X; a class of observations, O, cannot be dealt with by framework B; method P is satisfactory for all problems Q to R. Studies which have looked closely at the variety of different beliefs expressed by scientists show how unlikely it is that there will be consensual agreement on these issues. It seems probable that analysts in the Kuhnian tradition, who strive after the delineation of paradigms and categories of this kind, are inevitably, if often implicitly, going to become embroiled in the practical problems which face scientists when ordering their social worlds in ways that are best for them.

CHAPTER SEVEN

READING READINGS

In this chapter I will continue the emphasis of the last on explicating the detailed interpretative procedures used by psychologists in the construction of their discourse. However, I will move away from the concern with notions such as criteria for theory choice and social categorisation which are central to traditional social research on science. Instead the analysis will concentrate on the topic of reading. It will address the question of how participants make sense of and account for spoken and written discourse. This issue has had little interest for researchers concerned to give causal accounts of the nature of scientific belief or for sociologists whose aim is to show the lack of impact the 'natural world' has on theory selection. It is only when we start to consider a systematic analysis of scientific discourse that the central importance of this question becomes clear. For work from this perspective has documented the complex and heterogeneous nature of scientific texts[1] and shown that divergent systems of concepts may be used to account for scientific action and belief[2]. Given these findings, which show that texts and spoken discourse are far from being a transparent and straightforward medium of communication, the question arises of what practical procedures scientists use to deal with texts and to respond to spoken discourse, tasks which form a large part of their professional lives.

Reading Transcript

As a prelude to an analysis of participants' 'readings' it is important to clarify what will be meant by this term. The notion of reading will be used in the restricted sense of recent literary theorists. The analysis will not be concerned with revealing some elusive inner experience of the text had by a reader. Insofar as participants are explicating, interpreting and commenting upon written texts they are publically displaying techniques of sense making in the production of versions of texts' meaning. It is these versions which I shall call 'readings'. There is probably no hard divide between readings of this kind and the more general interpretative practices which are discussed in the other chapters of this thesis. In each case what is available for analysis are sequences of texts. It is convention only that we call one text in the sequence which appears to refer back to another a reading of that other text. We might treat a discussant's formulations concerning a paper presented earlier as embodying a reading of that paper. Yet it can be argued that <u>all</u> texts have to refer back to others as the very condition of their making sense; it is just that most of the time these all-pervasive citations go unacknowledged or are so 'obvious' that they are not explicitly alluded to. This point will be elaborated in the discussion section of this chapter.

The specific materials for analysis were chosen with this restricted notion of reading in mind. In these data recurrent and explicit reference is made to particular and identifiable anterior texts. It is for this reason alone that I will refer to them as 'readings'. Two main sets of data are drawn on. The first consists of 34 pages of transcribed discussion between a group of 11 Personal Construct Psychologists at a residential workshop at a South Coast seaside resort (see pages 78). This discussion concerned some transcribed material which had been circulated to all the participants before the workshop. The precirculated transcripts were accompanied by a letter (figure 1) suggesting that participants might like to read them in preparation for a discussion. At the workshop session at which the materials were discussed they were introduced as follows:

> My fantasy of what will happen this morning is a bit like what my workmates and I do at York, which is to sit round a table with chunks of speech. And people say 'well, I thought what was going on on page three was rather strange, interesting or so and so', 'I thought such and such was happening'; or 'I was curious about what this was'...
> I mean, I don't particularly want to start off by going through, or saying things which I think are going on, mainly because I have been through these things extremely briefly; probably no more more than you have, apart from having typed them out some years

Figure 1: Letter accompanying precirculated transcripts

no. PS/EP

6 October

Dear

I enclose an item for our PCT workshop. It consists of two extracts (pp. 6, 2, 15-6, 1 (i), II and 8, 2, 13-8, 2, 21) from the transcripts of our previous meeting. It has been suggested that we might use them as material for a discussion at some point. So do read them through beforehand if you can.

I know that I have taken a bit of a risk in distributing attributable dialogue in this way. But it seemed to take some of the life out to remove people's names. I trusted in the good spirit of the meeting that nobody would be too grieved.

It's interesting though to reflect on the difference between saying something momentarily to someone, and having it recorded for scrutiny at leisure.

Best wishes,

ago. But afterwards I might try and explain a bit more the perspective I might take, if people would be interested in that. (Jonathan, transcript, 1-2)

This introduction was deliberately as nondirective as is compatible with it being an introduction at all. Its aim was merely to initiate discussion concerning the transcripts, not to influence its direction.

Clearly this data is not 'naturalistic', in the sense that the participants are engaged in an activity structured by the researcher which is not part of their everyday conference activities. However, through using this artificially constrained situation it was hoped to elucidate the 'competence' of the participants; i.e. it was hoped to reveal the sort of readings that participants could make. Although further work would of course be needed to show when these sorts of readings would be used in practical contexts.

The transcription material which provided the topic for this discussion was taken from another workshop of Personal Construct Psychologists, which had taken place some 18 months earlier (see pages 77-78). Indeed a number of the participants were common to both workshops: Dennis, Ian, Jonathan, Janthia, Mike, Neil and Richard. Some participants, however, were at the second workshop but not at the first: Anne, Frank, Chris, Shirley; and vice verse: Alan, Carol, Sue, Grant, James. The two extracts from this earlier workshop were selected because they contained what appeared to be fairly self contained disputes about specific issues. The analytic interest of this exercise, however, in no way depends on these selection criteria being accurate. In the event virtually the entire discussion was devoted to one 14 page transcript out of the two. Accordingly the discussion of the other transcript will be ignored here.

Because of its embedded nature - the transcripts record participants discussing themselves as they appear in a transcript - the following exposition becomes convoluted in places. To some extent this is unavoidable, but to avoid undue confusion I will refer to the precirculated transcript as 'the transcript' and the recorded discussion of that transcript as 'the discussion'. In addition, participants speaking in the transcript will be given the superscript 1. So, for example, 'Richard interprets Richard[1] as saying X' will mean that in the discussion Richard interprets his previously transcribed utterances to be saying X. Before going on to the analysis proper I will briefly examine the way reading has been dealt with in modern literary theory.

Reading Resources

A central theme in the style of discourse analysis which has emerged on the Continent, particularly in the work of Roland Barthes, is that literary texts should not be seen as direct representations or mere causal products of some extra-linguistic entity. I intend to suggest that this argument has some relevance to the present analysis, so I will run the risk of over simplifying some highly complex sets of ideas and try to outline it.

Traditionally, Barthes argues, both analysts (literary critics and sociologists of literature) and lay people have wanted to explain the existence and form of discourse (texts) by reference to one of three 'sites' or entities existing 'beyond' the text: the author, the world and ideology. These sites, or resources, are used to produce definitive interpretations of the text and close off possible competing interpretations.

In the first case, although literary critics have persistently tried to use the intentions or proposals of authors as a way of producing definitive readings of texts, the attempt is doomed to failure. This is not merely due to the pervasive and incorrigible problem of deciding what the author's 'real' intentions are. It is due to the possibility, inherent in all texts, of reading them in different ways. To claim that only those ways 'authorised' by writer's intentions are acceptable is, Barthes argues, a moral or ideological claim; for non authorised readings are perpetually available with any text. This is not to say that versions of author's intentions or psychological makeup are not interesting; rather that such versions can have no <u>special</u> legitimating function. They do not 'close off' the possibility of alternative readings. As Barthes puts it:

> It is not that the Author may not 'come back' in the Text, in his text, but he then does so as a 'guest'. If he is a novelist, he is inscribed in the novel like one of his characters, figured in the carpet; no longer privileged... He becomes, as it were, a paper-author: his life is no longer the origin of his fictions but a fiction contributing to his work... (3)

This is, of course, fully compatible with Barthes's general enterprise of producing a semiology of reading practices to replace the flawed interpretative 'science' of criticism which is concerned with the goal of producing definitive readings of texts[4].

The argument about the role of ideology is very similar. Barthes rejects the position, which is represented in his own early work as well as in other places[5], that readings are determined by ideology. For although ideology may be implicated in the formation of texts it cannot determine the way they are read. Take, for instance, Barthes's classic example of the Paris Match cover he sees while visiting the barbers. This shows a young negro in French army uniform saluting the French flag. Barthes describes what the cover signifies:

> that France is a great Empire, that all her sons, without any colour discrimination faithfully serve under her flag, and that there is no better answer to the detractors of an alleged colonialism than the zeal shown by this Negro in serving his so-called oppressors. (6)

Yet Barthes's own reading is a deconstruction of the text; it displays the operation of ideological processes and is certainly not a passive victim of these processes. In response to problems such as these in his later work Barthes argues that ideological processes influence the way texts work to construct the world, but do not prevent alternative readings such as his own. Another way of putting this is to say that readers are not passive victims of the world views expressed in texts but make a much more active contribution to the reading process.

Barthes's argument about the relation between texts and 'the world' and the way 'the world' is used to warrant particular readings, is central to his total semiological perspective. It is essential to note, however, that he is not attempting to solve one of the basic problems of Western philosophy; his is not an epistemological argument for anti-realism. Rather, he is concerned with processes of sense making in complex texts and the attempt by literary critics to read texts as literal descriptions of a (sometimes imaginary) world. The most sustained critique of this view is found in his book S/Z where he takes an apparently classic realist text by Balzac and attempts to elucidate how it makes sense to the reader by splitting the story into 561 fragments ('lexias') and showing how each functions in the complete text[7]. In particular, Barthes undermines the view that the text's sense is derived through process of description and denotation. Instead, he argues, the text aquires its meaning through the reader bringing to it 5 cultural codes or accounting systems which embody an organised corpus of background knowledge concerning narrative, theme, character, cultural sociology and symbolism. The text cannot therefore be seen to have a single definitive meaning independently of the specific readings made by particular readers.

A simple example may make what Barthes is suggesting clearer. The sentence 'Midnight had just sounded from the clock of the Elysee-Bourbon' appears right at the begining of Balzac's text. Barthes suggests that its significance derives not from what it denotes but from what it connotes. For readers with the appropriate background knowledge will know that the Elysee-Bourbon is in a wealthy neighbourhood of Paris (Faubourg Saint-Honore) and furthermore that this wealth is held by the nouveaux riches who acquired it through speculation and similar means. Thus the literal, denotative meaning of this sentence is rather unimportant and could be replaced by many different sentences. What is important is that this sentence (or any possible replacement) conveys through its connotations, the information about wealth which is structurally central for making sense of the text. So when Barthes writes:

> denotation is not the first meaning, but pretends to be so; under this illusion, it is ultimately no more than the last of the connotations (the one that seems both to establish and to close the reading) (8)

he is stressing that the sense of the text is constituted through the interpretative systems the reader brings to it and, moreover, claiming that this sense is mistakenly seen to arise from the text's literal description of the world[9].

Overall, then Barthes attacks the idea that the sense of a text is an unproblematic consequence of either the intentions of the author, the ideological processes that may have influenced the text's construction, or of the world that the text is taken to represent. In each case he changes perspective and treats these ideas as worthy of explanation in their own right. In more usual sociological terms, he takes certain resources commonly drawn on by literary critics and readers for making readings and treats them as interpretative procedures to be analysed, as topics for study[10]. Thus, although he sees authors' intentions, and ideology, as not being grounds for definitive textual exegeses, he nonetheless sees the study of such attempts as illuminating; and instead of treating 'realism' as a product of acute and literal description he treats it as a linguistic effect. The traditional question of how accurately a text describes is therefore replaced by a more coherent analytic question which asks how the the organisation of discourse within a text achieves the <u>effect</u> of merely describing[11].

Resources and Repertoires

If we return now to this chapter's theme of scientists' approaches to their own discourse, we can see the relevance of Barthes's argument. It suggests the pertinence of the following questions: do scientists draw on certain resources to achieve and sanction readings? And if they do what might these resources be? In other words, what are the similarities between the reading practices of scientists and the reading practices which Bathes identifies as typical of traditional literary critics. To make these questions even more specific I will concentrate on the way readings are produced by using the 'empiricist' and 'contingent' repertoires.

In chapters 3, 4 and 5 I have examined the way these accounting systems or 'registers'[12] are used for characterising actions and beliefs. In chapter 3 I showed the way in which a social psychologist is able to characterise his applied work either in terms of a standard empiricist model of theory application or in a way which stresses more contingent processes. In chapter 5 the ways scientists' characterised the role of criteria in theory choice were examined. It was found that they were more likely to treat criteria as determinate and clear-cut when describing their own theory choices; but when describing those of their competitors they would more often treat criteria as socially contingent and open to strategic manipulation. Furthermore, the use of these systems of accounting for action and belief has been extensively documented in the work of Gilbert and Mulkay[13]. These repertoires have been described in detail in chapter 3 (pages 92-93). Briefly, the empiricist repertoire corresponds roughly to traditional conceptions of scientific rationality. Data are seen as arrived at by way of standardised impersonal routines and are taken to provide a clear-cut criterion for selecting theories. The contingent repertoire recognises the importance of a variety of 'social' influences and takes facts to be dependent on fallible interpretative procedures.

What has not so far been examined is the way these accounting systems are utilised by scientists for dealing directly with discourse. Such utilisation is implied in some of the studies that have examined the use of these systems; for scientists do not have unrestricted and persistent access to each other's lives and workplaces, and even if they did their understanding would be textually mediated. Thus scientific texts will inevitably play a significant role. Yet for the most part scientists' accounts of actions and beliefs take the form of (more or less) direct descriptions of these things. They seldom make reference to any textual mediation in arriving at such descriptions. Textual sources which may have played a part in the construction of these accounts are rarely displayed in the account itself. By analysing transcripts in which participants are discussing in detail certain discursive materials it is hoped that some light may be thrown on the interpretative procedures used to construct readings.

In the examples that follow the sorts of stylistic differences between repertoires documented by Gilbert and Mulkay are unlikely to become apparent. For instance they show a specifically impersonal form of reportage of empiricist actions to be common in scientific research papers[14].

The differences I will be examining are predominantly lexical, and centre on the alternative ways that the transcript can be seen as revealing the actions and beliefs of the conference participants. Psychologists at the workshop may read a particular section of transcript as revealing actions which are, for instance, concerned with the disinterested development of theory. It is in this sense that I will speak of 'empiricist readings'. Alternatively a section of transcript may be seen to embody actions which are orientated towards more personal, 'interested' goals rather than neutral scientific ones. Thus it is not the <u>form</u> of the reading itself which is contingent or empiricist, but the <u>content</u> of its interpretation of the transcript. The text is read either <u>as</u> empiricist or <u>as</u> contingent.

Now it is clear that not all accounts describing the text in personal or social terms can be called contingent, as in certain situations personal or social processes may be quite separate from any issues of scientific relevance. For instance, if there is a pause in the discussion for an argument about seating positions this might be totally separate from scientific questions of any kind and should not therefore be classed as contingent. Yet perhaps such an argument <u>was</u> a struggle for psychological supremacy which would influence the outcome of the next hour's theoretical debate. How would the analyst decide? As a resolution of this difficulty I have chosen to examine accounts in which participants characterise the same section of transcript in contrasting ways, and in particular where some give an empiricist reading and others a contingent reading. In this chapter, therefore, I will use the term contingent in the restricted sense of those accounts which <u>re</u>characterise empiricist versions of actions and beliefs using terminology from the contingent repertoire.

<u>Participants' Readings</u>

To start with I will examine a relatively straighforward empiricist reading of part of the precirculated transcript. In it the speaker is responding to two earlier speakers who have identified certain themes in the transcript. One of these themes concerns the nature of the processes actually occurring in the group, andthe other concerns the ideal group which would be implied by Construct Theory (a 'Kellian group'). The speaker, Mike, takes the previous speaker to be seeing these things as equivalent, and disagrees with this equation.

1 Mike. (1)Um, I think that these two things are different and they are playing out against each other. (2)I mean we are using the group to think about the nature of a Kelly group, and then we are [laughs] using the nature of a Kelly group to think about how the group is operating. (3)I mean they are, I thought it was working - I mean, reading it - it works quite nicely. (4)We are both using the direct shared experience of everybody who has been there and yet trying to produce a more generalised view of what, what Kelly thinking would do to group dynamics. (Discussion, 8-9)

In sentence 1 Mike formulates the two themes - the processes occurring in the group and the nature of a Kellian group - as separate and suggests that there is a tension between them. This tension, however, is positive: 'it works quite nicely' (3). Each is being used to throw light on the other: the participants' 'shared experience' of the group is being used as a resource for explicating the nature of a Kellian group and their theoretical understanding of what a Kellian group would be like is used to elucidate their specific group interaction (2 and 4). In this extract, therefore, the speaker treats the transcript as revealing what is going on - the acts and actions - in the previous workshop. Three particular classes of activities are identified: A) 'Using' - the actual group, a hypothetical notion of a group; B) 'Thinking' - about the nature of a Kellian group, about the activities of the particular group; C) 'Producing' - a more generalised idea of the application of construct theory to group dynamics. In addition the transcript is described in more general terms as showing the activities in A and B successfully 'playing out against each other' (1). Overall, then, this speaker distinguishes between the theme concerning social processes occurring in the group and the theme concerning the content of the group's discussions and he sees a successful and reflexive interaction to be taking place between them.

Furthermore, it is interesting to note that at the point where the speaker appears to be making a direct evaluation of the group's success he corrects himself and emphasises that his understanding is derived from reading the transcript (3). In a preliminary way this speaker formulates a 'gap' between the transcript and the actual actions of participants. He thereby starts to raise the possibility that the actual actions might be other than they appear from the transcript, and in so doing indicates the fallibility of his interpretation. In later extracts we will see this point raised in a more explicit fashion.

In the next passage the interpretation of the transcript is more complex. Again the speaker characterises the actions which the transcript reveals happening at the previous workshop. This time, however, two different versions of these actions are formulated. The transcript is described as explicitly saying one thing which hides a more significant implicit message.

2 Dennis. (1)Yes, I mean, reading it again I got a sense that some of the tension in it is to do with, er, [laughs] to do with the way in a sense you can cheat slightly and we do. (2)That is you can, you can be actually making a remark about the behaviour ofother people in the group, in the sense of not wanting it, or regretting its being there, but you can frame that into saying that an ideal Kellian group would be like this. (3)And the unsaid part is: 'but not like what you've been'. (4)And a couple of times you, and I think I, and I don't know if anyone else as well, referred to other groups we had been in. (5)Er, and that was quite interesting, because it sort of again it [Mike laughs] was said 'isn't it interesting: I have been in a group that works like this'. (6)But you could see behind that the possibility of saying: 'and why haven't you buggers!'

Mike. (7)[laughs] Exactly, yes.

Dennis. (8)I mean in my group I specifically said that we were very good at rapidly throwing in experiments, little formalisations, which everybody picked up and role'd through; unlike say Mike Davies who opts out. (9)[Mike laughs] (Discussion, 9-10)

The first speaker in this passage, Dennis, starts by alluding to the 'tension' mentioned by the previous speaker (extract 1). However, in this case tension is given a rather different meaning. The previous speaker treats tension as a creative product of the theoretical analysis and the group process being used to illuminate each other. In contrast, Dennis treats tension as a consequence of the fact that participants can and do 'cheat slightly' (1).

This cheating consists of using points which appear to be to do with the nature of Kellian groups to make points which are actually critical comments on the behaviour of other group members (2). These implicit points are characterised as a sort of hidden speech, or subtext. Thus a comment that 'an ideal Kellian group would be such and such' is depicted as conveying the implicit message: 'but not like what you've just been' (3). In all Dennis gives three different examples to illustrate this dichotomy between what is actually said and what is apparently said (sentences 2 and 3, 5 and 6, and 8). In each case, the ostensive meaning is given along with a translation which provides the real meaning.

This technique is similar to that of 'preformulation' discussed in the previous chapter (page 191). In this case what is preformulated is an incorrect reading of the text. Den formulates two versions of the transcript being discussed. One is what it appears to say and the other is what it really says. The real message of the speech is seen as implicit. Yet it is clearly thought to be understood by at least some of the other participants - Dennis is not describing a solipsistic joke on his part. For convenience I will refer to this discursive technique as the R/A (reality/appearance) device.

Towards the end of the extract Mike, the speaker in extract 1, expresses support for Dennis's claims (7). Yet, although this support might be taken to imply that his points and Den's are the same, as I have noted the two accounts are differently organised. Mike suggests that the participants constructively use an understanding of the actual group processes and the notion of an ideal Kellian group to illuminate each other and promote theory development. That is, he interprets the text as a document of empiricist actions. Dennis, in contrast, implies that what appear to be instances where the theoretical discussion of ideal groups is used to throw light on the actual group's interaction, and vice verse, are in fact veiled contributions to the actual group's interaction. Put another way, he claims that what appear to be dispassionate comments on the interaction are actually partisan and critical contributions to the interaction itself. It is not merely claimed that one theme is used as a way of making points relevant to the other; Dennis's point is that one theme is being used to make a contribution to the processes which are the topic of the other theme. He interprets the text as revealing contingent actions: certain participants in the transcript are taken to be strategically couching points of personal dispute in the form of disinterested theoretical comment. In this way Dennis formulates two possible readings: an empiricist reading of what the text appears to say and a contingent reading which reveals what it really says.

As I have noted the main aim of this chapter is to elucidate the organisation of certain kinds of readings. The details of the relationship between the readings and the transcript which is read are of only secondary concern. Nevertheless, it is important to show (however unlikely it might be) that participants are not simply reproducing the transcript in some mechanical fashion; i.e. that 'reading' as a constructive and active phenomenon is a viable topic for analysis. I will thus examine the section of transcript on which Dennis's reading is based. In extract 2 he does not explicate all the exact parts of the transcript to which he is referring. Nevertheless, it is clear that the following passage is one of them.

3 Dennis[1]. (1)I have worked regularly in a group that meets fortnightly, and one thing[] I have noticed is that we are developing and getting very adept at - and it is to do with, I think, essentially with the notion of people's experiments - is quite quickly as it were one or other will think of a form for a quick experiment, you know. (2)And it can be thrown in, and there almost now seems to be an agreement that you never resist a form, you know, even if that doesn't particularly [Mike[1]. Um, um, um.] attract you; that is not an issue.

Mike[1]. (3)I think that is interesting, because I think the group made a decision not to go along with that form. (4)I mean, I consciously had decided I was not going to name my form, [Dennis[1]. Yes.] yesterday evening [laughs]. (Transcript, 223-224)

In sentences 1 and 2 Dennis[1] characterises some of the features of a group he regularly attends. This appears to be the reference of sentence 4, extract 2. He particularly emphasises that group members willingly took part in any interpersonal experiments suggested by other members, even if they were not particularly interested in them (2). In his reply, Mike[1] suggests that their present group decided not to act along those lines (3). And, referring back to an 'experiment' which Dennis[1] had suggested, but which had not been completed, Mike[1] notes that he had decided that he was not going to take part (4).

