

AN ETHNOGRAPHIC EXAMINATION OF ACCOUNTANCY

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by

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ABSTRACT

This Thesis constitutes the findings of an ethnography of accountants. Specifically, it will describe and analyse the relationship between the presentation of self and the business of auditing; examine how this relationship is reflexively accomplished; and will explicate the mundane reasoning underscoring the production of weekly accounts. As a background these empirical discussions, the thesis will consider the relevance of the later philosophy of L. Wittgenstein. It will be argued that his philosophic method can be used to delineate sociology that makes sense from that which does not. It will be argued that sociology that does not make sense suffers from conceptual confusion. These confusions will be seen to occur in the sociological program known as ethnomethodology and in the sociology of accountancy. Many of these confusions derive from attempts to integrate and synthesise different sociological methods and approaches. To avoid these kinds of conceptual confusions, sociological pluralism is advocated. Thus, these theoretical discussions justify and explain the use of diverse methods and approaches in the empirical chapters.

DECLARATION

No portion of the work referred to in this thesis has been submitted in support of an application for another degree or qualification at this or any other university or other institute of learning.

CANDIDATE'S EDUCATION AND RESEARCH EXPERIENCE

The author commenced graduate studies in Manchester in 1983; was a recipient of a Regent's Fellowship to the University of California, Santa Barbara, 1984-5; returned to Manchester in 1985; and took a full time research post at the Sociology Department, University of Lancaster, in 1987. The author is presently employed as a Senior Research Fellow on a project in to the Social Organisation of Information Technology and Police Work at the same Institution.

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I have many friends to thank. They have forgiven my short tempers, my pomposity, in short, the ridiculous self obsession that over comes one when writing a Ph.D. I want to say thanks to Philip, Graham, Andrew; Anna, & Paula. I also have to mention Shellie because she asked me to.

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Dedicated to Wes, John and Rod

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CHAPTER ONE

INTRODUCTION

It would not be unreasonable to suggest that one rule-of-thumb in science is that the scientist should not simply describe, experiment and test, but should, first of all, devise a question, and use it as a guide, delineating the topic and the methodology. *Ethnography*, in contrast, is often embarked upon without any clear idea what question will be asked. The ethnographer has little to guide him, is unsure of his topic, and has virtually no idea of method. Of course, hardly anyone would claim that ethnography is a science, and so it is unsurprising therefore that ethnographers do not abide by this particular rule-of-thumb. But this difference does serve to underline some important issues. In particular the ambiguity of the ethnographers task: what precisely is he supposed to be about? Is ethnography simply the description of exotica?; a travelogue if you will, of an academic? There are, undoubtedly, many possible answers to this question, but I do not propose to adjudicate between them here. My purpose in these opening pages is to describe the intellectual path that led to the thesis taking its present form. I want to explain how it was that I have given almost half of it over to the problem of conceptual confusion in sociology. I want to show how this is a consequence of my original efforts to sort out the ambiguities and reportage of my ethnographic research.

Ethnographic Research Methods

I began my ethnographic research without any strong sense of what I would discover. What I found interesting was only recognised during the research process and was not expected beforehand. To be sure, there are many limitations to such a research attitude. For one thing it is likely that a researcher will not be able to brief himself properly beforehand; another is that precious research time could be wasted inquiring into matters that turn out to be uninteresting. But one possible *outcome* of this approach to ethnographic research is that the areas found to be interesting may seem unrelated. Although, for example, accountants may be the general topic, the particular aspects of their behaviour seen to be interesting and ultimately chosen for analysis, may be, in a variety of ways, distinct. And indeed, this is what happened in my case. For I found interesting the following matters. First, how accounting reasoning is built upon 'common sense knowledge'. This knowledge maybe about such things as the typical, routine features of an organisation's accounts. Lack of knowledge about these matters may make even the most simple accounting task problematic and can bring into doubt the professional competence of the individual concerned. This is the topic of chapter five. A second matter that struck me as interesting is how individuals learn to adopt behavioural strategies that accord with the 'professional manner'. This is considered important by accountants themselves for a number

of reasons. One is that the manner impresses potential clients. As a consequence, these individuals may be more likely to bring their business to the firm in the future. In short, adopting the professional manner is good for business. This is the concern of chapters six and seven.