It seems clear, therefore, that Mike[1] treats Dennis's[1] description of the way 'experiments' are accepted in his group as related to a particular instance in which the present group decided not to take part in an experiment. Yet there is nothing in Mike's[1] reply which forces us to read it as a response to implicit criticism rather than merely a further explication of the differences between the present group and the one Dennis[1] takes part in; that is, there is no obvious feature of the transcript which suggests that Mike's[1] point is any more than a contribution to the theme of what group processes are actually occurring. Mike[1] draws on an apparently neutral descriptive terminology without recourse to terms which explicitly display criticism, apology or anger. Thus we cannot use Mike's reply to sanction Dennis's interpretation of his own utterance (extract 2).

It is of course possible that Dennis uses the fact that a dispute is not displayed in Mike's[1] reply as a ground for reading the personal/critical force of his point as implicit rahter than explicit. In this case Dennis's imputation of implicit force to his utterance could be seen as an accounting device, which deals with the troubling fact that Mike's[1] response does not appear to treat Dennis's[1] point in a way appropriate to the gloss which Dennis wishes to give it. Yet, as Dennis himself makes plain, his own speech (extract 3) also appears neutral and disinterested. Dennis is therefore reading as contingent a speech which he accepts appears to be empiricist and, moreover, which a subsequent speaker appears to take as empiricist. Furthermore, although Mike appears to express agreement with Dennis about his reading (extract 2, sentence 7) his own reading (extract 1) appears to be totally empiricist: it makes no reference to conflicts of interest or interpersonal dispute. The interaction is depicted by Mike as concerned with elucidating the nature of a Kellian group and the way their own group is operating.

It is clear, therefore, that neither Mike's nor Dennis's readings can be considered as straightforward, literal descriptions of a reality unproblematically revealed by the transcript. The transcript appears susceptible to the production of alternative versions of the actions and beliefs which it embodies in just the same way as other aspects of scientists' social reality.

Let us now examine another reading which uses the R/A device. Again both contingent and empiricist versions of the actions revealed by the text are formulated. In this case, however, the R/A device is used to display en empiricist reality behind a contingent appearance; i.e. the inverse of the previous example. Ian suggests that one of his points appears to be a self-interested contribution to the psychological processes going on in the group. Yet, he claims, it is really concerned with questions of a theoretical nature.

4 Jonathan. (1)So you think that - I mean, to try and and keep it to this - do you think that's what is going on on the bottom of [page] four B?

Ian. (2)I think there's a triple sort of complication there. (3)I mean I look at myself saying that there and think, well I just, I chose a very silly example. (4)You know, what I was trying to do was to say something about theory and about what construct theory has to say about groups. (5)And then chose as an example something from the here and now, which is of course bound to end up in a spiral of infinite complication. (6)And I think Richard[1] is quite right to say keep it away from that. (7)I mean, one should, one should choose examples, I mean if you choose an example from the here and now it obviously becomes a complicating factor, doesn't it.

Jonathan. (8)So you read Richard[1] there as saying 'keep it away from the there and now'?

Ian. (9)Well I was certainly not wanting to get into the here and now. (10)I was wanting to get some ideas about how construct theory in groups, you know, at a, at a theoretical level. (11)I chose an example from the here and now because, you know, you think 'oh well, it's nice and clear and everyone can see it'. (12) [laughs] But, of course, everybody can't see it, because it is part of the very engine that is going on at that very moment...
(13)...it's clear isn't it, it's nice, you see what four, my first bit on four B is basically talking at the level of theory. (14)OK. (15)And in line four I take me and James[1] as an example. (16)OK. (17)Then Richard[1] takes that as being not interesting, but that, I wasn't particularly interested in it; only as an example. (18)I wasn't interested in it as

> a piece of interpersonal business to be sorted out. (19)So I think you were right to do that. (20)And then I think I go along with what Richard[1] is saying in practice, because on five A I am actually continuing to talk theory. (21)And not interested in getting into the examples either. (22)I think that that statement there of mine is what I was trying to say in a nutshell: that, you know, that goes back to an earlier distinction and that is the distinction between 'up there' and 'right here'. (23)And what I am trying to say about groups is that it is a very concrete business. (24)That the constructs are expressed absolutely concretely, in people. (25)And that to try and make out that it is some kind of floating thing up in the ceiling is, is um, something I wanted to disagree with at the time. (26)Does that clarify it? (Discussion, 12-14)

In his use of the R/A device Ian's point is the inverse of Dennis's (extract 2). It suggests that one of his statements which has been read as, and appears to be, contingent is really empiricist. Ian brings out this contrast most strongly in lines 13 to 21. He identifies his speech in the transcript ('my first bit on four B') and says that it is 'basically' concerned with theory (13). Furthermore, he characterises his point about the dispute between himself and another participant, James[1], as an 'example' of his theoretical point; he is not interested in it for its own sake (15). Ian then turns his attention to the response to his points from Richard[1]. He characterises Richard[1] as reading them as 'not interesting' (17) because they are a 'piece of interpersonal business' (18) which has to be sorted out. Yet he claims that he too is not interested in them for this reason (17-18); he is only interested in them 'as an example' which illustrates his theoretical point. And he goes on to give a gloss which depicts a particular piece of speech as summarising the whole 'in a nutshell' (22-26).

Overall, then, Mike contrasts what he sees as Richard's[1] misreading with his own proper understanding of the transcript. It is interesting to note, however, that he goes to some lengths to make Richard's[1] misreading accountable. He points out a number of reasons why Richard[1] should (mis)read his points as an attempt to contribute to the 'interpersonal business' rather than as a contribution to the theoretical discussion. For instance says that he was 'silly' to choose an example from the here and now to illustrate a theoretical point, because it was 'bound to end up in a spiral of infinite complication' (4-5). And he says that Richard[1] is 'quite right' in saying that they should not get involved in that (6); a claim he reiterates in sentence 19. Although he does not totally endorse Richard's[1] reading - which he sees as encouraging him not to use examples from the processes in the present group - he depicts it as both understandable and having a positive effect. Ian identifies Richard's[1] misreading as one which could be made by any reader not in possession of specialised knowledge or prepared for the degree of reflexivity which Ian[1] has introduced.

Richard is present during this discussion of the transcript and the possibility of contesting Ian's interpretation is therefore open to him. It is thus possible that Ian means this emphasis on the accountability of Richard's[1] reading to dissuade him from contesting his interpretation of the reading. For if Richard does contest it this will involve questioning the very positive attributions that Ian makes. (And indeed, Richard does not question the interpretation proferred - although it is not possible to say how far this was due to the form of Ian's account).

As before, I will examine the section of transcript which is being discussed. In this case it is possible to clearly pick out the passage being referred to.

5 Richard[1]. (1)I think I saw [this group] in the terms in which I have seen other groups, and would want to see other groups in the future, which is simply as being a set of constructs. (2)OK. (3)And these constructs are not in any simple way the constructs that are held by each individual. (4)They are a set of constructs that are, as it were, pushed up into the air - I mean I think of it very physically - by people talking to one another... (5)I mean the - this is my perspective, admittedly - it's success as a Kellian group in the way in which I would want to see it is that people do take away that set of constructs and identify it, for the sake of economy, or sort of cognitive processing and all the rest of it, as the set of constructs that were thrown up by the construings of particular people in a particular place in a particular time.

Ian[1]. (6)Yeah, that's the sort of thing; I mean the typical sort of thing that you get in human behaviour, or whatever you want to call it, is that in different contexts people will do different things. (7)Now, it might be that me and you, James[1], argue about something - OK - and you are defending so and so and I am attacking it. (8)It might be that next week somebody is attacking your position and I will come in and I will really defend your position. [James[1]. Um.] (9) You know, because I have taken away the entire set of alternatives that has been generated by this group and I then use it. (10)And I have become bigger as a result of being with this group.

<u>Richard[1]</u>. (11)I am not interested - sorry, just to come back in an egocentric way - that does not fit what I said. (12)I am not terribly interested in what happens to your or what is happening to you and that's why I am just going back to where we started in this bit of the discussion; you and James[1] is really not terribly interesting.

<u>Ian[1]</u>. (13)Yes, well, I was just trying to illustrate what you are saying.

<u>Richard[1]</u>. (14)Well it doesn't illustrate what I am trying to say from the point of view of a Kellian group. [Ian[1]. Um.] (15)Because the set of constructs that constitutes a Kellian group for me... is at a totally different level from what is happening between the two of you. (16)That's why I think it is very unprofitable to start talking about a Kellian group in terms of what's happening between two people, because one very quickly gets locked into that way of seeing the group.

<u>Ian[1]</u>. (17)I don't see how you can separate them; you are talking about group constructs, or whatever you call them, as being up there in the air, you literally went like that [gestures] and I don't think they are up there; I think they are between us; I think they are very concrete; they are acts.

<u>Richard[1]</u>. (18)Yes, but they are not between you and James[1].

<u>Ian[1]</u>. (19)That was just an example.
(Transcript, 227-228)

At the start of this extract Richard[1] gives an account of the way he would characterise the construct workshop from the perspective of personal construct theory (1-4). Ian[1] responds positively to this and suggests that it is a 'typical' feature of 'human behaviour' (6). He then goes on to present his interaction with James[1], another participant, as an example of what Richard[1] is saying (7-10). Furthermore, by using such constructions as 'it <u>might</u> be' (7 and 8, emphasis added) Ian[1] seems to depict this example as hypothetical.

Richard[1] responds to Ian[1] by claiming that this example does not fit what he himself said (11). As we saw above, when discussing the transcript Ian reads Richard[1] as saying he is 'not interested' in the example because it was from the 'here and now'. This interpretation of Richard's[1] reading takes it to be primarily concerned with contingent matters of dispute peculiar to the group. However, although emphasising sentences 11 and 12, it ignores sentence 14, where Richard[1] denies that Ian's[1] example illustrates the theoretical point he is developing, and sentence 16 where Richard[1] seems to single out the *form* of the example, rather than its specific content, as the thing with which he disagrees. However, at the time Ian[1] seems to respond *both* in the way he does in the discussion (taking Richard[1] to be misreading his point) *and* by taking Richard[1] to disagree about (but correctly read) his point.

In Ian's[1] first response to Richard[1] (13) he stresses that his point was just meant as an illustration of what Richard[1] has been saying. It thus seems to be taking Richard[1] to interpret his point in contingent terms, as a contribution to group processes, and attempts to show this reading to be incorrect. However, Ian's[1] second response to Richard[1] (17) is very different. It argues for theoretical reasons against Richard's[1] theoretical claim that it is unprofitable to try to understand a Kellian group in terms of processes occurring in dyads. In doing so, Ian[1] can be seen to be treating Richard[1] as accepting that Ian's[1] point is actually a contribution to theoretical discussion. For Richard[1] is no longer seen to be mistaken over what *kind* of point Ian[1] is making; rather it is a theoretical point which Ian[1] sees as dividing them. Furthermore, these two responses to Richard's[1] points seem to conflict with one another in two distinct ways. Firstly, as we have noted, they presuppose different interpretations of Richard's[1] readings. Secondly, while one speech claims only to be an *illustration* of what Richard[1] is saying the other expresses *disagreement* with what he is saying. So what first appears as support and elaboration is later changed to criticism and dissent. As with the last example, then, there is no straightforward analysts' reading of the transcript which will allow an evaluation of the correctness of different participants' readings.

Let me summarise the analysis up to now. So far in this section I have documented three rather different ways of reading or characterising this transcribed discussion. The first depicts the transcript in purely empiricist terms. This is seen in extract 1 where the speaker characterises the discussion as a constructive interchange concerning two basic themes: the present group's interaction; the nature of a hypothetical Kellian group. Although the social psychological processes occurring in the group might appear to be a more contingent than empiricist matter, the speaker characterises them as a topic for discussion; and as such they are not depicted as influencing the discussion in any way. Rather they are depicted as if they were an available and rich source of data which is open to disinterested analysis. This empiricist reading thus takes the group processes as a topic for discussion but does not indicate that they should have any relevant influences on the course of the discussion. The topic of the group processes at work is seen as distinct from the effects or functions of such processes. The work of the discussion is treated as entirely concerned with evaluating the coherence of Kellian theory and checking its adequacy for dealing with a particular form of social interaction.

The second way of reading the transcript (extract 2) also draws on this notion that the discussion concerns two separate themes. However, this time the transcript is read as a document of a contingent reality. The speaker identifies what appear to be comments on the question of what a Kellian group should be like but which are really contributions to the group processes whose discussion constitutes the other theme; i.e. instead of being neutral and disinterested comments on the theoretical point, they are seen as a consequence of and a contribution to the contingent interaction occurring in the group.

This reading strategy is more complicated than the first. It makes use of the R/A device. That is, it does not merely identify the text as constituting a certain kind of discourse; it also explicitly formulates an alternative reading which is mistaken. It treats the text as if its real nature is concealed: the participants who produce it are 'cheating' by making points in such a way that their actual meaning is hidden. In extract 2 the speaker thus formulates the appearance of the text, which is neutral and innocent, but contrasts this with the real nature of the text, thereby subverting the appearance of innocence and displaying the interpersonal business which, he suggests, is in fact being conducted.

This strategy is related to the accounting technique of 'preformulation', used for dealing with conflicting views, which I outlined in chapter six. I noted that instead of speakers merely formulating their own individual position they may also formulate the conflicting position of their (real or potential) competitors. The conflicting position can then be dealt with in two ways. One way is to give straightforward criticisms on contingent grounds: the experiment is flawed; there is a contradiction in the theory. Another is to give an explanation of why that speaker holds such views. That is they can be rationally criticised or causally explained, respectively. In formulating their own version of opponents' views they can be 'prepared' for rejection by emphasising certain features and ignoring others. In the case of extract 2 the apparent version is explained as an artful disguise to allow partisan interpersonal business to appear as if it is a theoretical point, which is (implicitly) more acceptable in this context.

The third way of reading the transcript (extract 4) has a similar structure to the second. Again it uses the R/A device. An apparent, although mistaken, reading is formulated; and this is contrasted with the reading of the transcript which properly reveals the nature of the participants' interaction. In this case, however, the apparent reading takes the discourse to be orientated towards contingent goals while the discourse is seen to actually be empiricist. The apparent reading which is formulated in this extract is that of a specific participant at the workshop, rather than the unspecified reader of extract 2. Nevertheless, it is explained as an accountable misreading which might have been made by any participant; it is not identified merely with the particular interests of Richard[1], the specified (mis)reader.

From this analytic section, then, it is possible to document three provisional conclusions. Firstly that, in some cases at least, scientists may interpret discourse in terms of the kind of empiricist and contingent interpretative repertoires that I have discussed in other places in this thesis. They may thus account for talk in similar ways to those with which they account for action and belief. Secondly, it is possible to read this discourse in a number of rather different ways. In the extracts above there are examples of both the same scientists and different scientists giving alternative and contradictory readings of the same piece of talk. However, as yet I have made no detailed attempt to explicate the function of these different readings, although I will make some suggestions in this direction later in the chapter. Thirdly, I have outlined the structure of a certain sort of deconstructive reading made by participants by use of the R/A device. In this reading they formulate as apparently literal a certain section of talk and then undermine this appearance of unproblematic reference with some further account or explanation. It is then contrasted with a reading which puports to state the literal meaning of the talk. In the cases I have examined, this sort of accounting is used to depict apparently contingent talk as empiricist and vice verse. Clearly if such a reading strategy is commonly available to scientists then it allows considerable interpretative flexibility in the way participants' construct versions of their social worlds. Let me now return to specific cases and examine this question in more detail.

In the following extract from the discussion we will see two further uses of the R/A device in the context of textual readings. The participants are discussing the meaning of a certain part of the transcript, and in particular of a statement by Carol[1] (who is not present for the discussion).

6 <u>Niel</u>. (1)Why did Carol[1] say 'but I don't like being rejected like that'?

<u>Ian</u>. (2)I don't know. (3)It's a very important bit, isn't it. (4)I can't remember.

<u>Dennis</u>. (5)Again I think that there is a/ [Mike cuts Dennis off]

<u>Mike</u>. (6)It effectively shuts Carol[1] up for about a page and a half. [laughs]

<u>Shirley</u>. (7)Well, Carol's[1] remarkably quiet through the whole thing when you look, really. (8)She comes

in occasionally, here and there, doesn't she.

Frank. (9)Isn't it this reference about leadership, that that refers to there? (11)I would infer that, not knowing what went on, just from the text, that in fact Carol[1] is reacting to Ian's[1] previous statement that he [Mike[1]. Right.] simply wants to describe, wants to describe what's going on as a bid for leadership of the group. (12)And she responds by saying 'oh, that hurt', 'I don't like being rejected like that'. (13)And other people join in by saying 'we don't like it, either'. (14)And, er, you say 'I don't like role theory'. (15)That's what it looks like. (16)It may not be that.

Mike. (17)But I, I think in effect Carol[1] loses at that point. (18)I mean, a bit anyway, whatever the decision was, it took the direction of the next page and a half rather than the direction which she wanted it to go.

Ian. (20)Can you say all that again Frank. (21)I wasn't at the right place.

Frank. (22)Um, I think that there was a general reaction against the way of describing group processes which you offered in the previous piece, in which you tend to describe it as a bid for leadership. (23)And it looks like a reaction against that. (24)And it is not just Carol[1], it is a collective.

Ian. (25)And where's the bid for leadership? Three A?

Frank. (27)Er, four A.

Shirley. (28)Just at the last few lines of Ian[1].

Mike. (29)Towards your, towards the end of your...

Ian. (30)I got it, yes.

Mike. (31)You see, but I think also that whilst everybody is saying 'no' there, the actual understanding is a recognition that probably that was going on: let's not acknowledge it, let's acknowledge it but not say it. (32)So there is a sort of 'let's ease it over that bump'. (33)I mean the fact that it is so universal is, wasn't a rejection of it, it was an acknowledgement of it with [laughs] 'let's not play that, play that through again'.

Jonathan. (34)I, I mean, what's the 'like that' referring to, then, in Carol's[1] speech? (35)What is the specific 'this' for rejection?

Richard. (36)I had assumed it was being talked about in role terms, rather than the person. (37)I mean, having one's discussion reduced to role play.

Mike. (38)But I think that also hides the fact that there was an agenda negotiation going on there. (39) And that she's lost the agenda negotiation, as well as having it lost in terms of having it discussed in role terms. (40)I mean it is/ [Neil interrupts]

Neil. (41)I mean, she does bring up the aganda.

Mike. (42)She brings it up later, [Neil. Yes.] yes.

(43)But my feeling was that there was a negotiation about which direction the thing was going to go. (44)And I can't put my finger on it, but Carol[1] is trying to make it go in one direction [Neil. Ahh.] and that whilst this thing about role is put in those terms she in fact loses the agenda argument, or discussion. (Discussion, 28-30)

At the very start of the extract Neil asks why Carol[1] made the statement 'I don't like being rejected like that' (1). Ian responds to this question by noting that it is 'very important' (3), but he is unable to answer it (2). There is an interesting difference between the two grounds that Ian offers for his inability. At first he simply professes lack of knowledge (2). Yet, almost immediately after this, he claims that the problem is in his <u>retrieving</u> the knowledge, that he 'can't remember' (4). The difference between Ian's two claims is illustrated by the acceptability of sentences such as 'I know, but I can't remember'. Not remembering implies knowledge, not lack of knowledge. Ian's two formulations, then, can be seen as implying two different models of retrieving what 'really went on' or what the participants' actions actually were. One suggests that they can be recovered from the text, while the other suggests that such information may be exhumed through a feat of memory. This sort of distinction operates in several places in extract 6. In this case the formulation in terms of remembering seems to suggest that Neil's query <u>could</u> be answered simply through memories of the earlier conference and without relying on the transcript, although Ian is not able to provide an answer at this point.

Mike follows Ian's point by drawing attention to the <u>consequences</u> of the interaction (6). He notes that Carol[1] makes no more contributions in the next page and a half of transcript and explains this as a result of the interaction 'shutting Carol[1] up'; i.e. he depicts her silence as a relevant and noticeable feature of the transcript rather than a 'normal' pause between conversational turns. Mike's speech appears to be intended to endorse Ian's point about the importance of the interaction being discussed. It shows the speech to be important by making explicit its effect of 'shutting Carol[1] up'. In making this interpretation Mike makes no explicit claim to any special knowledge that might have come from his attendence at the previous conference. Indeed, his categorisation of the time in which Carol[1] is quiet in terms of 'pages' suggests that he is drawing closely on the organisation of the transcript.

In sentences 1 to 6, then, three participants who were at the original conference (Neil, Ian and Mike) stress the importance of a particular section of interaction. Yet at this point they do not interpret it or provide any specific account for it. As we saw, Ian raises the possibility of remembering what went on, but none of these participants appear to use special knowledge of this kind.

Two turns now follow from people who were not participants at the earlier conference: Shirley and Frank. Shirley's point is a response to Mike, and she seems to imply that what he identifies as the interactionally special absence of Carol's[1] contributions is in fact normal for the whole transcript (7). However, this is not clearly a contradiction of Mike's point - indeed, it can be read as endorsing it. For she depicts the frequency of Carol's[1] silences as 'remarkable' throughout the extract. This does not contradict the noteworthiness of the phenomenon that Mike identifies (Carol[1] being 'shut up') but rather suggests that the phenomenon may be commonplace in the transcript. While Shirley does not propose any specific interpretation of the interaction, being content to elaborate on the issue of Carol's[1] silence, Frank attempts a detailed gloss on the piece of transcript being discussed.

There are a number of interesting features of Frank's gloss. In the first place, as I have noted, Frank did not participate in the original conference. Yet here he is the first to give an account of some interaction which went on there. That he does this shows that participation in the original conference is not a necessary condition for interpreting the transcript. Or, put another way, that these participants do not seem to think it is necessary to witness 'what went on' (including the non-verbal communication, seating arrangements and so on) or experience it (the 'heat' of the dispute, the 'affinities' between participants) in order to contribute. Nevertheless, Frank's point does formulate a distinction between what appears *from the text* to be going on and what is really going on. In other words, we can see Frank applying the R/A device to this distinction. In this case instead of the R and A categories being 'contingent' and 'empiricist' they are 'the text' and what is 'really' going on. Thus in sentence 11 Frank notes that his interpretation is inferred 'just from the text' and that he does not 'know what went on'. Likewise, in sentences 15 and 16 he contrasts his *interpretation* of the interaction with what the interaction actually is. In formulating his reading as an appearance which may or may not correspond to the reality of events Frank should not be seen as undermining his own point; rather he proposes an interpretation while allowing the *possibility* that special knowledge (the existence of which is hinted at by Ian) may undermine it. Frank depicts his point as correct but not infallibly so.

The structure of Frank's interpretation is complicated. Apart from its being framed by an R/A device it involves a hierachy of readings. Thus Frank reads Carol^1 as reading Ian^1 as reading 'what's going on' as being an interaction of such and such a type. One way to look at this would be as a 'four layered reading' (figure 2). Yet the 'bottom' three layers of talk are all formulated in Frank's own discourse and each of them is accessible (at least partially) independently of the higher layers. Thus a more accurate scheme for understanding his interpretation would be figure 3. Frank sees Ian's^1 reading (B, figure 3) as constituting a 'description' (11); and he sees Carol^1 as taking Ian^1 to be interpreting the interaction in this way (A, figure 3), and as 'reacting' against it (11). Thus Carol^1 is depicted as 'reacting' to Ian's^1 claim to be correctly describing what is happening. Furthermore, Frank suggests, other people join in to agree with Carol^1. And when Ian (20) asks Frank to say all that again Frank strengthens his account from saying that (unspecified) 'other people' contest Ian's^1 claim to saying there is a 'general reaction' against it by a collective of people which includes Carol^1 (22-24).

Throughout Frank's two speeches (9-16, 22-24) he gives no clear indication of whether the statements 'reacting to' and 'general reaction against' should be read as 'dis*agreeing*

A 'FOUR LAYERED' READING

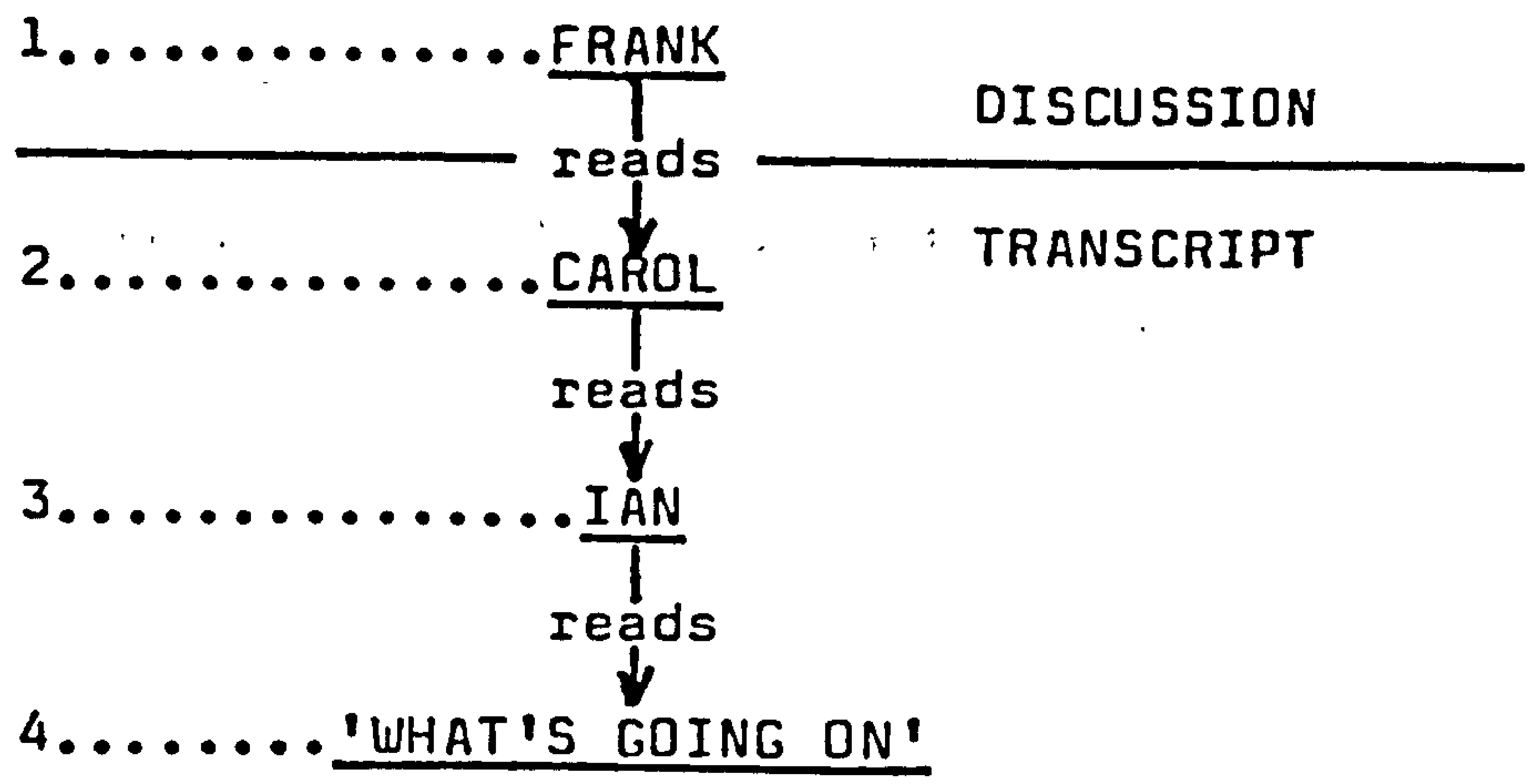

Figure 2.

A 'DOUBLE LAYERED' READING

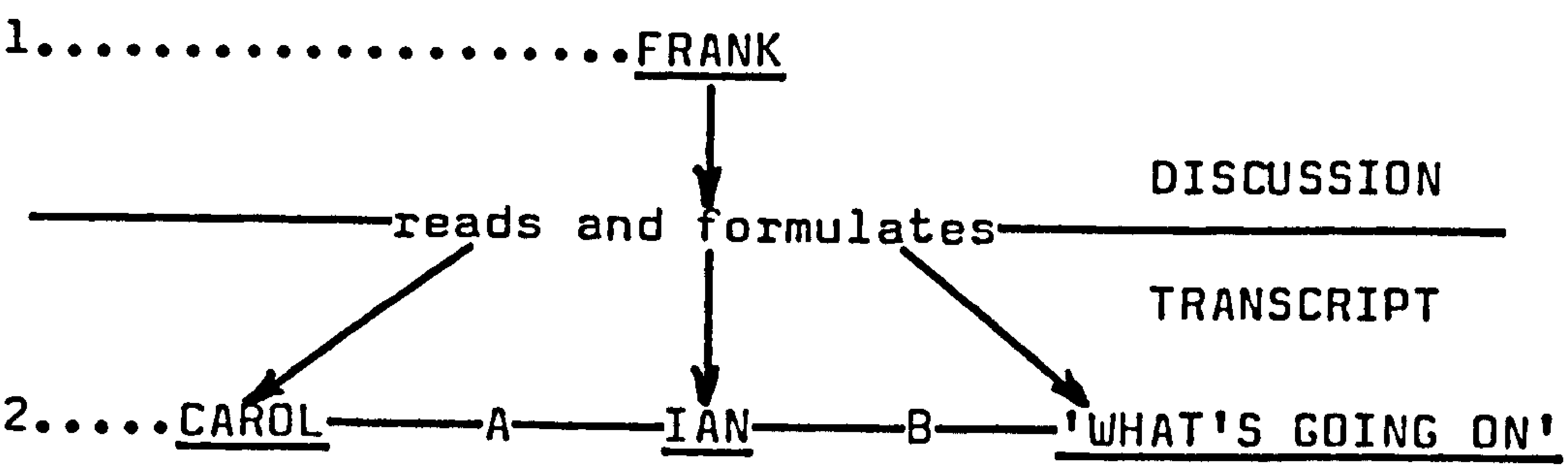

Figure 3.

with' - with the implication that Carol^1 and others think it is not the case that such and such - or as 'disliking' with the implication that Carol^1 and others think that it is the case that such and such. Frank's account makes no fully articulated and explicit commitment with respect to the exact interpretation that Carol^1 and others make of Ian's^1 descriptions, although he is more definitive with respect to their evaluation of Ian's^1 description.

It is now clear how Frank's point answers Neil's original question: 'why did Carol say 'but I don't like being rejected like that'' (1). For Frank the rejection is constituted by Ian^1 describing Carol's^1 behaviour in contingent terms as being a 'bid for leadership of the group' (11). Carol's^1 reaction is thus a 'natural' response to the pejorative overtones of Ian's^1 account.

Frank could perhaps be read as interpreting the interaction in terms of a 'norm' that contingent accounts are inappropriate in this particular context and making the 'reaction' intelligible in terms of the perceived contradiction of this norm. However, the 'reaction' can, I think, be better understood in terms of more specific contextual features to be taking Ian's^1 point as an 'accusation' that Carol's^1 behaviour displays her hidden motivation to become group leader. It is probably because of the danger of interactional problems such as these arising that accounts of this kind are rarely found in more formal contexts. Moreover, the restriction of such accounts to informal situations could explain why participants recurrently claim that what is 'really interesting' (for sociologists and psychologists studying science) at conferences is talk over dinner, in the bar and so on. It may well be that here, in situations where the information flow can be carefully managed (or appears so) that much informal accounting of this kind takes place. It is here that participants construct what they see as the sociological and psychological versions of events. Because of the privacy of these contexts such accounts are less likely to provoke the kind of 'reaction' described by Frank.

Before continuing the analysis of extract 6 I will briefly recap on what has been suggested so far. I have noted that the participants make use of a distinction between knowledge of what went on derived directly from the text and forms of special knowledge such as memories of what went on. As yet, however, we have seen no clear indication of such special knowledge used in practice. In addition, we have seen this difference formulated within the framework of an R/A device, where the reality of 'what went on' is contrasted with the (possibly) distinct appearance presented by the text. We have also seen the way readings can be formulated in hierachical structures of considerable complexity in the course of providing explanatory accounts.

Returning now to extract 7, we can see Mike's turns of talk (6; 17-19; 31-33 and particularly 38-40; 42-44) using the R/A device in the manner of Ian in extract 4. In this case Mike reformulates part of Frank's account of the interaction as the appearance to which he himself provides a contrasting reality. The R/A device is used to reframe a previous turn of talk and undermine its claim to veridicality rather than to reformulate an account given by the present speaker, as we have seen in extracts 2 and 4. As before the structure of Mike's discourse is complicated.

In sentences 17 to 19 Mike interprets the transcribed interaction under discussion in contingent terms, as Carol[1] 'losing'. What he sees her as losing, as is shown in line 38, is the negotiation over the agenda. He describes the consequence of this loss as Carol[1] being unable to influence the direction of the discussion in the next page and a half of text (19). However, in sentence 31 Mike appears to extend the concept of loss even further, to cover Carol[1] losing the bid for leadership. It is here that Mike makes use of the R/A device.

In sentence 31 Mike reformulates the point made by Frank - in which he suggests that there is a general reaction in the group against Ian's[1] description of what is going on as a bid for leadership. As I noted above, Frank's discourse does not appear to be explicitly committed on the question of whether people are disagreeing with the factual status of Ian's[1] description. However, Mike interprets Frank's gloss as definitely taking people to <u>disagree</u> with the factual status of Ian's[1] account; he claims that 'everybody is saying' that Carol[1] is not attempting to gain leadership of the group (as Ian[1]) suggests). Yet Mike frames this version of Frank's gloss, using the R/A device, as the appearance which he contrasts with what is actually happening. He contrasts what 'everybody' is saying (Carol[1] is <u>not</u> making a bid for leadership) with their 'actual understanding' (Carol[1] <u>is</u> making a bid for leadership).

Mike's use of the R/A device is in some ways similar to Frank's. He contrasts what is <u>said</u> with what is <u>really going on</u>, just as Frank contrasts what can be derived from the text (the transcript of what is said) to what is actually happening in the interaction. Yet, while Frank proposes a disjunction between the text and the actual events which is <u>potential only</u>, Mike proposes that the two things are <u>in fact</u> different in this case. Mike goes on to elaborate on this interpretation by providing an explicit 'translation' of the hidden speech or subtext, just as we saw in extract 2 above. This translation takes the form of quotation. It is speech as if from the participants' perspective, rather than the speaker's, although it is, of course, not actually said[15]. Mike describes the participants as wishing to acknowledge Carol's[1] bid for leadership but not explicitly say that it is happening (31). Indeed, Mike uses the very unanimity of the participants' <u>not saying</u> that it is happening as grounds for his claim that the participants are really acknowledging that it is happening (33). Additional implicit speech is 'reported' which explains why there should be this conflict between what is said and what is being done. Mike suggests that there will be interactional difficulties raised by making this contingent version of events explicit. So, to avoid these problems, there is only implicit acknowledgement of the contingent reality (32-33). What everybody is rejecting, according to Mike, is not the reality, which is contingent, but the problems and distractions that acknowledging its existence would create.

In effect, then, Mike's use of the R/A device contrasts a contingent version of events which is <u>really</u> going on (Carol bidding for leadership of the group and - the same thing? - for control over the topics to be discussed;

but being prevented from achieving this goal) with an alternative version, apparent in the participants' talk, which contradicts this. Moreover, the very unanimity of the spoken version is used as grounds for warranting the correctness of the unspoken version. It might seem that here is a prime instance where the special knowledge held by participants at the original conference is used succesfully to give an interpretation which 'goes beyond' the restricted verbal record of events provided by the transcript. Yet there are certain indications which belie this suggestion that Mike is simply revealing the correct version which Frank, with his lack of first hand experience, is unable to do.

These indications are apparent in Mike's response to a point made by Richard (who also participated in the earlier conference). In sentence 36 Richard proposes a rather different explanation from Mike's for Carol's[1] statement about rejection (1). In this explanation 'rejection' comes not from an implicit accusation that Carol[1] is attempting to gain leadership over the group, but from the theoretical categories used to characterise the interaction. Characterising it in specifically role terms is described by Richard as undermining the personal aspects of the interaction. What is interesting in the present context is the way Mike, instead of disagreeing with Richard, attempts to introduce features of Richard's account into his own. Accordingly in sentence 38, directly following Richard's contribution, Mike depicts the issue of the role characterisation as 'hiding' the agenda negotiation and the fact that Carol[1] lost it. Yet Mike then notes that being discussed in role terms also constitutes losing the agenda argument (39). He tries to assimilate Richard's explanation to his own whilst retaining the central features of each. And at this point he starts to downgrade the facticity of his own account. The interpretation of an agenda negotiation, which has previously been confidently asserted is now hedged as a 'feeling' (43). Moreover, although he reiterates the claim that the role characterisation in some way leads to the loss of the argument (44), he this time claims that he cannot give an exact interpretation of this: 'I can't put my finger on it' (44). Mike therefore moves from the internally consistent and confident 'factual' account of sentences 31-33, which reformulate Frank's claim using the R/A device, to the vaguer, hedged around, 'intuitive' account of sentences 42-44.

Overall, then, through the course of this extract a number of differing accounts of what is 'going on' in a section of transcript are offered. The two accounts which I have concentrated on (Frank's and Mike's) both use an R/A device. In each case the talk itself is formulated as the appearance. For Frank this appearance corresponds to what is really going on (although he raises the possibility that it might not). For Mike the two do not correspond. Furthermore, Mike explicitly formulates the interpretation given in the previous turn (Frank's) as the appearance, rather than formulating this appearance entirely in his own discourse. This difference may, however, be only a matter of degree. For even where participants formulate appearances in their own discourse this may be intended to undermine other locally produced versions.

With the previous two examples I went on to examine the section of transcript which is the topic of discussion. In this case the complexity is such that there is no space for such an examination here. I will simply reproduce the appropriate section of transcript so that the reader is free to examine questions of the relationship between discussion and transcript and the 'basis' for the different readings produced by Frank, Mike and Richard.

7 Dennis[1]. The group has a kind of understanding that you are going to have to kill the experiment if you opt out because even if you opt out somehow you have done something that is part of the experiment, you know, that is you have said something about yourself. You know, it is kind of like the question of freedoms and constrictions.

[pause]

Ian[1]. When you say the group has an understanding I am not sure that, he was saying that the reason he didn't want to do it was for purely personal reasons, he didn't want to tighten; in other words a personal construct theory analysis of why you didn't do it. I would say that we have to say more than that. We have to look at the whole role structure of the group, the way it is begining to develop and the fact that that was a piece of process that went on which was bidding for leadership of the group and saying 'I am going to structure the next half hour'.

Carol[1]. But I don't like being rejected like that. And I have a personal/ [Dennis cuts Carol[1] off]

Dennis[1]. We don't like...

Mike[1]. [laughs] And I don't like role theory. [some laughter]

Ian[1]. Yes, well I don't want to use the word role but that was the one/ [Ian[1] is cut off]

[several people talk at once, laughter]
(Transcript, 226)

Discussion: Reading, Resources and Reality

Let me now examine some of the broader issues that arise out of these findings. In the course of the analysis I have discussed a number of ways in which participants characterise text. In particular I have examined the use of what I have termed the R/A device and the way participants draw on the contingent and empiricist repertoires when describing discourse.

Participants have the option of treating any particular section of discourse either as a kind of transparent medium which provides direct access to the actions of participants or as a medium which can conceal these actions. In the former case a speaker will simply list or describe the acts which may be 'seen' in the transcript. Extract number 1 is of this type. The speaker uses a number of act and action categories to describe what the transcript reveals is 'going on' in the earlier workshop. The transcript is taken to document the shared group of acts of 'using' (a hypothetical notion of a group), 'thinking' (about this notion), 'producing' (a more generalised theory of the application of Kellian ideas to group dynamics) and so on. Furthermore, at a higher level of generality these actions, taken together, are depicted as 'working' successfully. Such an account is not merely a reformulation of what is said, it also starts to describe what the saying is for, or what is done by what is said. Thus the utterances discussed in extract 1 are taken by the speaker to be doing certain sorts of theory development. In addition, the speaker indicates that his interpretation is based on a reading of the text (as opposed to memory, for example) and that it conflicts with an earlier interpretation. Nevertheless the relation between these actions and the text from which they are inferred is treated as unproblematic.

An alternative way in which participants treat the discourse is to see it as concealing those actions which do occur. In this case the text will appear to embody certain actions; yet these are not the actual actions of participants. It is this form of accounting that I have described by the term R/A device. Such accounts appear in extracts 2, 4 and 6. In these cases two versions of the actions embodied in the text are proposed. One of these is depicted as obvious to the reader or as the version which would be arrived at by taking the text at face value. The other version is depicted as concealed by this obvious version, as implicit, yet not apparent, in the text. Extract 2 shows how this works. The speaker characterises a section of transcript as appearing to embody a statement about the ideal form of group predicted on theoretical grounds. However, the speaker goes on to say that the text actually embodies criticism of the behaviour of other members of the group. This device thus suggests superior knowledge or skills on the part of its user. For it implies that the user is able to penetrate the appearance to reveal what actions are actually occurring.

Clearly such a device presupposes two readers: the speaker and some notional or implied reader. In the different instances of use of the R/A device above various notional readers are suggested. For instance, in extracts 2 and 4 the notional reader who recovers only apparent actions is equated with a generalised reader who is without special knowledge and reads the text innocently. We might compare this to Dorothy Smith's initial reading of the account of K's illness[16]. In this reading the account is taken to be actually what it appears (to a naive reader, to Doroth Smith at first) to say. In extracts 4 and 7 on the other hand, although the idea of a generalised notional reader is not abandoned a specific reader of the text is also introduced. Thus in extract 4 Ian characterises Richard's[1] readings as only recovering what the text appears to say (only those actions which the text appears to embody) not what it actually says (the actions it actually embodies). Yet Ian describes Richard's[1] misreading as only to be expected because of the organisation of the discourse and the limits to people's reading skills. He thus implies that Richard's[1] reading is equivalent to that of a generalised reader; it is not therefore a reading promoted by any specific interests that Richard[1] might have. Similarly, in extract 6, Frank's specific reading is undermined although it is acknowledged to be congruent with how the text would appear to any reader without special knowledge[17].

What is the role of the R/A device in scientific discourse? Clearly it is used in the construction and rejection of versions of events. However, it is specifically suited to certain tasks, for instance those in which the speaker is dealing with a version of events which is taken as obvious or natural. In such cases simply disagreeing with the version, or simply producing a competing version, might well be ineffective. For it would not deal with the question of the alternative's obviousness. At best such an approach would succeed in producing a viable competitor. However, the R/A device works by undermining one version in tandem with the production of an alternative. Moreover, it addresses the issue of the obviousness of the competing account. Instead of trying to contradict this obviousness, it is accepted. However, the R/A device, in its formulation of a reality which lies 'behind' appearances, distinguishes obviousness from correctness. Indeed, it specifically formulates obviousness as equivalent to incorrectness[18].

A second interactionally important feature of the R/A device is that users may formulate their own versions of the position which is to be rejected. This allows them to 'prepare' the position for rejection by emphasising those features which will be used as grounds for rejection or which might support certain appearances but not actual events and ignoring or downplaying others. For instance, in extract 6 Mike uses the R/A device to display an earlier speaker's version as only an appearance. Mike formulates the basis for the version he is criticising as lying in what the participants are saying. But he implies that if readers 'look beyond' what is said a different, real version will become clear. It is only by formulating the rejected version in this way that Mike can sustain the credibility of the rejection. If, for example, the earlier version was characterised as derived from, say, the 'whole context' Mike's replacement versions would become merely one possible competitor not the the now 'obvious' successor.

A third important feature of the R/A device is the flexibility in accounting which it facilitates. In the examples analysed above there are typically important differences between the two versions formulated. For instance, one will be contingent and the other empiricist (as I will discuss in a moment) or, as in extract 6, a unanimous verbal rejection is taken as hiding a unanimous acceptance. The actual degree of flexibility will, of course, be a practical issue which will depend on each specific context of use. However, it does seem that the R/A device is suitable for dealing with highly disjunctive versions.

Such accounting will not, of course, necessarily be successful; other participants will not always be convinced of the reality implied by the R/A device. Indeed, in the examples discussed, instances where participants' produce competing versions subsequent to the use of the device can be found. For example, Richard does so after Mike's use of the device in extract 6. Further work would be necessary to show in exactly what contexts the device could be used most effectively. However, its use does seem to be one practical, if not omnipotent, solution to certain interactional problems.

In addition, the device provides one way in which the problem of competing versions can be resolved in practice. For instead of discussion and dispute amassing increasingly large numbers of competing versions of events, versions are continually rejected as being of appearances only. The notion of a real version which may or may not lie behind appearances is continually reinforced by the use of the R/A device. It thus maintains the idea of a definitive truth while explaining away the existence of a multitude of inconsistent versions of it[19].

I will now return to the issue with which I introduced this chapter, namely the issue of resources being used to construct definitive versions of actions embodied in texts. As we saw, Barthes has suggested that readers and critics draw on particular kinds of resources when producing readings of literary texts. These resources might be typically the 'intentions of the author', the 'ideology behind the text' or the 'nature of the world' which the text is seen as a representation of. While in no way wishing to exclude the possibility that these resources have important functions in the interpretation of scientific discourse (clearly the latter two have been much utilised by sociologists and philosophers, respectively), in the analysis above I have concentrated on the use of two specific resources: the contingent and empiricist repertoires.

In what way can these repertoires be seen as resouces in the sense outlined by Barthes? As I have noted, when dealing with the pre-circulated transcript the participants generally take it to be a document of specific acts and actions. Giving a reading of the transcript therefore involves providing an interpretation of what acts and actions are performed by or through what is said. Frequently such interpretations involve specifications of acts using the contingent and empiricist repertoires. For example, talk might be interpreted in empiricist terms as a development of theory (extract 1) or in contingent terms as an interpersonal criticism couched in the language of theoretical commentary (extract 2). Thus in the same way that Barthes sees the use of a resource such as the notion of ideology as enabling and legitimating a specific reading of a text, the participants draw upon the interpretative repertoires to give definitive readings of the transcript and close off alternative readings.

Perhaps a more precise analogy would be with the more specific cultural codes which Barthes discusses in S/Z. Each of these codes consists of an organised corpus of background knowledge about, for instance, character and personality. It is through the use of these codes that the reader makes sense of the text. Indeed, the text cannot be properly considered to have a meaning outside of these constructive acts of sense making. The empiricist and contingent repertoires could similarly be considered to be codes or semiotic systems for making sense of transcribed discussion and other forms of scientific discourse. It is these codes which allow the reader to construct actions (such as 'using a specific notion' - extract 1) and complete acts (such as 'producing a better theory' - extract 1) from discourse. Although at times participants appear to treat this move as natural and unproblematic, the frequent production of competing readings as I have documented above exposes its interpretative nature[20].

The possibility of competing readings and the problematic nature of the relation between discourse and actions becomes particularly clear where the R/A device is used with empiricist and contingent as the contrasted categories. In these cases participants characterise what is really going on in, say, contingent terms, and contrast what appears to be going on in empiricist terms. The two repertoires are thus treated as incompatible alternatives for making sense of the transcribed discourse. One repertoire is treated as providing one single reading of the reality which underlies the text and the other as only providing a mistaken version which captures how the text appears but not what it really says. The participants treat the transcript as embodying a single set of actions, which it is the task of reading to reveal. Exactly what actions are embodied is a practical problem about which there may be some disagreement. Nevertheless, it is assumed that given time or special knowledge it will be possible to produce such a single correct version.

This tendency to treat actions as having one particular meaning - as being bids for leadership but not theory development, or intellectual discussion but not personal criticism - could be part of the reason why in a number of cases a distinction is made between the transcript and the actions of the participants (see extracts 1, 2, 6). For a distinction of this kind can deal with the potential conflict between the notion that there is a single correct reading of the text and the fact that there is argument over the transcript's various interpretations and that participants may find it enigmatic. If the relations between text and action were natural and transparent this would imply that there ought to be little disagreement over what the participants are doing on any particular occasion being discussed. Moreover, the transcript would be a straightforward record of the participants' actions which would not therefore need interpretative work to recover them. Without such a clear distinction between the discourse and participants' actions these disagreements and ambiguities would suggest that the same discourse could be performing a variety of different actions and perhaps not clearly any one single action. This would therefore undermine the assumption that only a single, unitary set of actions is being performed at any one time. However, by stressing the fallibility of the transcript this assumption can be maintained in the face of the practical difficulties encountered while reading.

I want now to move from these specific conclusions and outline how the above study might throw light on a general analytic problem which has been raised by Steven Yearley[21]. This concerns the status of analysts' readings of scientific and other texts. Briefly stated, the problem concerns the potential of texts to affect readers: for instance, if a text embodies certain 'externalising devices', as Woolgar claims of accounts of discovery[22], should these be seen as routinely effective in influencing the reader's perception of discovery? Yearley suggests that the problem of much work on the organisation and function of texts is that it is not sufficiently empirical, in the sense that it does not pay enough attention to the way that texts are generally read. Instead analysts have tended to concentrate on the construction of texts and have presupposed that this will affect the reader.

Now it is, I think, true that the question of how texts have been read by scientists has been a somewhat neglected topic and Yearley is right that studies addressing the problem of the effectiveness of texts ought to be carried out. However, I wish to suggest a slightly different approach to the studies of Smith, Woolgar and Barthes to the one given by Yearley. He suggests that Smith's and Woolgar's studies of discourse are empirical in the sense that they draw upon their own responses as readers as a basis for analysis. However, although their own responses are implied they are not crucial for their analysis as long as we are careful to treat them as deconstructive studies of texts and not of readers' experience. These studies are not concerned with the phenomenology of reading, in the sense of whether the authors were persuaded of the reality of K's illness or the 'out-there-ness' of discovery, but with the organisation of discourse concerning these things. It is true that Smith claims that she was initially persuaded, and that some of the interest and significance of her study is derived from this, but the actual analysis is concerned with peculiarities in the discursive organisation in an account of mental illness. Smith's procedure is to reveal the descriptive contingency of the text by showing that it cannot be taken as a coherent, unitary account of action, but should be seen as a more fragmentary, heterogeneous account which, although 'authorising' one version by the use of a number of textual devices, is nevertheless open to alternative readings. Likewise, Woolgar is concerned to display the way certain sorts of sequential constructions using a quasi-passive voice imply that new discoveries force themselves into the scientist's world; that they are come across, as it were, like driftwood on a beach. He does not talk of his experience but gives a detailed analysis of certain textual constructions.

Yearley is right that Woolgar and Smith do trade on certain implicit phenomenological assumptions. Yet these can be excised without the studies losing their interest. It is important to distinguish rigorous analysis of textual organisation and speculation about the effectiveness of texts. In each of these studies it is legitimate to ask: do readers of the illness account take K to be mentally ill? did hearers of Hewish's Nobel Lecture register the 'out-there-ness' of pulsars? Nevertheless Woolgar's and Smith's work does not become uninteresting because it does not answer these questions. They are not more fundamental than analyses of the organisation of the original texts, nor are they a prerequisite for such analysis.

The reason it is not possible to treat one of these questions as the more basic is that it is not possible to generate an absolute distinction between texts and readings. There is a considerable body of writing which suggests that all texts, not just those which explicitly pose as readings, are inescapably parasitical on previous texts. For instance, much of the force of S/Z lies in its attack on the realist notion of writing which treats texts as 'copies of the world' whose language gains its meaning through the process of denotation. Barthes' project is to show that such a view was untenable and that texts could only be made fully intelligible through referring to other signifying systems and in particular the 5 cultural codes. As Coward and Ellis put it, summarising this claim.

> Each text is suspended in the network of all others, from which it derives its intelligibility. Realism is 'a copy of a copy', supported by connotation, a 'perspective of citations'. It is silent quotation, without inverted commas, with no precise source.
> (23)

It is for this reason that many continental discourse analysts, including Barthes, Derrida, and Kristeva - who coined the notion - stress that 'intertextuality' is a basic condition of writing and reading, and that it is quite misleading to think of texts as autonomous[24]. From this perspective readings share the feature of being related to previous texts; they merely make the relation more explicit.

There is a second problem with the suggestion that a study of readings is necessary to be able to identify those features of the text which are effective in influencing readers. For this suggestion implies that readings are somehow transparent and straightforward in a way other texts are not. That is, it seems to take readings as literal reports of experiences rather than contextualised discursive products like any other. It is clear, however, from the above analysis that discourse which poses as a reading of a text is subject to the same variability as other forms of discourse. This variability comes as no surprise, of course, as even such 'professional' readers as literary critics ceaselessly produce varied interpretations of texts[25]. The basic problem is the same as with accounts of other scientific activity: if different, but equally plausible, versions are available how is the analyst to choose which one is the correct one? Or, in terms of readings, which reading is the one which displays the reader's genuine experience of the text? For example, in extract 6 above at least three different readings of a section of transcript are proposed. How, then, could we use these to legitimate the incisiveness of a particular analyst's reading?

This problem can be made manageable, I suggest, by dropping the unhelpful distinction between texts and readings. I have talked of readings throughout this chapter for reasons of convention. However, this, to Anglo-Saxon ears anyway, gives the misleading impression that what is being analysed is some subjective experience which accompanies scanning the text. In fact, what I have been calling readings are accounts of the meaning of a text made within a very specific social context. As such they are as open to analysis, or not, as any other accounts. Certain incidental features, such as brevity perhaps, may make readings appear to be easier to handle in practice. Yet these should not delude us into thinking that readings have any special transparency nor that they are somehow representations of inner experience. There is no a priori reason for thinking that the forms of sense making deployed in the discourse identified as 'readings' in this chapter will be very different from the kinds of discursive practices discussed in earlier chapters. At least at first, therefore, analysts should approach each form of discourse in the same way, attempting to expose their organisation and the varied means through which each constructs the social and natural world.

The next stage in the study of reading, then, - and here I am concurring with Yearley's conclusions[26] - should be to examine the organisation of readings in more detail. In the analysis above I have been concerned only with the structure of a particular kind of reading which uses the R/A device, and with showing some of the different ways in which it functions. What is needed is an examination which starts to show in what contexts these readings are used and what is achieved by their use. For example, it would be important to know in exactly what contexts participants may draw upon the R/A device and in what situations its use is effective. From this perspective reading becomes similar to other topics analysed in this thesis and elsewhere. The analytic goal is to document the variability and then start to make it intelligible according to the contingencies of particular interpretative situations. This approach would not make the problem of the effectiveness of texts disappear. But in lieu of a more adequate conceptualisation it transforms the question of reading into a manageable one which can be encompassed using the analytic procedures outlined in this thesis. The analysis of this chapter provides one example of this approach in practice; Yearley's studies of the reading of geological texts provides another[27].

In addition to these general suggestions, it is important that further studies transcend some of the limitations inherent in the present analysis. It is important to supplement the above findings, which are from an unusual and artifically constrained situation, with analyses of readings occurring in more natural contexts. Furthermore, I have only examined a very small number of readings which use the R/A device; although I have documented the use of a similar accounting technique in chapter seven. It may be also that Personal Construct Psychologists are an unusual group of scientists in that they give considerable emphasis in their writings to personal and reflexive concerns. However, this feature also makes them a particularly interesting group for study, because they have to face, in an unusually explicit form, problems of integrating what would traditionally be thought of as cognitive and social concerns.

CHAPTER EIGHT

SPEAKING AND WRITING SCIENCE

In this final chapter I want to move away from the discussion of specific analytic and theoretical issues which has occupied the last five chapters and return to more general questions concerning current and future developments in the study of scientific discourse. This will initially involve an examination of the way the notions of 'interpretative repertoire' and 'social context' are developed in this thesis and the way the findings of this research mesh with other work using these notions. Following that I will discuss the detailed interpretative procedures which have been documented here and comment on how their study could be fruitfully developed in the future. Finally I will address some of the critical points which have been directed at the programme of discourse analysis by sociologists of science and describe some of their shortcomings.

Repertoires and Contexts

The analytical chapters of this thesis provide further documentation of features of scientific discourse which have been explored by other researchers in other scientific specialties. In particular they give further evidence of the generality and pervasiveness of the contingent and empiricist repertoires in the accounting for scientific activity and belief. As should now be familiar, the empiricist repertoire treats scientific knowledge as dependent on the experimental disclosure of facts through the use of standardised techniques coupled with the application of impersonal rules and criteria. Except where personal and social factors interfere, theoretical interpretation is taken to be derived unproblematically from the facts. In contrast, the contingent repertoire allows that a variety of personal and social factors influence both the production of scientific facts and their theoretical interpretation. Intuitive and craft skills are seen as essential to research, whilst rules and procedures are not taken to be binding in themselves.

The use of these contrasting accounting systems is described in chapter 5, in which the analysis showed that the role of criteria or values in theory choice could be characterised in radically different ways. On the one hand, they were depicted as impersonal constraints which determined acts of theory choice and thereby belief in specific theories. In this view criteria are outside the sphere of scientists' social control and can be unproblematically applied to different scientific theories. Conversely, they were depicted as dependent on further interpretative work for their application and thus open to strategic and self-serving manipulation. In each case selections from the empiricist and contingent repertoires are used to give a strongly contrasting account of the operation of criteria.

The analysis in chapter 7 showed a similar bifurcation of accounting possibilities. In this case the participants were characterising actions performed at a conference session by way of a verbatim transcript of the session. While in some instances the transcript was taken as a document of rational discussion and clarification of theoretical issues, along with evaluations of the explanatory adequacy of theories; in others the same sections of transcript were taken to reveal social conflicts and psychological processes specific to the participants. The interest of this second example is that it shows how empiricist and contingent repertoires can be used at the level of detailed description of discourse. Clearly such processes are presupposed at some level by all the studies which have described the operation of these repertoires. For scientists' understandings of each others actions and beliefs are inevitably discursively mediated. Yet because scientists construct their accounts as if they were direct and privileged descriptions of the actions and beliefs of other scientists, making little reference to specific texts and talk, these studies do not explicate the procedures used <u>specifically</u> for characterising discourse.

In chapters 3 and 4 I dealt with the topic of the application of science. Although up to now studies of the contingent and empiricist repertoires have concentrated mainly on accounts of theoretical belief and its relation to research practices, there are strong similarities between the pattern of accounting in these areas and with respect to the practical utilisation of knowledge. Here one perspective depicts successful practice as straightforwardly based on correct theories, and assumes a generally strong relationship between knowledge and utility. Practical application is here taken to validate scientific knowledge. From the other perspective, the relation between science and utility is treated as circumspect and often erratic; non-scientific expertise is seen as necessary to make theories useful and the process of application is taken to be open to a variety of social contingencies. The two versions of the connection between knowledge and utility appear very similar to the contrasting ways in which the relationship between data and theory is characterised. While at its extreme the contingent account of scientific knowledge treats it as totally unconstrained by experimental findings, the strongest contingent versions of application treat science and utilisation as two totally unrelated and unconnected activities. It seems, therefore, that the empiricist and contingent repertoires might be usefully extended so that they cover the situation of knowledge utilisation as well as the purely scientific realm. The participants appear to use the same resources for each.

Another similarity between the different forms of utility accounting and the contingent and empiricist repertoires is the way that they are organised across different forms of scientific discourse. The formal research literature examined draws almost exclusively on the standard empiricist account of application. In the two analyses of informal discourse described in chapters 3 and 4 the style of accounting is different, both from the formal literature and from each other. In the interview with the social skills trainer discussed in chapter 3 the standard utility account is used, most commonly in general glosses on the nature of application. However, at other places in the interview it is readily abandoned in favour of more contingent accounts which stress the problematic nature of the science-utility relationship. In the conference transcript discussed in chapter 4 the standard account is maintained as primary throughout the interchanges analysed. In this case, however, the participants engaged in supplementary interpretations of their scientific actions in response to questions from others at the conference. Although these reinterpretations were made coherent with the standard account, they differed in important respects from the versions given at earlier stages in the proceedings and in the published papers.

In general, this is in line with the observations of Gilbert and Mulkay that empiricist accounts predominate in the formal literature while they are mixed with contingent accounts in less formal situations[1]. One question which arises, however, is why should the interviewee in chapter 3 readily abandon the standard account while the participants in chapter 4 attempts to maintain its primacy. A satisfactory answer to this must wait further research. Nevertheless, two possibilities suggest themselves. One is that the social skills trainer, as someone primarily concerned with practice and not, at the time of interviewing, conducting any theoretical research had no strong reason for depicting his research as completely dependent on theory. The contingent version of utility displayed the paramount contributions of the practitioner in interpreting and modifying theory and responding to practical circumstances. The conference participants, in contrast, were primarily engaged in research, seeing application as a job for others. Thus to display their research's importance and show the need for the theoretical niceties of each study, the standard account is drawn upon. This emphasises the essential role of the specific research findings while downgrading the contributions of the user. It also glosses over the question of exactly how the research is utilised. A second possibility is that the adversorial, semi-public situation of the conference led to a heavier stress on maintaining the general applied characterisation of the formal presentations and papers, even if this was at the expense of inconsistencies of other kinds.

In chapter 5, also, the pattern of contingent and empiricist accounting for the operation of criteria in theory choice parallels Mulkay and Gilbert's findings.

In this case only conference discourse was examined with the formal papers excluded from study. The pattern of contingent and empiricist accounting is here very similar to that identified in accounting for error[2]. The psychologists used the empiricist repertoire to characterise their own theory choices as clearcut and constrained by criteria while treating those of their opponents as socially contingent. This 1st person/3rd person asymmetry corresponds to the correct/incorrect asymmetry found in error accounting. For the psychologists treat their own theoretical views as correct and those of competitors as incorrect. They use empiricist accounts of criteria for dealing with 'correct' belief and contingent accounts for dealing with 'false' belief.

Taken together, then, the studies reported in this thesis provide further evidence for the existence of the two broad accounting repertoires and for their differential occurrence across varying contexts of discourse. It should be remembered that the number of instances which have been examined here is small and this should lead us to generalise only with caution. However, the fact that the same repertoires, organised in the same fashion, can be documented in a different specialty, using discourse drawn from different social contexts, concerned with different topics, suggests that these discursive phenomena may be recurrent in science.

At this point it is worth making a few comments about the notion of social context. One interpretation of the findings I have just discussed would say that the nature of scientific accounts is dependent on the nature of the social context, with the implication that the context <u>determines</u> the kind of discourse that is admissable. This sort of interpretation of the goal of analysis of scientific discourse has recently been made by Gieryn[3]. Moreover, he suggests that because the context and discourse are distinct, and the notion of context is used as an analytic resource, analysts must draw upon extra-discursive information concerning scientists' actions and beliefs to construct their analysis[4].

Gieryn's point would be a good one if context and discourse were actually distinct in this way. However, rather than conceptualising contexts as some sort of extra-discursive frame they are better seen as a way of characterising the systematic organisation of accounting. This means that context and discourse are not distinct entities; rather to refer to different contexts is to refer to systematic differences between forms of accounting. As Gilbert and Mulkay put it:

> It is not that different forms of discourse literally occur in or are determined by different social contexts. Rather, different social contexts are constituted through participants' selective employment of linguistic registers. (5)

Thus scientists draw differentially on the interpretative repertoires when they are giving accounts appropriate to specific contexts of discourse, and when they do so they help to reproduce the systematic features of those contexts. This kind of perspective on language usgage is elaborated helpfully by Halliday, particularly in his theory of 'register', although on occasion he formulates a more causal version of context dependence[6].

While on this topic it is worth examining another potential criticism of this approach. As I have noted earlier, Gilbert and Mulkay have characterised the contingent and empiricist repertoires as two separate linguistic registers which differ in stylistic and grammatical terms as well as lexical[7]. Coulthard has suggested, in relation to earlier work using the notion of register[8], that registers may simply be a circular way of noting change between topics. Thus, for example, he suggests that 'the language used in dressmaking patterns is the register of dressmaking and the register of dressmaking is that used in dressmaking patterns' giving an entirely vicious circle[9]. However, even if this criticism is applicable to some of Halliday's own formulations it cannot be sustained against the categorisation of scientific discourse into repertoires. To start with, there is no simple identification of 'science' with a particular register, classifying the language of scientists as the register of scientists[10]. Indeed, the identification of contingent and empiricist repertoires cuts right across such a simple-minded equation and is based on a detailed descriptive study of the way discourse is used in science. Secondly, there are instances where the difference between registers becomes a practical problem for participants rather than a solely analytic construction. Such situations occasion specific forms of accounting. For example Gilbert and Mulkay show that at certain junctures in interviews with scientists the empiricist and contingent repertoire may be employed together. At such times participants display evidence of interpretative difficulties and typically use a device which reconciles potential contradictions by stressing the long term paramountcy of empirical findings: the 'truth will out device'[11]. Phenomena of this kind would not be expected if registers were merely a circular, unheuristic redescription as Coulthard suggests.

Interpretative Procedures

Although one strand of this thesis has been concerned with documenting the way participants draw upon interpretative repertoires for characterising their own and others' actions and beliefs, another strand has been concerned to examine their more detailed interpretative practices. So far research on scientific discourse has tended to concentrate on describing the variable accounts which scientists produce rather than looking at the means through which that variability is achieved; although studies of this kind have begun to appear[12].

Heritage[13] has suggested that flexibility is a characteristic feature of all natural language use, but it appears particularly evident when actions and beliefs are characterised in terms of general accounting repertoires or broad interpretative schemes such as the standard utility account. If we take chapter 4, for instance, the variability there cannot be understood solely by reference to the general context of 'conference discourse'. It is, however, possible to make sense of this variability by making reference to the specific interpretative contexts in which the different accounts are produced. Thus although the general form of the standard utility account is maintained throughout the conference discussions analysed, perhaps because of its general legitimatory potential or because it meshes with the applied theme of the conference, the specific characterisations of the account are modified. For instance, if research is characterised as applicable because of its theoretical development, and then theoretical aspects of the research are undermined in discussion, its applied potential can be reasserted through characterising its empirical findings as utilisable. The initial formulation of the standard utility account becomes untenable through questioning and is replaced by a new version which is modified to accomodate to the points of the questioner. In this instance the specific interpretative context is an achievement of the conference participants (although, of course, they are drawing upon conventional resources to carry it off).

This process must be characterised with care. The aim is no more to suggest that the specific interpretative context acts as a causal constraint than it was for the general social context. Nevertheless, it is clearly an important consideration for the speaker. In the particular example I have been discussing (see pages 128-134) the speaker tried on a number of occasions to dispute the adequacy of a critic's version of his work. It is only when this produced no removal or modification of the criticisms that the speaker gave his alternative characterisation of his work's applicability. In general, as the discussion proceeds moment by moment, participants are both producing, reproducing and responding to the interpretative context. No one is in 'intentional control' of the context; yet neither does it have a determinate influence on their discursive products.

In this example it seems likely that the variation in accounts could be sustained without becoming a topic of note for the participants themselves because of the temporal separation of versions combined with their lack of opportunity to expose discourse to systematic analytic scrutiny. However, it is possible to identify procedures of accounting which deal more actively with the possibility of inconsistency between versions and facilitate the flexibility needed to successfully mesh discourse with changing interpretative contexts. One of these accounting procedures is progressive relexicalisation. This involves a sequential substitution of terms such that one description or version is replaced by another through a series of intermediate changes. The role of progressive substitutions appears to be to lessen the perceived contrast between alternative versions.

Only preliminary comments have been provided on relexicalisation in this thesis; however it appears that in the fast flow of conversation such procedures may be quite effective. For it is taxing the skills of participants to the limit to be able to explicitly formulate such gradual shifts in meaning as a topic for comment. It is interesting to note that the examples which Trew gives in his study of newspapers are from characterisations of events made in subsequent editions[14]. This change between texts probably reflects the special possibilities for rereading and the 'simultaneous presence' of written texts. Readers are unlikely to compare subsequent editions, whereas such changes within a particular article might become a topic for comment. A fruitful future line of study may be to examine sequences of scientific texts in which a particular experiment or theory is described to see if equivalent processes of relexicalisation take place. For instance, the accounts given in an original experimental report could be compared with accounts given in general reviews and then textbook representations of the experiment. All this is not, of course, to suggest that there are not modifications and conflicting versions within single texts. Indeed Yearley has clearly documented this phenomenon, noting for example the contrast between versions in stretches of discussion and their formulations in summaries and introductions[15]. However, relexicalisation in spoken discourse and between written texts is probably more straightforward to accomplish and more rarely treated as a topic salient to the concerns of participants.

A second interpretative procedure which promotes flexibility in accounting is what I have termed the Reality/Appearance device. In this case a shift between versions is accounted for by treating one as an appearance and the other as the reality. As I have noted in chapter 7, one version can be replaced by another sometimes highly divergent version by means of this device. For instance, a contingent account can be replaced by an empiricist account or vice versa. Instead of smoothing over the transition between versions, as is the case with relexicalisation, the R/A device explicitly formulates a sharp contrast between them. However, by formulating reality and appearance as alternative viewpoints[16] the device provides an account for the contrast between versions using the visual metaphor of 'distorted' appearances 'hiding' the 'real situation'.

This kind of accounting structure can be used with a prospective as well as a retrospective orientation. That is, it can be used to formulate future versions and undermine them in advance. I have called this accounting procedure 'preformulation' (see chapter 6). One of its crucial features is that it allows the speaker to formulate the criticised version in a way most suitable for its disposal and to create a sympathetic contrast with the version which is to replace it. What this procedure seems to achieve is an interpretative context in which it is very difficult to appropriately fit the preformulated version. For the production of such a version would be likely to involve not only replying to specific points of criticism but also accounting for the differences between the preformulated version of the speaker and the version produced in the reply. Preformulation generates a hostile environment, as it were, to place certain subsequent versions.

Clearly the work which has been done up to now hardly scratches the surface of the possibilities for studying the detail of scientists' accounting procedures. In particular, much of it has been concerned with the way scientists account for their social world, for instance in chapter 6 how they construct and modify versions of the categories of scientists within their discipline. However, it will be interesting in the future to examine in much more detail the accounts produced of particular research findings and of the detailed relation of these findings to theoretical interpretation. That is, it will be interesting to examine how particular formulations of data and theory are made out in terms of the empiricist repertoire. Furthermore, it seems likely that such an approach would benefit from a comparative study of realistic or 'factual' discourse whether it occurs in research reports, newspapers, novels or everyday conversations[17]. It may well be that modern work on 'realism' in literary theory, because it has not been held back by concerns with the referential status of language, can throw new light on scientists' procedures for making sense of the 'factual' world[18].

Positivism, Vassalage and Scottish Bluebells

In the final pages of this thesis I will respond to some of the various criticisms that have been made of the programme of discourse analysis by researchers concerned with social processes in science. With the details of the programme outlined and some specific examples of analysis to draw on it is possible to properly address the central points of criticism. These are: that discourse analysis claims an unproblematic and uninterpreted data base and thereby poses as a form of positivism; that discourse analysts' classifications of and approach to accounts necessarily presupposes that they go beyond discourse and use participants' knowledge of various kinds; and that discourse analysis on its own is not a sufficient approach to the social study of science.

The question of the status of discursive data has been raised by a number of commentators. Shapin[19] suggests that the claim that the discourse analysts 'is no longer required to go beyond the data'[20] is highly suspect. And Barnes, in a criticism of ethnomethodological approaches for features they share with discourse analysis, suggests that they should properly be thought of as an 'extreme form of positivism'[21]. Collins mirrors these points by suggesting that arguments for the necessity of analyses of discourse are dependent on the (mistaken) presupposition that discourse is a realm of 'pure data'[22].

These characterisations, however, do not do justice to the arguments which underpin the emphasis on discourse analysis as a fundamental approach. For the argument is not that discourse is somehow a realm of pure data - any such claim would indeed be very hard to sustain. Clearly when participants' discourse is being characterised in terms of different registers, as involving processes such as relexicalisation, or as using interpretative schemes like the standard utility account, it is being interpreted and ordered by the analyst. The sense in which the analyst is not going beyond the data is the limited one of not using discourse as simply a medium for reaching the actions and beliefs of scientists. That is, the analyst is not using an account of 'theory X', say, or of the nexus of socio-economic factors that led to its rejection, as evidence that the theory is as described and the socio-economic factors are such. Of course, traditional social analysts will not treat every account in this way and would readily accept that a certain degree of selectivity must go on when generating 'an overall reconstruction of what went on' in any particular scientific episode[23]. Nevertheless, they do presuppose that the construction of accounts of what went on is a manageable analytic problem, that by using processes such as triangulation from a number of data sources and eliminating unreliable responses such accounts can be achieved[24].

The argument against such a procedure is not an in principle one based on the fact that studies of this kind make inferences beyond the discourse. If discourse analysts were simply formulating an a priori critique, more traditional researchers could justifiably berate them for a form of groundless methodological purism akin to positivism. But this is not the crucial argument. Their critical stance with respect to attempts to construct definitive social accounts is based on an appreciation of the variability which is a pervasive feature of participants' discourse. The argument is that the proliferation of differing versions of scientific events, actions and beliefs in scientists' utterances and writing is such that dealing with it ceases to be a straightforward methodological problem[25].

All this is not to deny that analysts are able to produce unitary characterisations of events in science. Like the scientists under study they have a variety of resources at their disposal for dealing with variability. However, as I argued in chapter 1, when they do so it tends to be by ironising large tracts of participants' discourse while reifying others; or by selecting discourse from restricted social contexts in which only a subset of scientists' possible interpretative formulations appear. The suggestion that analysts should concentrate, at least initially, on the organisation and production of scientists' discourse thus need not be seen as a critique of 'positive theorising'[26] per se but rather a suggestion that while such theorising is grounded in inconsistent and unexplicated procedures for dealing with scientific discourse it is unsatisfactory[27]. This approach does not preclude the possibility of positive theorising; it merely wishes to place it on a firmer foundation.

Apart from depending on inconsistent and unexplicated procedures for constructing definitive versions, when traditional work treats certain accounts as literal and certain as ironical it begins to suffer from a second problem. For it moves away from the impartial stance often held as essential for studying science[28]. By selectively drawing upon discourse of certain kinds generated within particular social contexts (only from formal papers, say, or only from informal interviews) sociologists risk their conclusions becoming dependent on the interpretative procedures of scientists themselves[29].

To take an example from the present thesis, in chapter 5 I described Kuhn's account of the role of values in the selection of scientific theories. Values are treated as an important constraint on choice, although not a determinant. In chapter 1, I described Wynne's account of the selection of the ether theory. As is clear from the way scientists' formal papers are characterised, this treats overt values as being largely irrelevant to the selection process. However, in chapter 5, I showed that psychologists at a scientific conference characterise values in both of these ways, selecting one when they described the selection of their own (correct) theories and the other when they described the choice of their competitors' (false) theories. In Wynne's text accounts of both of these kinds are described - however, the 1st person accounts which treat values as central to choice are not taken to be literal statements of the way ether theorists chose their theory; while the 3rd personal accounts which describe the ether theory as not intelligible in terms of purely technical standards are incorporated as analytically unproblematic. The data base for Kuhn's study is not made explicit, but from the particular instances he cites - Copernicus and heliocentricism, Priestly and oxygen - it seems that it is based on historians' reconstructions of the selection of 'progressive' theories which formulate a set of rational grounds for choice. In each case there is a homology between the technical perspective on theory choice and the type of discourse which is taken to be acceptable evidence; the difference between Kuhn and Wynne becomes intelligible in terms of the alternative contexts from which data are selected and ultimately the different interpretative practices which define the nature of the context[30].

Mackenzie[31] has responded to a similar argument proposed by Woolgar[32] by suggesting that just because some of the explanatory categories used by analysts and participants are the same this does not mean that analysis will be flawed in any way or that the analysts' use cannot be kept separate from participants' use. He asks rhetorically:

> Ought the social historian delay any analysis of nineteenth-century society in terms of class until the uses of the term 'class' by members of that society are fully understood? (33)

Yet the shared use of particular terms is not the key issue; it is whether the explanatory work done by the term in the analysts' technical account can be distinguished from the work done by the participants. In Mackenzie's example, if the historian's version is built up using participants' accounts of class, or by using materials which were themselves constructed using the participants' concept, then an understanding of the meaning of the term would indeed be a precondition. Otherwise the status of the historical version will be undecidable. Moreover, the concept may be fitted to particular kinds of interpretative work in its original context - just as the categorisations 'humanist' and 'mechanist' were shown to be in chapter 6. Adopting the concepts as unproblematic may thus incorporate participants' interpretative work: the situation which Mulkay refers to as 'vassalage'.

In the light of this it is perhaps significant that those commentators from relativist, interest and normative approaches who have chosen to discuss the programme of discourse analysis have treated its claims as methodological stipulations abstracted from the analytic findings concerning variability. For by doing this their arguments appear most powerful. However, it must be emphasised that the force of the methodological suggestions cannot be appreciated in isolation from a consideration of actual instances. In Yearley's terms, the response to naturalistic approaches in the sociology of science is basically analytic and practical rather than derived from a critique of their conceptual coherence[35]. Nevertheless, given that the programme of discourse analysis is not attempting to infer actions and beliefs from discourse and thereby construct definitive versions[36] of events, the question remains of how discourse is interpreted.

In one sense the response to this question is relatively straightforward. It is interpreted in the ways displayed in the various studies of scientific discourse. This suggestion is not as empty as it appears. For one of the features of these studies is that they attempt to make the process of interpretation as explicit as possible. Typically this has involved presenting as much of the data as is manageable to the reader, and where selection does take place to make it as representative as possible of the variation in the materials studied. This is combined with a fine-grain approach which links interpretation to specific parts or features of the discourse. These procedures contrast markedly with the textual practice common in other approaches[37] as I have shown in chapter 1. They allow other analysts the opportunity of assessing the veracity of particular interpretations and contesting them if they wish. Collins notes that the kinds of extracts from participants' discourse used in these studies are open to alternative interpretations[38]. However, this possibility is nowhere denied[39]. What Collins, or any other critic, needs to do is demonstrate how an alternative interpretation can be sustained in practice and therefore how the kinds of variability and interpretative procedures identified by discourse analysts do not undermine the status of the critics' own studies. That is, simply to claim that discourse analysis is an interpretative exercise is insufficient; if they are to rebut criticisms they must undermine the specific interpretations which have been produced.

Given this point, what sorts of interpretations are produced by discourse analysts? One of Collins's claims is that a considerable amount of background knowledge is presupposed both of the nature of the English language and of details of the research specialty being studied[40]. Without this background knowledge and specific practical competence, it is argued, it would be impossible to separate renditions of the Bluebells of Scotland on a comb-and-paper from serious scientific contributions. That is, the high quality accounts could not be sifted from the low quality. Whilst accepting that a certain amount of background knowledge of English is presupposed - after all the writing of the papers themselves would be difficult without it - as well as some expectations about what is likely to be fruitful data, there seems to be no reason to canonise this as analytically special. In other words, there is no reason why the researcher should not attempt to expose ideas as to what accounts are of high quality, or who are scientists and who not, to analytic scrutiny. Thus if contributors to conferences did indeed play the Bluebells of Scotland on comb-and-paper, and if respondents then asked questions - about the methodology, perhaps, or theoretical presuppositions - then analysts would have to start treating it as an item of scientific interest. For instance, in chapter 5 a number of extracts are presented in which participants' dispute over who is acting scientifically and who is not. There is no reason why the analysts should try to settle this question; indeed there are very good reasons for not attempting to do so, as it would hardly be compatible with an impartial stance. Nevertheless, the way participants are made out as scientists or not is an interesting topic for study. Moreover, as Gilbert and Mulkay note, distinguishing what data might be relevant to the study of science is quite different from producing an adequate analytic account of that data. Indeed, this critique itself trades on the importance of an understanding of the patterned nature of participants' discourse[41].

A further question to do with the interpretation of discourse is of whether it is being treated as a form of action in itself, as a series of speech acts. This issue is of particular complexity, and can only be touched upon here. However, I think there are certain considerations which suggest that this, at least in its traditional form, is not an adequate way of viewing discourse analysis. This is not merely because defining speech acts involves a specification of a speaker's intention, which will be subject to the same methodological and practical difficulties as are involved in giving definitive versions of actions and beliefs lying 'beyond the text'. But such a specification would start to cut across the participants' procedures of reconceptualising and transforming the meaning and significance of events through time.

Jaques Derrida makes this point rather well in a critique of Austin and Searle's speech act philosophy[42]. He emphasises the 'iterability' of language in practice, that it continues to be meaningful in the absence of a specific speaker or determinate knowledge of the intentions 'lying behind it'. And he suggests that Searle's emphasis on conventions which ensure that discourse is interpreted successfully, and on felicity conditions which must be satisfied in the performance of speech acts, presupposes that communication is generally successful. Searle's position becomes a moral theory, claims Derrida, which treats certain kinds of discourse - insincere and unauthentic speech acts - as parasites, which are dependent for their meaning on the sincere forms. Much of Derrida's critique is directed towards demonstrating that 'serious, literal' speech acts are as much dependent upon humorous or parasitical forms as vice versa.

It is not easy to present a concise summary of Derrida's argument; for much of it depends on its style which continually parodies and disturbs the conventions he sees as underpinning Searle's writing. However, its general force is clear. Trying to assign a definitive action description to a specific section of discourse would be to freeze a continual process and formalise what is essentially practical; and to think of discourse as deriving its significance from an intention held by the individual speaker or writer is to uphold an idealised and unworkable conception of language. If Derrida is right - and here is obviously not the place for a thorough evaluation - the idea of discourse analysis identifying speech acts (or their written equivalent[43]) is misplaced and would be to try and constrain practical language use into an unwarranted formalism. These issues are clearly very complex - but as the programme of discourse analysis develops it seems likely that they will come increasingly to the fore.

Finally to the point that discourse analysis is not a sufficient approach to the study of science. This is made, in one way or another, by all the critics discussed above[44]. Yet, as I hope is now clear, one of the significant contributions of discourse analysis has been to reveal basic problems which underpin traditional approaches. Indeed, it has started to show how the conclusions of much of this work could be an unintended consequence of paying too little attention to the social generation of scientific discourse. To say that discourse analysis is not enough is to miss the point that traditional problems may well be insoluble without it. Of course it may also show that some problems have to be totally reconceptualised and others are not coherent problems at all, but are perhaps confusions based on an over-literal approach to scientists' interpretative formulations[45]. But that is in the nature of scientific progress!

APPENDIX A

INTERVIEW SCHEDULE: SOCIAL SKILLS TRAINING

A) Introduction and entry into the field.

1. How did the respondent (R) get into the area of social skills training (SST)? Was it through reading, friends, etc.?
2. Who else in the country does this sort of thing? Names and institutions?
3. How long has this field been going in clinical psychology?
4. Does the R work with a team of clinicians or alone?

B) The specific practice of SST.

1. How many patients does R normally treat in this way?
2. How often does R see these patients?
3. Get R to describe a normal session. How long does it take? Exactly what skills are taught?
4. How far is this procedure modified to fit each individual patient? Or is it modified around particular problem issues?
5. How much is construction, intuition and trial and error in each particular session?
6. How are patients selected for SST? Are these ideal?
7. How has R's practice been modified from its original form? Has this resulted in much improvement?
8. How is the impact of SST on any particular patient evaluated?

C) The use of theories in SST

1. What are the important theories used in SST? E.g. Argyle and Dean?
2. Does R consider these to be good or bad theories? Are they theories at all?
3. Does R consider himself to be applying these theories, even if only in some diffuse fashion? How exactly?
4. What exactly is the relation between these theories and R's practice?

5. Have changes in theory - e.g. of NVC - affected R's practice in any way? How exactly?
6. Are there different approaches to SST? What distinguishes these approaches? Is it anything to do with different theories?
7. Might these different approaches relate to the 'background assumptions' of the researcher, e.g. might, say, a behavioural psychotherapy approach lead to certain interpretations of the function of SST?
8. Are theories involved in the evaluation of the efficacy of SST?

D) <u>Literature and communication</u>.

1. Who are the key figures in SST in this country? Abroad?
2. Does R make distinctions between clinicians and theorists?
3. Which of 1 are theorists? Could SST have got off the ground without their contribution?
4. Does R contribute to the literature on SST? Does this contribution lead towards the more applied or more theoretical end of the spectrum? Try to get some references.
5. What is the basic introductory literature to SST?
6. What sorts of things does R read to keep abreast with new developments?
7. Does R have informal communication with others in the area to discuss new techniques etc.? What form does this communication take and with whom?

E) <u>General perspective on application</u>

1. How useful has SST been?
2. Is SST considered to be an important part of what clinical psychologists do?
3. Have the results and experience of SST had any input back into theories of NVC and skills? How exactly?
4. Should the results of clinical practice have an important role to play in evaluating theory?
5. Does R see there being ethical or political problems with the application of social psychological theories in SST?

APPENDIX B

THE SECOND FORMAL CAKE ACCOUNT

...I follow Janet Hurst (1978), and almost every other contributor to this volume, in classifying psychologists into two opposing camps, the humanists and the mechanists [] The differences between these two are very great at a theoretical, even ideological level. Humanists tend towards 'idealism': that is, they believe that the essence of knowledge lies within the ideas which we have about the nature of the world. Mechanists tend towards 'realism': that is, they believe that the essence of knowledge is in the nature of the real world and that we possess knowledge only when our ideas match that reality. Humanists prefer 'mentalist' and 'holist' explanations. Mechanists prefer 'materialist' and 'reductionist' explanations. Humanists believe in 'free will', mechanists deny its existence. Humanists place great reliance on 'introspective evidence', while mechanists often adopt a dogmatic 'behaviourist' line. Humanists are pre-occupied with human values and purposes. Mechanists spend their time investigating human limitations and resources.

I would not expect any single psychologist to meet all these defining characteristics of either mechanist or humanist psychology. Nevertheless these caricatured positions do correspond to the main ideological and theoretical divisions among psychologists today, and these divisions are often very unsympathetic to each other. Humanists claim, with some justification, that mechanists provide only an impoverished account of human nature. This may be because they deliberately cut themselves off from the rich descriptions of human nature which are to be found in the wider humanist tradition in our culture, which is of course not all psychological. Mechanists claim, again with some justification, that humanists are careless in their attitudes to evidence. This may be a consequence of taking an idealist view of the nature of knowledge.

In what follows, let me try to demonstrate that humanists and mechanists differ much less in practice than they do in theory...

NOTES AND REFERENCES

CHAPTER ONE: SCIENTIFIC PRACTICE AND DISCOURSE

1 Some general reviews of the field are given in: K. Knorr-Cetina and M. Mulkay (1983) Science Observed: Contemporary Analytic Perspectives, London: Sage; M. Mulkay (1979) Science and the Sociology of Knowledge, London: Allen Unwin; M. Mulkay and V. Milic (1980) 'The sociology of science in East and West', Current Sociology, 28, 1-342; S. Shapin (1982) 'History of science and its sociological reconstructions', History of Science, 20, 157-211; I. Spiegel-Rosing and D. de Solla-Price (1977) Science, Technology and Society, London: Sage.

2 The variety of different kinds of quantitative studies in this field, and some of their problems, are helpfully discussed in: D. Edge (1979) 'Quantitative measures of communication in science: A critical review', History of Science, 17, 102-134.

3 H. White, D. Sullivan and E.J. Barboni (1979) 'The interdependence of theorv and experiment in revolutionary science: The case of parity violation', Social Studies of Science, 9, 303-329.

4 Ibid., 304.

5 Ibid., 323.

6 Ibid., 322.

7 Ibid., 322.

8 Ibid., 324.

9 Ibid., 318.

10 Ibid., 324-325.

11 M. Mulkay (1974) 'Methodology in the sociology of science', Social Science Information, 13, 107-119; Halliday, M.A.K. (1978) Language as Social Semiotic, London: Arnold; S. Woolgar (1980) 'Discovery: Logic and sequence in a scientific text', in K. Knorr-Cetina, R. Krohn and R. Whitley (eds.) The Social Process of Scientific Investigation. Dordrecht: Reidel.

12 I. Lakatos (1970) 'Falsification and the methodology of scientific research programmes', in I. Lakatos and A. Musgrave (eds.) Criticism and the Growth of Knowledge, Cambridge: Cambridge University Press.

13 White et al., op. cit. note 3, 323.

14 D. Edge and M. Mulkay (1976) Astronomy Transformed. New York: Wiley.

15 White et al., op. cit. note 3, 323.

16 Ibid., 305.

17 Ibid., 306.

18 Ibid., 306.

19 Ibid., 307.

20 Ibid., 307.

21 Ibid., 307.

22 Ibid., 307.

23 Ibid., 308. White et al. mention a further category of 'applications of general theory' but do not discuss this in detail.

24 G.N. Gilbert (1976) 'The transformation of research findings into scientific knowledge', Social Studies of Science, 6, 281-306; G.N. Gilbert and M. Mulkay (1980) 'Contexts of scientific discourse: Social accounting in experimental papers', in k. Knorr-Cetina et al., op. cit. note 1; see also chapter's 4 and 5 above.

25 White et al., op. cit. note 3, 306.

26 Ibid., 306.

27 Ibid., 325.

28 M. Mulkay (1981) 'Action, belief or scientific discourse', Philosophy of the Social Sciences, 11, 163-171.

29 White et al., op. cit. note 3, 322.

30 G.N. Gilbert (1977) 'Referencing as persuasion', Social Studies of Science, 7, 113-122.

31 S.W. Woolgar (1976) 'Writing an intellectual history of scientific development: The use of discovery accounts', Social Studies of Science, 6, 395-422.

32 White et al., op. cit. note 3, 318.

33 Ibid., 318.

34 Ibid., 319.

35 Ibid., 319.

36 Ibid., 319.

37 Ibid., 305.

38 G.N. Gilbert and M. Mulkay (1981) 'Experiments are the key: A preliminary analysis of scientific history making' presented at the Society for Social Studies of Science Conference, Atlanta, Georgia; November.

39 T.F. Gieryn (1982) 'Relativist/constructivist programmes in the sociology of science: Redundance and retreat', *Social Studies of Science*, *12*, 279-297.

40 For instance: "there is now a real interest in our natural knowledge as a product of our way of life, as somethino we have *constructed* rather than something which has been, so to speak, revealed to us." B. Barnes and S. Shapin (1979) 'Introduction', in B. Barnes and S. Shapin (eds.) *Natural Order: Historical Studies of Scientific Culture*, London: Sage. See also B. Barnes (1977) *Interests and the Growth of Knowledge*, London: Routledge.

41 In this section I occasionally draw upon the joint work B. Latour and S. Woolgar (1979) *Laboratory Life: The Social Construction of Scientific Facts*, London: Sage. I do not, however, take this work to be homogenously constructivist; indeed, in places the text appears to adopt an approach similar to that used throughout this thesis.

42 Sellars and Suppe, cited in K. Knorr-Cetina (1981) *The Manufacture of Knowledge*, Oxford: Pergamon.

43 Ibid., 1.

44 Ibid., 3. See also K. Knorr (1977) 'Producing and reproducing knowledge: Descriptive or constructive', *Social Science Information*, *16*, 669-696.

45 R. Bhaskar (1975) *A Realist Theory of Science*, Brighton: Harvester.

46 Latour and Woolgar, op. cit. note 41, 174-183.

47 B. Latour (1980) 'The three little dinosaurs or a sociologist's nightmare', *Fundamenta Scientiae*, *1*, 79-85.

48 Knorr-Cetina, op. cit. note 42, 3.

49 R. Harre, Preface to Knorr-Cetina, op. cit. note 42, viii.

50 R. Harre (1979) *Social Being*, Oxford: Blackwell. See J. Potter, P. Stringer, and M. Wetherell (1983) *Social Texts and Context: Literature and Social Psychology*, London: Routledge, for a critique of this book from the perspective adopted in this thesis.

51 Knorr-Cetina, op. cit. note 42, 5. See also Knorr-Cetina, op. cit. note 44.

52 Latour and Woolgar, op. cit. note 41.

53 Knorr-Cetina, op. cit. note 42, 20-21.

54 There is no space here for a thorough deconstruction of Knorr-Cetina's textual practice. Yet even a cursory inspection shows such metaphors to be a recurrent feature of her discourse. This stress on the events in the lab at the expense of wider social pro-

cesses appears somewhat at odds with her claim to be integrating the micro and macro features of social life: K. Knorr-Cetina (1982) 'Scientific communities or transepistemic arenas of research', Social Studies of Science, 12, 101-131; K. Knorr-Cetina and A. Cicourel (eds.) (1982) Advances in Social Theory and Methodology, London: Routledge.

55 Latour and Woolgar, op. cit. note 41, chapter 3.

56 G.N. Gilbert and M. Mulkay (1983) Opening Pandora's Box: Sociological Analysis of Scientists' Discourse, Cambridge: Cambridge University Press.

57 Latour and Woolgar, op. cit. note 41, 28-29. These suggestions are attributed to J. Ravetz (1971) Scientific Knowledge and its Social Problems, Harmonsworth: Penguin.

58 K. Knorr-Cetina (1983) 'The ethnographic study of scientific work: Towards a constructivist interpretation of science', in Knorr-Cetina and Mulkay, op. cit. note 1, 2-3 (mimeo).

59 See Harre, op. cit. note 50 and Potter et al., op. cit. note 50, chapter 4.

60 Knorr-Cetina, op. cit. note 42, 21.

61 E.g. Knorr-Cetina, op. it. note 42, chapter 5; K. Knorr-Cetina (1978) 'From scenes to scripts: On the relationship between research and publication in science', Research Memorandum 132, Vienna: Institute of Advanced Studies.

62 E.g. Knorr-Cetina, op. cit. note 42, chapters 2-4 and K. Knorr-Cetina (1980) 'The scientist as an analogical reasoner: A critique of the metaphor theory of innovation', in Knorr-Cetina et al., op. cit. note 1.

63 K. Knorr-Cetina (1979) 'Tinkering towards success: Prelude to a theory of scientific practice', Theory and Society, 8, 347-76.

64 There is no in principle reason, of course, why these data ought not to be called observations. However, as I have noted, a lot of Knorr-Cetina's methodological support comes from the way she characterises her work as if it takes the form of a close visual study. It seems very far from this when it is compared to some of the detailed, moment by moment action descriptions used by M. Lynch, E. Livingstone, and H. Garfinkel (1983) 'Temporal order in laboratory work', in Knorr-Cetina and Mulkay, op. cit. note 1.

65 B. Latour (1980) 'Is it possible to reconstruct the research process? Sociology of a brain peptide, in Knorr-Cetina et al., op. cit note 11.

66 Ibid., 53. Latour mentions Collins, Latour, Knorr-Cetina, Pinch, Harvey and Callon as supporting this claim.

67 My comments about the hetrogeneous nature of the research process can be applied equally to Latour's notion of the idiosyncracy of research. The section on research as fiction building is not strictly relevant here as it takes us away from the ethnographic study of laboratory pracitices.

68 Latour, op. cit. note 65, 56.

69 Latour and Woolgar, op. cit. note 41, 107-112.

70 Latour, op. cit. note 65, 56.

71 Ibid., 56.

72 M. Mulkay (1976) 'Norms and ideology in science', Social Science Information, 15, 637-656; J. Potter (1982) 'Nothing so practical as a good theory: The problematic application of social psychology', in P. Stringer (ed.) Confronting Social Issues: Applications of Social Psychology, London: Academic Press. See also chapters 3 and 4 above.

73 Gilbert and Mulkay, op. cit. note 56, as well as chapter 6 above.

74 Latour, op. cit. note 65, 57.

75 Ibid., 57.

76 Ibid., 57.

77 Ibid., 57.

78 M. Mulkay and G.N. Gilbert (1982) 'What is the ultimate question? Some remarks in defence of the analysis of scientists' discourse', Social Studies of Science, 12, 309-320; Gilbert and Mulkay, op. cit. note 56; and J. Potter and M. Mulkay (1983) 'Scientists' interview talk: Interviews as a technique for revealing participants' interpretative practices', in M. Brenner, J. Brown, and D. Canter (eds.) The Research Interview (Uses and Abuses), London: Academic Press.

79 Latour, op. cit. note 65, 69.

80 This is a recurrent finding of studies of scientists' discourse.

81 Latour, op. cit. note 65, 60.

82 Ibid., 60.

83 Gilbert and Mulkay, op. cit. note 24 and Gilbert and Mulkay, op. cit. note 56, chapter 3.

84 See, for example, M. Mulkay (1980) 'Interpretation and the use of rules: The case of norms in science', in T.F. Gieryn (ed.) Science and Social Structure, Transactions of the New York Academy of Sciences, Series III,

39, New York, 111-125; M. Mulkay and G.N. Gilbert (1981) 'Putting philosophy to work: Karl Popper's influence on scientific practice', *Philosophy of the Social Sciences*, *11*, 389-407; J. Potter (forthcoming) 'Testability, flexibility: Kuhnian values in scientists' discourse concerning theory choice, *Philosophy of the Social Sciences*.

85 D. Bloor (1976) *Knowledge and Social Imagery*, London: Routledge.

86 B. Barnes (1974) *Scientific Knowledge and Sociological Theory*, London: Routledge; Barnes, op. cit. note 40; B. Barnes, (1982) *T.S. Kuhn and Social Science*, London: Macmillan.

87 Bloor, op. cit. note 85.

88 For instance Barnes and Shapin, op. cit. note 40 and Shapin, op. cit. note 1.

89 For example: M. Hesse (1980) *Revolutions and Reconstructions in the Philosophy of Science*, Brighton: Harvester; Laudan, L. (1981) 'The pseudo-science of science', *Philsophy of the Social Sciences*, *11*, 173-198; H. Meynell (1977)'On the limits of the sociology of knowledge', *Social Studies of Science*, *7*, 489-500; E. Millstone (1978) 'A framework for the sociology of knowledge', *Social Studies of Science*, *8*, 111-125; Freudenthal, G. (1979)'How strong is Dr Bloor's "Strong Programme"?', *Studies in the History and Philosophy of Science*, *10*, 67-83; A. Gellatly (1980) 'Logic, necessity and the strong programme for the sociology of knowledge', *Studies in the History and Philsophy of Science*, *11*, 325-339.

90 S. Woolgar (1981) 'Interests and explanation in the social study of science', *Social Studies of Science*, *11*, 365-394, and S. Yearley (1982) 'The relationship between epistemological and sociological cognitive interests', *Studies in the History and Philosophy of Science*, 13, 353-388.

91 M. Hesse (1974) *The Structure of Scientific Inference*, London: Macmillan.

92 W.V.O. Quine (1953) 'The two dogmas of empiricism', in *From a Logical Point of View*, New York: Harper Torchbooks; P. Duhem (1962) *The Aim and Structure of Physical Theory*, New York: Atheneum. See also W.V.O. Quine and J.S. Ullian (1970) *The Web of Belief*, New York: Random House.

93 It is worth noting that this position is not without its critics within philosophy. For instance, Dummett has argued that this model ends up by underdetermining its own internal structure, because the idea that any statement can be retained in the face of recalcitrant experience suggests that there is no content to the notion of a periphery and a centre of the network. Moreover, the logical connectives which make up the

structure of the network become as susceptible to to revision as any other statement. Taken seriously, the network model can thus turn itself into a formless, structureless fog. M. Dummett (1973) Frege: Philsophy of Language, New York: Harper and Row.

94 These coherence conditions are intimately associated with criteria for theory selection. Both stress the significance of conventions and the underdetermination of choices by observations. Quine proffers 5 criteria: simplicity, conservatism, generality, modesty and refutability (Quine and Ullian, op. cit. note 92). These can be compared with the 5 criteria suggested by Kuhn for dealing with the same problem: accuracy, consistency, scope, simplicity, frutifulness; T.S. Kuhn (1977) 'Objectivity, value judgment and theory choice', in The Essential Tension, London: University of Chicago Press.

95 B. Barnes (1981) 'On the conventional character of knowledge and cognition', Philsophy of the Social Sciences, 11, 303-333, 316 (author's emphasis).

96 Hesse, op. cit. note 89 and 91 and M. Hesse (1978) 'Theory and value in the social sciences', in C. Hookway and P. Pettit (eds.) Action and Interpretation: Studies in the Philsophy of Social Sciences, Cambridge: Cambridge University Press.

97 D. Bloor (1982) 'Durkhiem and Mauss Revisited; Classification and the sociology of scientific knowledge', Studies in the History and Philosophy of Science, 13. The quote is from pages 22-23 of the draft manuscript.

98 Barnes, op. cit. note 86 (Kuhn book) 22.

99 Barnes, op. cit. note 95, 325. Barnes is not always consistent on this point; sometimes he suggests that pure instrumentality is a possibility - see Yearley, op. cit. note 90, 357-361.

100 Barnes and Shapin, op. cit, note 40, 9-13; Bloor, op. cit. note 85, 4.

101 The fourth tenet will not be discussed here as it plays little part in the analytic practice of interest theorists.

102 D. Mackenzie (1978) 'Statistical theory and social interests: A case study, Social Studies of Science, 8, 35-83.

103 B. Wynne (1979) 'Physics and psychics: Science, symbolic action and social control in late Victorian England', in Barnes and Shapin, op. cit. note 40.

104 Bloor, op. cit note 97.

105 Shapin, op. cit. note 1.

106 B. Barnes and D. Edge (eds.)(1982) Science in Context. Milton Keynes; Open University Press.

107 Barnes, op. cit. note 86 (Kuhn book) 116.

108 Wynne, op. cit. note 103, 176.

109 Ibid., 168 and 181.

110 Ibid., 168.

111 M. Douglas (1975) *Implicit Meanings*, London: Routledge.

112 Wynne, op. cit. note 103, 184.

113 In this claim Wynne is in accord with studies of rule use and criteria which stress the underdetermination of practices by rules: e.g. Mulkay, op. cit. note 84; Mulkay and Gilbert, op. cit. note 84; Potter, op. cit. note 84; D.L. Weider (1974) 'Telling the code', in R. Turner (ed.) *Ethnomethodology*, Harmonsworth: Penguin; A.J. Wootten (1975) *Dilemmas of Discourse*, London: Allen and Unwin. This argument is also developed in Barnes's 'finitist' account of concept application, Barnes, op. cit. note 86 (Kuhn book). Wynne himself elsewhere argues strongly for an interpretative link between norms and practices: (1979) 'Between orthodoxy and oblivion: The normalisation of deviance in science', in R. Wallis (ed.) *On the Margins of Science: The Social Construction of Rejected Knowledge*, University of Keele, Sociological Review Monograph 27.

114 Woolgar, op. cit. note 90, Yearley, op. cit. note 90.

115 Wynne, op. cit. note 103, 169.

116 Ibid., 181.

117 Ibid., 181.

118 Ibid., 170-171.

119 Ibid., 171.

120 M. Mulkay and G.N. Gilbert (1982) 'Accounting for error: How scientists construct their social world when they account for correct and incorrect belief', *Sociology*, *16*, 165-183.

121 Wynne, op. cit. note 103, 170-172.

122 E.g. Woolgar, op. cit. note 11; S. Yearley (1981) 'Textual persuasion: The role of social accounting in the construction of scientific arguments', *Philosophy of the Social Sciences*, *11*, 409-435; Potter et.al. op. cit. note 50. More generally, work by post-structuralists and deconstructionists is relevant here: C. Belsey (1981) *Critical Practice*, London: Methuen; J. Culler (1982) *The Pursuit of Signs: Semiotics, Literature, Deconstruction*, London: Routledge; (1983) *On Deconstruction*, London: Routledge; J.V. Harari (ed.) (1979) *Textual Strategies*, London: Methuen; C. Norris (1982) *Deconstruction: Theory and Practice*, London: Methuen; S.R. Suleiman and I. Crosman (eds.)(1980)

The Reader in the Text, Princeton: Princeton University Press.

123 S. Yearley (1982) 'Contexts of evaluation: A sociological analysis of scientific argumentation with reference to the history of earth science', unpublished DPhil. thesis, University of York.

124 Wynne, op. cit. note 103, 177.

125 Ibid., 179.

126 For instance, Wynne spends considerable time emphasising the 'metaphysical' nature of the Cambridge physics. This allows him to strongly contrast the 'non-metaphysical' naturalistic views of nature. However, this contrast is not so easily made. In one extract (171) for example, a Cambridge physicist appears to identify natural selection as metaphysical - and this is taken as naturalism par excellence in other papers in the Natural Order collection.

127 Wynne, op. cit. note 103, 175-176.

128 Ibid., 173.

129 Ibid., 178.

130 Ibid., 184.

131 H.M. Collins (1983) 'An empirical relativist programme in the sociology of scientific knowledge', in Knorr-Cetina and Mulkay, op. cit. note 1.

132 H.M. Collins (1982) 'Special relativism - the natural attitude', Social Studies of Science, 12, 139-144.

133 Ibid., 140.

134 H.M. Collins (1981) 'What is TRASP? The radical programme as a methodological imperative', Philosophy of the Social Sciences, 11, 215-224, 122.

135 Bloor, op. cit. note 85, 5.

136 Collins, op. cit. note 134.

137 Ibid., 218 (author's emphasis).

138 H.M. Collins (1981) 'Stages in the empirical programme of relativism', Social Studies of Science, 11, 3-11.

139 T.S. Kuhn (1962) The Structure of Scientific Revolutions, Chicago: University of Chicago Press, chapters 3 and 4.

140 Collins, op. cit. note 131.

141 Collins, op. cit. notes 131 and 138.

142 Collins, op. cit. note 131, 15 (draft chapter).

143 K. Knorr-Cetina (1982) 'Relativism - what now?', Social Studies of Science, 12, 133-136; Collins, op. cit. note 132.

144 Hesse, op. cit. note 89.

145 Knorr-Cetina, op. cit. note 143, 134.

146 Collins, op. cit. note 132, 141.

147 H.M. Collins (1981) 'Son of seven sexes: The social destruction of a physical phenomenon, Social Studies of Science, 11, 33-63.

148 H.M. Collins (1975) 'The seven sexes: A study in the sociology of a phenomenon, or the replication of experiments in physics', Sociology, 9, 205-224.

149 Collins, op. cit. note 147, 34.

150 Ibid., 44.

151 Ibid., 36.

152 Collins, op. cit. note 132, 141.

153 Collins, op. cit. note 131.

154 Collins, op. cit. note 147, 48-49. This claim is stated in even stronger terms in the introduction to the whole collection: '...one mechanism of closure... namely, the use of rhetorical and presentational devices by [Quest's] group of experimenters to make their own interpretation of the experimental series the one credible possibility' is discussed; Collins, op. cit. note 138, 5.

155 There is, of course, a prior question concerning whether the controversy was 'ended' or 'closed down' at all. Collins assumes this throughout, although he presents no data to show it, and clearly at least one of his central participants (Joseph Weber) is not treating it as closed down.

156 H.M. Collins (1981) 'Respondent's talk and participatory research' presented at the Accounts of Action Conference, University of Surrey, December, 10.

157 Although this participation is more extensive in the ideal case, Collins notes that it often must take the restricted form of interviews conducted 'with the object of full participation in mind' (Collins, op. cit. note 131). The latter is true of the 'Son of Seven Sexes' study - as yet Collins has not conducted gravity wave experiments or published in this field.

158 Collins, op. cit. note 147, 46.

159 Ibid., 47.

160 Ibid., 47.

161 Ibid., 47.

162 Ibid., 43.

163 Ibid., 34.

164 Collins seems to have adopted certain participants' versions of consensus in an unreflexive manner here. See chapter 6 above and Gilbert and Mulkay op. cit. note 56.

165 Collins, op. cit. note 147, 43.

166 Ibid., 43.

167 Gilbert and Mulkay, op. cit. note 120.

168 Collins, op. cit. note 148, 216.

169 Collins, op. cit. note 147, 47.

170 Even though this account is central to Collins's presentation it is far from clear that it is necessary to read it in the way Collins does. To be sure it describes the work as 'no longer physics', but this is immediately qualified as not lookino for gravity waves but criticising Weber. It is not hard to imagine Quest justifying this by recourse to some Popperian criterion of falsification. The debate over the existence of gravity waves might then become a debate over the proper application of Popperian rules! See Mulkay and Gilbert, op. cit. note 84.

171 Collins, op. cit. note 147, 47.

172 M. Mulkay, J. Potter and S. Yearley (1983) 'Why an analysis of scientific discourse is needed', in Knorr-Cetina and Mulkay op. cit. note 1.

173 Collins, op. cit. note 156.

174 Collins, op. cit. note 134, 218.

175 Ibid., 223.

176 Ibid., 223.

177 Ibid., 216.

178 This is implied by Barnes, op. cit. note 98, 97 and Shapin, op. cit. note 88, 200.

179 See chapter 7 above.

180 The notion of interpretative context will be elaborated in a number of the following chapters.

181 See note 122 and C. Bazerman (1981) 'What written knowledge does: Three examples of academic discourse', <u>Philosophy of the Social Sciences</u>, <u>11</u>, 361-387; J. Gusfield (1976) 'The literary rhetoric of science: Comedy and pathos in drinking driver research, <u>American Sociological Review</u>, 41, 16-34.

182 J. Heritage (1978) 'Aspects of the flexibilities of natural language use', *Sociology*, *12*, 79-105.

183 See D. O'Keefe (1979) 'Ethnomethodology', *Journal for Theory of Social Behaviour*, *9*, 187-219.

184 H. Garfinkel, M. Lynch and E. Livingstone (1981) 'The work of a discovering science construed with the materials from the optically discovered pulsar', *Philosophy of the Social Sciences*, *11*, 131-158; Lynch et al., op. cit. note 64; M. Lynch (1979) 'Art and artifact in laboratory science: A study of shop work and shop talk in a research laboratory', unpublished PhD thesis, University of California, Irvine; S. Woolgar (1981) 'Critique and criticism: Two readings of ethnomethodology', *Social Studies of Science*, *11*, 504-515.

185 Lynch et al, op. cit. note 64.

186 Ibid., 1 (draft chapter).

187 Ibid., 4.

188 Ibid., 5-6.

189 Ibid., 26.

190 Lynch, op. cit. note 184, 401.

191 Lynch et al., op. cit. note 184, 43. Lynch et al. also include Mulkay and Gilbert op. cit. note 120, however!

192 Lynch et al., op. cit. note 184, 32-33.

193 In practice it may be that Lynch et al. stick closer to observational data than Knorr-Cetina or Latour. However, they do not avoid the problems which are faced in principle by observational data. See pages 14-19 above.

194 Gilbert and Mulkay, op. cit. note 78.

195 Collins, op. cit. note 148; H.M. Collins (1976) 'Upon the replication of scientific findings: A discussion illuminated by the experiences of researchers into parapsychology' presented at the 4S/ISA First International Conference on Social Studies of Science, Cornell, November.

196 See also Mulkay and Gilbert, op. cit. note 84.

197 Garfinkel et al., op. cit. note 184.

198 Ibid., 131.

199 My critique is similar to that proposed by Collins op. cit. note 131, 29-30 (draft chapter); however, my conclusions are rather different.

200 A. Brannigan (1981) *The Social Basis of Scientific Discoveries*, Cambridge: Cambridge University Press.

201 Collins, op. cit. note 131.

202 Lynch et al., op. cit. note 184, 28.

203 Gilbert, op. cit. note 24.

204 E.g. D. Smith (1978) 'K is mentally ill: The anatomy of a factual account', Sociology, 12, 23-55.

205 See H. Garfinkel (1967) Studies in Ethnomethodology, New Jersey: Prentice Hall, and B. Barnes and J. Law (1976) 'Whatever should be done with indexical expressions', Theory and Society, 3, 223-237 for discussions of the concept of indexicality.

206 Woolgar op. cit. note 31.

207 Mulkay, et al., op. cit. note 172.

208 This is not, of course, my original claim. Early studies which have emphasised the centrality of discourse include Gusfield, op. cit. note 181; Mulkay, op. cit. note 72; Woolgar, op. cit. note 11. See also Mulkay, op. cit. note 28 and Gilbert and Mulkay, op. cit. note 78.

CHAPTER TWO: ANALYTIC PRELIMINARIES

1 J.M. Ziman (1967) Public Knowledge: The Social Dimension of science, Cambridge: Cambridge University Press.

2 See chapter 6.

3 I. Lubek (1976) 'Some tentative suggestions for analysing and neutralising the power structure in social psychology', in L.H. Strickland, F.E. Abaud, and K.J. Gergen (eds.) Social Psychology in Transition, New York: Plenum.

4 Ibid., 322.

5 A. Koestler (1972) The Call Girls, London: Pan.

6 Ibid., 16.

7 There have been some papers published concerned with the role of conferences as part of the communication system of science: G. Zamora and A. Adamson (eds.) (1981) Conference Literature: Its Role in the Distribution of Information, New York: Irington, as well as some 'humorous' papers: e.g. J.R. Edwards (1982) 'The psychology of the conference', Bulletin of the British Psychological Society, 35, 89-91.

8 M. Lynch (1979) 'Art and artifact in laboratory science: A study of shop work and shop talk in a research laboratory', unpublished PhD thesis, Univiersity of California, Irvine.

9 H. Garfinkel, M. Lynch and E. Livingstone (1981) 'The work of a discovering science construed with materials from the optically discovered pulsar', Philosophy of the Social Sciencs, 11, 131-158.

10 R. Williams and J. Law (1980) 'Beyond the bounds of credibility', Fundamenta Scientiae, 1, 295-315.

11 Koestler, op. cit. note 5, 106.

12 G.N. Gilbert (1980) 'Being interviewed: a role analysis', Social Science Information, 19, 227-236.

13 J. Potter and M. Mulkay (1983) 'Scientists' interview talk: Interviews as a technique for revealing participants' interpretative practices', in M. Brenner, J. Brown and D. Canter (eds.) The Research Interview (Uses and Abuses), London: Academic Press.

14 Ibid..

15 Authentic here just means that the participants are accepted as having the status of knowledgable members. See H.M. Collins 'An emprical relativist programme in

the sociology of scientific knowledge', in K. Knorr-Cetina and M. Mulkay (1983) *Science Observed: Contemporary Analytic Perspectives*, London: Sage.

16 M. Mulkay (1974) 'Methodology in the sociology of science', *Social Science Information*, *13*, 107-119.

17 See H.M. Collins and T.J. Pinch (1982) *Frames of Meaning: The Social Construction of Extraordinary Science*, London: Routledge.

18 R.S. Anderson (1981) 'The necessity for field methods in the study of scientific research', in E. Mendelsohn and Y. Elkana (eds.) *Sciences and Cultures*, Dordrecht: Reidel.

19 Koestler, op. cit. note 5, 100.

20 G.N. Gilbert and M. Mulkay (1982) *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse*, Cambridge: Cambridge University Press.

21 Even written papers, when examined closely, can be seen to embody forms of social accounting. See for instance: G.N. Gilbert and M. Mulkay (1980) 'Contexts of scientific discourse: Social accounting in experimental papers', in K. Knorr-Cetina, R. Krohn and R. Whitley (eds.) *The Social Process of Scientific Investigation*, Dordrecht: Reidel; S. Yearley (1981) 'Textual persuasion: The role of social accounting in the construction of scientific arguments', *Philosophy of the Social Sciences*, *11*, 409-435.

22 For instance, the great majority of articles in one journal which I examined were experimental studies: J. Potter (1981) The development of social psychology: Consensus, theory and methodology in the British Journal of Social and Clinical Psychology, *British Journal of Social Psychology*, *20*, 249-258.

23 Koestler, op. cit. note 5, 24.

24 All titles are, of course, fictional.

25 This phrase was contained in a letter written a year after the event.

26 R. Barthes (1975) *S/Z*, London: Cape.

27 Koestler, op. cit. note 5, 25.

28 Details of this system are to be found in J. Schenkein (ed.)(1978) *Studies in the Organisation of Conversational Interaction*, London: Academic Press.

29 E.C. Cuff (1980) 'Some issues in studying the problem of versions in everyday situations', *Occasional Paper No. 3*, Department of Sociology, University of Manchester.

30 Schenkein, op. cit. note 28, xi.

31 D.J. O'Connor and G.F. Arnold (1959) Intonation in Colloquial English, London: Longman.

32 M.A.K. Halliday (1967) Intonation and Grammar in British English, The Hague: Mouton.

33 D. Brazil (1981) 'The place of intonation in a discourse model', in M. Coulthard and M. Montgomery (eds.) Studies in Discourse Analysis, London: Routledge.

34 Ibid., 147.

35 M. Kreckel (1981) Communicative Acts and Shared Knowledge in Natural Discourse, London: Academic Press.

36 M. Coulthard (1977) An Introduction to Discourse Analysis, London: Longman, 137.

37 The comments by Derrida on speech act theory are pertinent here. Derrida stresses that speech act theory is based on a highly idealised view of language and its function and is thus unable to deal with the creative accounting possibilities which pervade real talk and writing. J. Derrida (1977) 'Limited Inc, abc', Glyph, 2, 162-255.

CHAPTER THREE: NOTHING SO PRACTICAL

1 Throughout this chapter I draw heavily on the argument of M. Mulkay (1979) 'Knowledge and utility: Implications for the sociology of knowledge', <u>Social Studies of Science</u>, <u>9</u>, 63-80.

2 N.D. Cartwrite (1974) 'How do we apply science?', in R.S. Cohen, C.A. Hooker and A.C. Michalos (eds.) <u>Boston Studies</u>, v. <u>32</u>, Boston: Reidel.

3 D. Shapere (1971) 'The paradigm concept: A review of the "Structure of Scientific Revolutions" by Thomas S. Kuhn and "Criticism and the Growth of Knowledge" by I. Lakatos and A. Musgrave, eds., <u>Science</u>, <u>172</u>, 1571-1577.

4 T.S. Kuhn (1962) <u>The Structure of Scientific Revolutions</u>, Chicago: University of Chicago Press.

5 B. Holzner and E. Fisher (1979) 'Knowledge in use: Considerations in the sociology of knowledge application', <u>Knowledge: Creation, Diffusion, Utilisation</u>, <u>1</u>, 219-245.

6 Stark, 166, cited in Mulkay, op. cit. note 1.

7 R. Johnston (1977) 'Science and rationality', Pt. 2. SISCON, Manchester, 23-24.

8 V.I. Lenin (1976) <u>Materialism and Empirio-criticism</u>, Peking: Foreign Languages Press.

9 M. Cornford (1963) <u>Theory of Knowledge</u>, London: Lawrence and Wishart, 175 (emphasis added).

10 D. Albury and J. Schwartz (1982) <u>Partial Progress: The Politics of Science and Technology</u>, London: Pluto.

11 Ibid., 124. The tension between this claim and the findings of Project TRACES, which questions the practical utility of military spending, are never made explicit by these authors, despite the fact that this research is cited elsewhere in their book. Project TRACES will be discussed in chapter 4.

12 R. Helmreich (1975) 'Applied social psychology: The unfulfilled promise', <u>Personality and Social Psychology Bulletin</u>, <u>1</u>, 548-560.

13 A.E. Gross (1976) 'Applied social psychology - problems and prospects: Some responses to Helmreich, <u>Personality and Social Psychology Bulletin</u>, <u>2</u>, 114-115.

14 Helmreich, op. cit. note 12, 553.

15 Ibid., 554.

16 J.R. Eiser (1980) 'Prolegomena to a more applied soc-

ial psychology', in G. Gilmour and S. Duck (eds.) The Development of Social Psychology, London: Academic Press, 271-272.

17 Ibid., 290.

18 J.H. Goldstein (1980) Social Psychology, New York: Academic Press.

19 P. Stringer (1982) 'Applied social psychology', Social Psychology Section Newsletter, 8, 14-19.

20 M. Billig (1982) Ideology and Social Psychology, Oxford: Blackwell.

21 K. Lewin (1951) Field Theory in Social Science, London: Harper and Row.

22 Goldstein, op. cit. note 18, 414.

23 O. Mayr (1976) 'The science-technology relationship', Technology and Culture, 17, 663-672; Mulkay, op. cit. note 1.

24 E. Hall (1966) The Hidden Dimension, New York: Doubleday.

25 These glosses on the nature of training and the role of theory should not be taken too seriously - they are to introduce the reader to the principal rather than define the state of the field.

26 G.N. Gilbert and M. Mulkay (1980) 'Contexts of scientific discourse: Social accounting in experimental papers', in K.D. Knorr-Cetina, R. Krohn and R. Whitley (eds.) The Social Process of Scientific Investigation, Dordrecht: Reidel.

27 Further discussion of these complex issues will appear in later chapters.

28 With more traditional methodological approaches this would perhaps constitute an interview bias. However, as in this case there is no attempt to recover the literal version of 'what went on' this problem is dissipated. Indeed, the construction of the question in this way can be justified by the extremely illuminating response it elicited. See J. Potter and M. Mulkay (1983) 'Scientists' Interview Talk', in M. Brenner, J. Brown and D. Canter (eds.) The Research Interview (Uses and Abuses), London: Academic Press.

29 B. Barnes and D. Edge (eds.)(1982) Science in Context, Milton Keynes: Open University Press, 148.

30 See for instance, H.M. Collins (1974) 'The TEA set: Tacit knowledge and scientific networks', Science Studies, 4, 165-185; M. Polanyi (1958) Personal Knowledge, London: Routledge; J. Ravetz (1971) Scientific

Knowledge and its Social Problems, Harmonsworth: Penguin.

31 Kuhn, op. cit. note 4.

32 N.R. Hanson (1965) *Patterns of Discovery*, Cambridge: Cambridge University Press.

33 Ravetz, op. cit. note 30.

34 Ibid., 202.

35 For instance H.M. Collins and T. Pinch (1982) *Frames of Meaning: The social construction of extraordinary science*, London: Routledge; G.N. Gilbert (1976) 'The transformation of research findings into scientific knowledge', *Social Studies of Science*, *8*, 281-306; B. Latour and S. Woolgar (1979) *Laboratory Life: The Social Construction of Scientific Facts*, London: Sage.

36 Mulkay, op. cit. note 1, 71.

37 D.E. Linder et al. (1976) 'Decision freedom as a determinant of the role of incentive magnitude in attitude change', *Journal of Personality and Social Psychology*, *6*, 245-254.

38 See pages 25-36 above, and also S. Woolgar (1981) 'Interests and explanation in the social study of science', *Social Studies of Science*, *11*, 365-394; S. Yearley (1982)'The relationship between epistemological and sociological cognitive interests', *Studies in the History and Philsophy of Science*, 13, 353-388.

1 M. Bunge (1967) 'Technology as applied science', Technology and Culture, 8, 329-347. This work is discussed in M. Mulkay (1979) 'Knowledge and utility: Implications for the sociology of knowledge', Social Studies of Science, 9, 63-80.

2 Bunge, ibid., 336.

3 R.E. Rice and E.M. Rogers (1980) 'Reinvention in the innovation process', Knowledge: Creation, Diffusion, Utilisation, 1, 499-515.

4 Ibid., 503.

5 D.J. Wood (1980) 'Models of childhood', in T. Chapman and D. Jones (eds.) Models of Man, Leicester: British Psychological Society.

6 D. Cardwell (1971) From Watt to Clausius: The rise of thermodynamics in the early industrial age, London: Heineman, 155-156.

7 L. Bryant (1966) 'The Silent Otto', Technology and Culture, 7, 184-200.

8 Bunge, op. cit. note 1, 334.

9 E. Layton (1977) 'Conditions for technological development', in K. Spiegel-Rosing and D.J. de Solla Price (eds.) Science, Technology and Society, London: Sage.

10 D.J. de Solla Price (1965) 'Is technology historically independent of science?', Technology and Culture, 6, 553-567 and (1969) 'The structures of publication in science and technology', in W. Gruber and G. Marquis (eds.) Factors in the Transfer of Technology, Cambridge, Mass.: MIT Press.

11 C.W. Sherwin and R.S. Isenson (1967) 'Project Hindsight', Science, 156, 1571-1577.

12 ITT Research Institute (1968) 'TRACES, technology in retrospect and critical events in science', NSF Report C-235. See also Battelle Memorial Institute (1973) 'Interactions of science and technology in the innovation process: Some case studies', Final Report to the NSF, NSF-C667.

13 B. Barnes and D. Edge (1982) Science in Context, Milton Keynes: Open University Press, 148. A more recent study has claimed to show that science makes a more significant contribution to technological innovation: R. Johnston and M. Gibbons (1975) 'Characteristics of information usage in technological innovation' IEEE Transactions on Engineering Management EM, 2, 1.

However, this may be partly accounted for by the very broad definition of science used by these authors: C. Ganz (1980) 'Linkages between knowledge diffusion and utilisation', Knowledge: Creation, Diffusion, Utilisation, 1, 591-613.

14 S. Blume and R. Sinclair (1973) 'Chemists in British universities', American Sociological Review, 38, 126-138.

15 Ibid., 131.

16 Mulkay, op. cit. note 1.

17 Toulmin and Goodfield, cited in ibid..

18 Ibid., 72.

19 B. Barnes (1982) 'The science-technology relationship: A model and a query', Social Studies of Science, 12, 166-172. See also Barnes and Edge, op. cit. note 13.

20 Barnes, ibid., 167.

21 Barnes and Edge, op. cit. note 13, 150.

22 The distinction between applied and applicable is discussed with respect to social psychology in J. Potter (1982) 'Nothing so practical as a good theory: The problematic application of social psychology', in P. Stringer (ed.) Confronting Social Issues: Applications of Social Psychology, London: Academic Press.

23 The names of all participants are fictitious.

24 In this account we can see the speaker deploying the sorts of contrast structures discussed by D. Smith (1978) 'K is mentally ill: The anatomy of a factual account', Sociology, 12, 23-55.

25 These are just a few of the descriptive terms taken from the conference transcript.

26 G.N. Gilbert and M. Mulkay (1980) 'Contexts of scientific discourse: Social Accounting in experimental papers', in K.D. Knorr, R. Krohn and R. Whitley (eds.) The Social Process of Scientific Investigation, Dordrecht: Reidel.

27 M. Mulkay (1976) 'Norms and ideology in science', Social Science Information, 15, 637-656.

28 See for example: M. Argyle (1980) 'The development of applied social psychology', in R. Gilmour and S. Duck (eds.) The Development of Social Psychology, London: Academic Press, and L.G. Tornatzky et.al. (1982) 'Contributions of social science to innovation and productivity', American Psychologist, 37, 737-747.

29 G.N. Gilbert and M. Mulkay (1982) 'Warranting scientific belief', Social Studies of Science, 12, 382-408; M. Mulkay and G.N. Gilbert (forthcoming) 'Scientists'

theory talk', Canadian Journal of Sociology.

30 Mulkay, op. cit. note 27.

31 Potter, op. cit. note 22.

32 See also M. Mulkay (1981) 'Action, belief or scientific discourse?', Philosophy of the Social Sciences, 11, 163-171; M. Mulkay and G.N. Gilbert (1982) 'What is the ultimate question? Some remarks in defence of the anal-ysis of scientists' discourse', Social Studies of Sci-ence, 12, 309-320; M. Mulkay, J. Potter and S. Yearley (1983) 'Why an analysis of scientific discourse is needed', in K. Knorr-Cetina and M. Mulkay (eds.) Science Observed: Contemporary Analytic Perspectives, London: Sage; S. Woolgar (1976) 'Writing an intellect-ual history of scientific development: The use of dis-covery accounts, Social Studies of Science, 6, 395-422.

33 G.N. Gilbert and M. Mulkay (1983) Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse, Cambridge: Cambridge University Press; M.A.K. Halliday (1978) Language as Social Semiotic: The Social Inter-pretation of Language and Meaning, London: Arnold.

CHAPTER FIVE: TESTABILITY, FLEXIBILITY

1 T.S. Kuhn (1977) 'Objectivity, value judgment and theory choice', in The Essential Tension, Chicago: University of Chicago Press. Although the paper was only published in 1977 it was presented in 1973.

2 I. Scheffler (1967) Science and Subjectivity, New York: Bobbs-Merrill.

3 I. Lakatos (1970) 'Falsification and the methodology of scientific research programmes', in I. Lakatos and A. Musgrave (eds.) Criticism and the Growth of Knowledge,

4 T.S. Kuhn (1962) The Structure of Scientific Revolutions, Chicago: University of Chicago Press.

5 Kuhn, op. cit. note 1, 322, author's emphasis.

6 Ibid., 330.

7 The similarity of Kuhn's suggestions and the Mertonian idea of norms in science has not gone unnoticed. See M. Mulkay (1980) 'Kuhn and the sociology of science', History of Science, 18, 298-301; T. Pinch (1979) 'Parigm lost? A review symposium', ISIS, 70, 437-440.

8 Kuhn, op. cit. note 1, 330.

9 Ibid., 331.

10 Ibid., 331.

11 Ibid., 331.

12 Ibid., 336.

13 Ibid., 335.

14 K.R. Popper (1963) Conjectures and Refutations, London: Routledge and Kegan Paul.

15 W.V.O. Quine and J.S. Ullian (1970) The Web of Belief, New York: Random House.

16 D. Crane (1980) 'An exploratory study of Kuhnian paradigms in high energy physics', Social Studies of Science, 10, 23-54.

17 A more detailed discussion of methodological issues is found in chapters 1, 2 and 8.

18 As before, all names have been changed to ensure the anonymity of the participants.

19 Much of this discussion has centred on the problem of holism and the Quine-Duhem thesis - see chapter 1, p. 26.

20 See G.N. Gilbert and M. Mulkay (1980) 'Contexts of scientific discourse: Social accounting in experimental papers', in K.D. Knorr, R. Krohn and R. Whitley (eds.) The Social Process of Scientific Investigation, Dordrecht: Reidel; S. Yearley (1982) 'Context of evaluation: A sociological analysis of scientific argumentation with reference to the history of earth science, unpublished DPhil thesis, University of York.

21 See M. Mulkay and G.N. Gilbert (1981) 'Putting philosophy to work: Karl Popper's influence on scientific practice', Philosophy of the Social Sciences, 11, 389-409.

22 Kuhn, op. cit. note 1, 339.

23 Kuhn, op. cit. note 4.

24 In the case of Norton this was a very real possibility; despite not being a speaker he was one of the most frequent contributors to the discussion. It is perhaps significant that the other person Young singled out to criticise was also absent from the hall at the time. Some of the unusual interpretative possibilities made available when key participants are absent from the scene of accounting are discussed in D. Smith (1978) 'K is mentally ill: The anatomy of a factual account', Sociology, 12, 23-55.

25 A further analytically interesting feature of this passage isthe laughter that the speaker elicits at three places in this speech. Each batch of laughter is associated with the speaker ironically recharacterising actions in contingent terms. A more detailed examination of the way humour can be used to provide insights into the organisation of scientific discourse is provided by M. Mulkay and G.N. Gilbert (1982) 'Joking apart: Some recommendations concerning the analysis of scientific culture', Social Studies of Science, 12, 585-615.

26 The notion of 'coherence' used by these speakers is very similar to Kuhn's notion of 'consistency'. See Kuhn, op. cit. note 1, 321-322.

27 Ibid., 336.

28 The inappropriateness of such assumptions for the social study of science is emphasised by H. Collins (1981) 'What is TRASP?: The radical programme as a methodological imperative', Philosophy of the Social Sciences, 11, 215-225.

29 The difficulty of deciding what should count as progress in any particular field is emphasised by a number of recent philosophers, for example P. Feyerabend (1975) Against Method, London: New Left Books. It was this problem, of course, which stimulated Kuhn's article in the first place.

30 Kuhn, op. cit. note 1, 325.

31 Ibid., 324.

32 See for example M. Mulkay (1980) 'Interpretation and the use of rules: The case of norms in science', in T.F. Gieryn (ed.) Science and Social Structure, Transactions of the New York Academy of Sciences, Series II, 39, New York, 111-125; and S. Woolgar (1981) 'Interests and explanation in the social study of science', Social Studies of Science, 11, 365-394.

33 See J. Heritage (1978) 'Aspects of the flexibilities of natural language use', Sociology, 12, 79-105.

34 In this case the relevant temporal period is that in which theory choice can be said to be accomplished.

35 See M.A.K. Halliday (1978) Language as Social Semiotic, London: Arnold.

36 M. Mulkay and G.N. Gilbert (1982) 'Accounting for error; How scientists construct their social world when they account for correct and incorrect belief', Sociology, 16, 165-183.

37 See J. Potter and M. Mulkay (1983) 'Scientists' interview talk: Interviews as a technique for revealing participants' interpretative practices', in M. Brenner, J. Brown and D. Canter (eds.) The Research Interview (Uses and Abuses), London: Academic Press.

38 I do not wish to suggest that there is anything improper about this sort of talk. It is the very ordinariness of these accounts which makes them analytically interesting.

39 M. Mulkay and G.N. Gilbert (1981) 'Opening Pandora's Box', mimeo, Universities of York and Surrey. Other sociologists have approached this issue by trying to provide a definitive account of the way the criterion of testability operates. For the most part the account of testability produced mirrors the 3rd person accounts produced by the psychologists discussed in this chapter. See H.M. Collins and T.J. Pinch (1982) Frames of Meaning: The Social Construction of Extraordinary Science, London: Routledge and Kegan Paul; G.D.L. Travis (1980) 'Creating contradictions: Or why let things be difficult when with just a little more effort you can make them seem impossible?', presented a the Annual Meeting of the Society for the Social Study of Science, Toronto, October 17-19; and particularly T.J. Pinch (1982) 'Theory testing in science - the case of solar neutrinos: Do crucial experiments test theories or theorists?', presented at the Combined Meeting of the HSS, PSA, SHOT & 4S, Philadelphia, October 28-31.

40 It would, of course, be interesting to compare these spoken accounts with more carefully planned written versions of the role of testability in theory choice, perhaps through examining debates in journal articles. However, although there might well be some difference between the two contexts, there is no reason to suppose

that written accounts will be more 'reliable' or 'literal'. See for example: Gilbert and Mulkay, op. cit. note 20; J. O'Neill (ed.) _Science Texts_, London: Routledge and Kegan Paul, forthcoming; S. Woolgar (1980) 'Discovery: Logic and sequence in a scientific text', in Knorr et al., op. cit. note 20; S. Yearley (1981) 'Textual persuasion: The role of social accounting in the construction of scientific arguments', _Philosophy of the Social Sciences_', _11_, 409-436 and (forthcoming) 'Analysing science and analysing scientific discourse: On the argumentative strategies of scientists in the public sector', _Zeitschrift fur Wissenschaftsforsschung_.

CHAPTER SIX: SCIENTISTS' SOCIAL CATEGORISATIONS

1 M. Mulkay, J. Potter and S. Yearley (1983) 'Why an analysis of scientific discourse is needed', in K. Knorr-Cetina and M. Mulkay (eds.) Science Observed: Contemporary Analytic Perspectives, London: Sage.

2 See chapter 2, 75-76.

3 S. Woolgar (1976) 'The identification and definition of scientific specialties', in G. Lemaine, R. Macleod, M. Mulkay and P. Weingart (eds.) Perspectives on the Emergence of Scientific Disciplines, The Hague: Mouton, 233.

4 The meanings of these terms are described in the postscript to the 1970 edition of The Structure of Scientific Revolutions, Chicago: University of Chicago Press.

5 Ibid., 182.

6 It is certainly possible to find statements in Kuhn's work which imply a good deal of agreement. For instance:

> 'the practitioners of a scientific specialty... have undergone similar educations and professional initiations; in the process they have absorbed the same technical literature and drawn many of the same lessons from it. ...the members of a scientific community see themselves and are seen by others as the men uniquely reponsible for the pursuit of a set of shared goals, including the training of their successors. Within such groups communication is relatively full and professional judgment relatively unanimous' (ibid., 177)

Other statements are more cautious. For instance, the characterisation of the various problems and techniques which make up paradigms as having a family resemblance implies that they may have no single feature in common. C.f. L. Wittgenstein (1953) Philosophical Investigations, Oxford: Blackwell, paragraphs 65-67.

Commentators, perhaps because of their emphasis on the problems raised by the notion of incommensurability, have often tended to emphasise the consensual, self-contained properties of Kuhn's notion of paradigms: e.g. A. Giddens (1976) New Rules of Sociological Method, London: Hutchinson; D. Papineau (1978) For Science in the Social Sciences, London: Macmillan; S. Toulmin (1972) Human Understanding, Oxford: Clarendon. From another perspective H.M. Collins and T. Pinch claim to have demonstrated incommensurability empirically and they argue that paradigms connot be adopted piecemeal: (1982) Frames of Meaning: The Social Construction of Extraordinary Science, London: Routledge and Kegan Paul; (1982) 'Rationality and paradigm allegiance in extraordinary science', in H.P. Duerr (ed.) The Scientists and the Irrational, Dordrecht: Reidel.

7 J. Gaston (1972) 'Communication and the reward system in science: A study of a national invisible college', Sociological Review Monograph, 18, 25-41; H.G. Small (1977) 'A co-citation model of a scientific specialty: A longitudinal study of collagen research', Social Studies of Science, 7, 139-166; D. Sullivan, D.H. White, and E.J. Barboni (1977) 'Co-citation analysis of science: An evaluation', Social Studies of Science, 7, 223-240.

8 D. Crane (1969) 'Social structure in a group of scientists', American Sociological Review, 34, 335-352; (1972) Invisible Colleges, Chicago: University of Chicago Press; D.J. de Solla-Price (1963) Little Science, Big Science, New York: Columbia University Press.

9 H.M. Collins (1974) 'The TEA set: Tacit knowledge and scientific networks', Science Studies, 4, 165-186; D. Edge (1979) 'Quantitative measures of communication in science: A critical review', History of Science, 17, 102-134; A. Musgrave (1971) 'Kuhn's second thoughts', British Journal for the Philosophy of Science, 22, 287-306.

10 G.L. Lewis (198) 'The relationship of conceptual development to consensus: An exploratory analysis of three subfields', Social Studies of Science, 10, 285-309.

11 D. Crane (1980) 'An exploratory study of Kuhnian paradigms in theoretical high energy physics', Social Studies of Science, 10, 23-55.

12 To take just a small sample of the possible examples: M. Brenner, P. Marsh and M. Brenner (eds.)(1978) The Social Contexts of Method, London: Croom Helm; A.V. Cicourel (1964) Method and Measurement in Sociology, New York: Free Press; N.K. Denzin (1970) The Research Act, Chicago: Aldine; R. Harré (1979) Social Being, Oxford: Blackwell.

13 Kuhn, op. cit. note 4, 47.

14 See particularly the chapter on consensus in G.N. Gilbert and M. Mulkay (1983) Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse, Cambridge: Cambridge University Press.

15 S. Woolgar (1977) 'Changing perspectives - A chronicle of research development in the sociology of science', in J. Farkas (ed.) Sociology of Science Conference, Budapest: Publishing House of the Hungarian Academy of Sciences.

16 Collins and Pinch, op. cit. note 6.

17 H.M. Collins (1979) 'The investigation of frames of meaning in science: Complementarity and compromise', Sociological Review, 27, 703-718.

18 For instance, see the discussion of the change from structuralist to post-structuralist approaches to lit-

erary competence: J. Culler (1982) The Pursuit of Signs: Semiotics, Literature, Deconstruction, London: Routledge and Kegan Paul; C. Norris (1982) Deconstruction: Theory and Practice, London: Methuen; D. Silverman and B. Torode (1980) The material word: Some theories of language and its limits, London: Routledge and Kegan Paul. See also J. Potter and P. Stringer (1981) 'Ambiguities of the home: An empirical critique of the ethogenic theory of social competence', presented at the BPS: Social Psychology Section conference, University of Kent, Canterbury, September 19-21.

19 D.S. Palermo (1971) 'Is a scientific revolution taking place in psychology', Science Studies, 1, 135-155; W. B. Weimer and D.S. Palermo (1973) 'Paradigms and normal science in psychology', Science Studies, 3, 211-244.

20 B.D. Mackenzie (1972) 'Behaviourism and positivism', Journal of the History of Behavioural Science, 8, 222-231.

21 N. Warren (1971) 'Is a scientific revolution taking place in psychology? - Doubts and reservations', Science Studies, 1, 407-413.

22 L.B. Briskman (1972) 'Is a kuhnian analysis applicable to psychology?', Science Studies, 2, 87-97.

23 W.B. Weimer (1974) 'The history of psychology and its retrieval from historiography: I the problematic nature of history', Science Studies, 4, 235-258. See also M.W. Lipsey (1974) 'Psychology: Preparadigmatic, postparadigmatic, or misparadigmatic', Science Studies, 4, 406-410.

24 E.G. Boring (1950) A History of Experimental Psychology, New York: Appleton - Century - Crofts.

25 Weimer nowhere addressed the question of whether this argument would also apply in the natural sciences.

26 G.L. Peterson (1981) 'Historical self-understanding in the social sciences: The use of Thomas Kuhn in psychology', Journal for the Theory of Social Behaviour, 11, 1-31.

27 Ibid., 9.

28 Ibid., 10.

29 J.M. Ziman (1967) Public Knowledge: The social dimension of science, Cambridge: Cambridge University Press.

30 Ibid., 132, emphasis added.

31 This phenomenon of less hedges being used when accounts are not for formal publication is also noted in chapter four. Unfortunately as the actual presentations were not transcribed at the Theoretical Perspectives Conference it is not possible to assess whether the spoken version was stronger still.

32 It is perhaps worth comparing this kind of construction with the contrast structures discussed by Dorothy Smith. The account leaves no room for ordinary oversight: D. Smith (1978) 'K is mentally ill: The anatomy of a factual account', Sociology, 12, 23-55.

33 Such a melding together of social and conceptual divisions is a recurrent feature of these accounts. It raises the question of whether these divisions are purely made by analysts, who have traditionally taken such distinctions as crucial, or whether they have consequences for the members themselves on certain occasions. There is not the space to properly address this question here.

34 The use of the highly vague notion of 'modern philosophers' also appeared in chapter 5 (page 152). It is possible to speculate that it is a warranting procedure drawing on the assumption that modern belief is more likely to be true than out-dated and experts are more likely to be correct than non-experts. Thus disagreements can be accounted for because they come from philosophers who are out of date or from scientists who are not experts.

35 C.f. chapter 5, 158.

36 M.A.K. Halliday (1978) Language as Social Semiotic, London: Arnold; R. Fowler, B. Hodge, G. Kress and T. Trew (eds.)(1979) Language and Control, London: Routledge and Kegan Paul; G. Kress and R. Hodge (1979) Language and Ideology, London: Routledge and Kegan Paul.

37 T. Trew (1979) 'Theory and ideology at work', in Fowler et al., ibid.. See also 'What the papers say: Linguistic variation and ideological difference', ibid..

38 Trew, 'Theory and ideology at work', ibid., 97-108.

39 With his response Carlisle poses Norton an interpretative problem. For Norton cannot very well criticise his own example. He could perhaps have questioned whether Carlisle's research was really like the example discussed - but that would have been difficult without detailed knowledge of Carlisle's work.

40 See also extract 2 in chapter 5, 149.

41 Trew, op. cit. note 38, 136.

42 Smith, op. cit. note 32, discusses the way the objectivity and disinterestedness of participants can be textually constructed.

43 In this chapter I have just examined variability insofar as it presents difficulties for traditional forms of analysis. An important question for future study is whether participants' identify such inconsistencies and what their responses are if they do.

44 B. Wynne (1979) 'Physics and psychics: Science, symbolic action and social control in late Victorian England', in B. Barnes and S. Shapin (eds.) Natural Order: Historical Studies of Scientific Culture, London: Sage.

45 J. Harwood (1979) 'Heredity, environment, and the legitimation of social policy', in Barnes et al., ibid.. See also (1976) 'The race-intelligence controversy: A sociological approach I - professional factors', Social Studies of Science, 6, 369-394.

46 Although their analytic goals are rather different to Harwood's, Collins and Pinch's work on parapsychologists' uses social categories in a similar way. H.M. Collins and T. Pinch (1979) 'The construction of the paranormal: Nothing unscientific is happening', in R. Wallis (ed.) On the Margins of Science, Keele: Sociological Review Monograph, 27. Problems which arise in their use of the categories 'parapsychologist' and 'orthodox scientist' are discussed in Mulkay et al., op. cit. note 1.

47 Harwood, op. cit. note 43, 236.

48 Ibid., 237.

49 See Mulkay et al., op. cit. note 1.

50 This is perhaps similar to the distinction between 'orthodox scientist' and 'parapsychologist' - each seems to make implicit evaluative assumptions. See Mulkay et al., ibid..

51 But see op. cit. note 33.

1 C. Bazerman (1981) 'What written knowledge does: Three examples of academic discourse', Social Studies of Science, 11, 361-387; J. Gusfield (1976) 'The literary rhetoric of science: Comedy and pathos in drinking driver research', American Sociological Review, 41, 16-34; M. Mulkay and G.N. Gilbert (1982) 'Joking apart: Some recommendations concerning the analysis of scientific culture', Social Studies of Science, 12, 585-615; S. Woolgar (1980) 'Discovery: Logic and sequence in a scientific text', in K. Knorr, R. Krohn and R. Whitley (eds.) The Social Process of Scientific Investigation, Dordrecht: Reidel; S. Yearley (1981) 'Textual persuasion: The role of social accounting in the construction of scientific arguments', Philosophy of the Social Sciences, 11, 409-435.

2 G.N. Gilbert and M. Mulkay (1980) 'Contexts of scientific discourse: Social accounting in experimental papers', in Knorr et al, op. cit. note 1; B. Latour and S. Woolgar (1979) Laboratory Life: The Social Construction of Scientific Facts, London: Sage.

3 R. Barthes (1977) Image - Music - Text, London: Fontana, 161.

4 J. Culler (1981) The Pursuit of Signs: Semiotics, Literature, Deconstruction, London: Routledge and Kegan Paul.

5 Barthes is not explicit, but is probably referring here to the work of Goldmann and Lukacs. See C. Slaughter (1980) Marxism, Ideology and Literature, London: Macmillan, for a discussion of these theorists.

6 R. Barthes (1972) Mythologies, London: Jonathan Cape, 116.

7 R. Barthes (1975) S/Z, London: Jonathan Cape.

8 Ibid., 9.

9 I have rather simplified Barthes's general deconstructive argument here. In particular, the implication that given the same background knowledge readers will arrive at the correct or intended reading of Balzac's text should be resisted. Some limitations of the deconstructive force of S/Z are discussed by B. Johnson (1981) 'The critical difference: Balzac's 'Sarrasine' and Barthes's 'S/Z'', in R. Young (ed.) Untying the Text: A Post-Structuralist Reader, London: Routledge and Kegan Paul. A more sociolinguistically orientated examination of S/Z is provided in R. Fowler (1981) Literature as Social Discourse, London: Batsford Academic, chapter 6. R. Coward and J. Ellis (1977) Language and Materialism, London: Routledge and Kegan Paul, provide a good general introduction to S/Z.

10 It is clear that Barthes wants to do more than this in some of his work (e.g. The Pleasure of the Text, and A Lover's Discourse). Yet it the reconceptualisation of reading to make it a topic for analysis in its own right which is most of interest in the present discussion. For a detailed examination of the relationship between developments in literary theory and social science see: J. Potter, P. Stringer and M. Wetherell (1983) Social Texts and Context, London: Routledge and Kegan Paul.

11 Apart from S/Z, one of the classic discussions of realism as a textual practice rather than a mode of description concerns realism in film: C. MacCabe (1974) 'Realism and the cinema: Notes on some Brechtian theses', Screen, 15, 7-27.

12 Gilbert and Mulkay have adopted Halliday's term 'register' to refer to interpretative repertoires. The advantage of this term is that it emphasises the relationship between the form and content of language and its context and function. M.A.K. Halliday (1978) Language as Social Semiotic, London: Arnold. Halliday, however, occasionally treats this term as having causal implications - specifically that register is determined by context - these do not mesh with the present perspective. The similarity of the notion of register and Barthes's idea of a code has been discussed in Fowler, op. cit. note 9, chapter 10.

13 Gilbert and Mulkay, op. cit. note 2.

14 Other commentators on science have noted that there are unusual stylistic features to scientific texts. Gilbert and Mulkay have emphasised the systematic nature of such differences as part of the broader organisation of scientific discourse. G.N. Gilbert and M. Mulkay (1983) Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse, Cambridge: Cambridge University Press.

15 This type of discourse was quite common in all the conference transcripts. A similar example occurrs in chapter 4, extract 4. It is a phenomenon worthy of analysis in its own right. Goffman's term 'footing' might be useful here: E. Goffman (1981) Forms of Talk, Oxford: Blackwell.

16 D. Smith (1978) 'K is mentally ill: The anatomy of a factual account', Sociology, 12, 23-55.

17 The notion of an 'implied reader' is discussed in detial in W. Iser (1978) The Act of Reading, London: Routledge and Kegan Paul. Iser's position is, however, rather different to that of this thesis.

18 For a more grandiose approach to this distinction see: D. Silverman and B. Torode (1980) The Material Word: Some theories of language and its limits, London: Routledge and Kegan Paul.

19 For an interesting discussion of the way the problem of competing versions is managed see: E.C. Cuff (1980) 'Some issues in studying the problem of versions in everyday situations', Occasional Paper No. 3, Department of Sociology, University of Manchester.

20 See Fowler, op. cit. note 9.

21 S. Yearley (1982) 'Contexts of evaluation: A sociological analysis of scientific argumentation with reference to the history of earth science', unpublished DPhil. thesis, University of York.

22 Woolgar, op. cit. note 1.

23 Coward and Ellis, op. cit. note 9, 52.

24 The term 'intertextuality' was introduced by Julia Kristeva (1980) Desire in Language: A Semiotic Approach to Literature and Art, Oxford: Blackwell. See Culler, op. cit. note 4 for a lucid, although limited, discussion of this notion.

25 Culler ibid., chapters 1 and 3; C. Belsey (1980) Critical Practice, London: Methuen; R. Crosman (1980) 'Do readers make meaning?', in S.R. Suleiman and R. Crosman (eds.) The Reader in the Text, Princeton: Princeton University Press, discuss this issue of the plurality of critical interpretations. By no means all literary theorists have been willing to renouce the goal of producing definitive or correct interpretations. See for example: W. Booth (1977) 'In defence of authors', Novel, 11; E.D. Hirsch (1976) The Aims of Interpretation, Chicago: University of Chicago Press. Interestingly, Booth draws an explicit analogy between the idea that there can be no definitive interpretation of a text and the idea that paradigms in science are incommensurable. Citing Popper for support, he claims that both ideas are forms of dogmatic relativism which suffer from the 'myth of framework'.

26 Yearley, op. cit. note 21, chapters 7 and 8.

27 Ibid.; see also A. McHoul (1980) 'The practical methodology of reading in science and everyday life: Reading Althusser reading Marx', Philosophy of the Social Sciences, 10, 129-150.

1 G.N. Gilbert and M. Mulkay (1980) 'contexts of discourse: Social accounting in experimental papers', in K. Knorr, R. Krohn and R. Whitley (eds.) The Social Process of Scientific Investigation, Dordrecht: Reidel; Ibid. (1982) 'Warranting scientific belief', Social Studies of Science, 12, 383-409; M. Mulkay and G.N. Gilbert (1982) 'Accounting for error: How scientists construct their social world when they account for correct and incorrect belief', Sociology, 16, 165-183; Ibid. (1982) 'Joking apart: Some recommendations concerning the analysis of scientific culture', Social Studies of Science, 12, 585-615.

2 'Accounting for error', ibid..

3 T.F. Gieryn (1982) 'Not-last words: Worn-out dichotomies in the sociology of science (reply)' Social Studies of Science, 12, 329-337.

4 Ibid., 322.

5 Gilbert and Mulkay, op cit. note 1, chapter 2 (in draft chapter).

6 M.A.K. Halliday (1978) Language as Social Semiotic, London: Arnold.

7 Halliday notes that the primary difference between registers is semantic with lexical and grammatical differences being generally the 'automatic' consequence of semantic differences. Ibid., 185.

8 M.A.K. Halliday, A. McIntosh and P. Strevens (1964) The Linguistic Sciences and Language Teaching, London: Longman.

9 M. Coulthard (1977) An Introduction to Discourse Analysis, London: Longman, 35-36.

10 C.f. R.D. Huddleston et al. (1968) Sentence and clause in scientific English, O.S.T.I., Report 5030.

11 M. Mulkay and G.N. Gilbert (1983) Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse, Cambridge: Cambridge University Press, chapter 5.

12 E.g. ibid.; S. Yearley (1982) 'Contexts of evaluation: A sociological analysis of scientific argumentation with reference to the history of earth science', unpublished D.Phil thesis, University of York.

13 J. Heritage (1978) 'Aspects of the flexibilities of natural language use', Sociology, 12, 79-105.

14 T. Trew (1979) 'Theory and ideology at work', in

R. Fowler, B. Hodge, G. Kress and T. Trew (eds.) Language and Control, London: Routledge and Kegan Paul.

15 Yearley, op. cit. note 12.

16 D. Silverman and B. Torode (1980) The Material Word: Some Theories of Language and its Limits, London: Routledge and Kegan Paul.

17 This argument is made in some detail with specific reference to the use of social psychology in J. Potter, P. Stringer and M. Wetherell (1983) Social Texts and Contexts, London: Routledge.

18 For a preliminary treatment of psychoanalytic writings in this way see the special issue of Diacritics (9, 1979) on 'Freud's Tropology'.

19 S. Shapin (1982) History of science and its sociological reconstructions, History of Science, 20, 157-211, 200.

20 Shapin's quote is from M. Mulkay and G.N. Gilbert (1982) 'What is the ultimate question? Some remarks in defence of the analysis of scientific discourse', Social Studies of Science, 12, 309-320, 314.

21 B. Barnes (1982) T.S. Kuhn and Social Science, London: Macmillan, 97.

22 H.M. Collins (1983) 'An empirical relativist programme in the sociology of scientific knowledge', in K. Knorr-Cetina and M. Mulkay (eds.) Science Observed: Contemporary Analytic Perspectives, London: Sage, 25 (draft chapter).

23 D. Mackenzie and B. Barnes (1979) 'Scientific judgment: The biometry-mendelism controversy', in B. Barnes and S. Shapin (eds.) Natural Order, London: Sage.

24 Such an approach is explicitly advocated in, for example R. Bogdan and S.J. Taylor (1975) Introduction to Qualitative Research Methods, New York: Wiley.

25 Methodological issues which arise in the analysis of scientists' discourse are discussed in detail in J. Potter and M. Mulkay (1983) 'Scientists' interview talk', in M. Brenner, J. Brown and D. Canter (eds.) The Research Interview (Uses and Abuses), London: Academic Press.

26 Barnes, op. cit. note 21, 98.

27 This may be a further difference between discourse analysis and ethnomethodological approaches to science. C.f. chapter 1.

28 D. Bloor (1976) Knowledge and Social Imagery, London: Routledge and Kegan Paul; H.M. Collins (1981) 'What is TRASP? The radical programme as a methodological imper-

ative', _Philosophy of the Social Sciences_, _11_, 215-224.

29 This is argued in M. Mulkay (1981) 'Action, belief or scientific discourse?', _Philosophy of the Social Sciences_, _11_, 163-171.

30 This example is elaborated in J. Potter (1983) 'Philosophy of science, sociology of science, or something else?', presented to the Department of Philosophy, University of St Andrews, 25 January.

31 D. MacKenzie (1981) 'Interests, positivism and history', _Social Studies of Science_, _11_, 498-504.

32 S. Woolgar (1981) 'Interests and explanation in the social study of science', _Social Studies of Science_, _11_, 365-394.

33 MacKenzie, op. cit. note 31, 500.

34 Mulkay, op. cit. note 29.

35 S. Yearley (1982) 'Naturalism and anti-naturalism in sociology: A methodological assessment of an epistemological debate', mimeo, St. Hugh's College, Oxford; see also Yearley, op. cit. note 12, chapter 8.

36 The claim that traditional approaches attempt to construct definitive versions is not intended to suggest (as Gieryn, op. cit. note 3 seems to interpret it) that these approaches produce descriptions and models that are beyond revision in some way. Rather it is the claim that they see actions and events as having a single recoverable meaning which it is their goal to identify, even if each particular act of identification is only provisional.

37 The ethnomethodological work on science is probably an exception here - see chapter 1.

38 Collins, op. cit. note 22, Appendix 1 and footnote 25.

39 See M. Mulkay, J. Potter and S. Yearley (1983) 'Why an analysis of scientific discourse is needed', in K. Knorr et al., op. cit. note 1; Mulkay and Gilbert, 'Joking apart', op. cit. note 1, footnote 24.

40 Collins, op. cit. note 38; H.M. Collins and T.J. Pinch (1982) _Frames of Meaning_, London: Routledge and Kegan Paul.

41 Gilbert and Mulkay, op. cit. note 11, chapter 1.

42 It is best to read Derrida's paper in conjunction with Searle's reply and then Derrida's extended criticism of the reply: J. Derrida (1977) 'Signature Event Context', _Glyph_, _1_, 172-197; J.R. Searle (1977) 'Reiterating the differences', _Glyph_, _1_, 198-208; J. Derrida (1977) 'Limited Inc. abc', _Glyph_, _2_, 162-258.

43 For an attempt to apply speech act theory to written

texts see: W. Iser (1978) The Act of Reading, London: Routledge and Kegan Paul.

44 Gieryn makes the rather different point that discourse analysts are suggesting that it is inappropriate to study anything except accounts and formal papers (op. cit. note 3). However, this is simply a mistake (as the subject matter of this thesis makes plain). Indeed, virtually everything on Gieryn's list of exclusions could potentially be addressed as discursive data, even citations, favourite of the most traditional of normative sociologists, could be examined as a form of textual practice: see G.N. Gilbert (1977) 'Referencing as persuasion', Social Studies of Science, 7, 113-122.

45 For instance, it seems unlikely that the study of discovery' will be approached in quite the same way following A. Brannigan (1981) The Social Basis of Scientific Discoveries, Cambridge: Cambridge University Press.