Now I do not want to explain why I found these topics interesting at this point. Rather, I want to draw attention to what kind of relationship topics such as these are likely to have to one another. For it was in an attempt to answer this question that led me to construct the thesis as I have.

One possibility that comes to mind when thinking about the relationship of these two matters is, obviously, to treat them as two sides of a single coin. Two faces as it were, of the accountants world. But to do so, it seemed to me, and now, after much consideration, I am convinced, would seriously oversimplify things. Although I do not have the space in this introduction to cover all the arguments, one way of illustrating how the matter is not at all simple is by considering how the term *professional* is used in each of the analyses. Even from the brief outline given above, it would appear that in chapter five, the word *professional* is used in conjunction with the word 'competence', and is a label for knowing certain things. In chapters six and seven it refers to patterns of personal presentation. Thus the use of the word professional is not the same in each, there

is some kind of resemblance, perhaps a *family* one, but not a direct correspondence. If then, the relationship between the same language terms in these chapters is not at all easy to define, then it seems not unreasonable to suggest that figuring out the relationship between the topics of the chapters as a whole will be difficult. To help me unravel this problem I turned, like many sociologists before me, to philosophy. In particular I turned to Wittgenstein.

The Winch Debate

As an undergraduate I had been introduced to P. Winch's *The Idea of a Social Science*, and hence to the so-called Winch debate about the importance for social science of the later work of Wittgenstein. There have been many participants in this debate, and what the implications of Wittgenstein's work are has not always been made clear. What is undisputed however, is that, according to Wittgenstein, language is sometimes used in a confused way, or more exactly, the meaning of language is sometimes 'knotted'. These knots or *conceptual confusions* show themselves in philosophical discourse. This was Wittgenstein's topic. But they also show themselves in social science. This was Winch's concern. One type of confusion occurs because some language terms mean slightly different things in different contexts. Confusion arises when the meaning appropriate for one context is mistakenly applied in the other. This is not to say that meaning is infinitely variable (the claim of some in the Winch debate), but draws attention to the idea that meaning

is related to context and use. If this is so, then it may be an unwise enterprise for example, to assume, say for analytic purposes, that a term used in more than one context means the same thing in all of them. For, to the extent that the usage and hence the meaning in each is distinct, such an analytic assumption could lead to a misunderstanding, or to a *conceptual confusion*. Ultimately this could result in language that does not make sense. So, it may be that the two uses of the word 'professional' just discussed are better left separate, for any attempt to fuse them or treat them as meaning the same thing may lead to confusion. For it would be unclear whether at one moment in time we were talking about *professional knowledge* or *professional image*. This does not mean, however, that the two uses do not have commonalities, or that there is no relationship. But it does mean that the relationship between them is *complex* and not one to be treated flippantly.

Given these arguments, then, it is clear that Wittgenstein's work will be relevant to the question of sorting out the relationship between the different topics that interested me. But my undergraduate knowledge of his philosophy was not sufficient for a doctoral analysis, and so to provide me with the wherewithal, and subsequent to the completion of my ethnographic research, I surveyed some of the literature in the Winch debate. This survey led me on to think about areas seemingly far removed from ethnography and the relationship between my ethnographically derived research

topics.

My first reaction to the survey was surprise. For I discovered that some of the criticisms of Wittgenstein's work were riven with the kinds of conceptual confusions that, it seemed to me, he had set out to unravel. Moreover, some of the other criticisms missed the point entirely. But once over this surprise I began to fully appreciate the importance of Wittgenstein's observations. If his arguments were correct, it meant that conceptual confusions were *endemic*. There was therefore, always a need for a Wittgensteinian unravelling of 'knots in language'. I thought about this. Clearly, it would be too immense a task to consider every aspect of sociology to see where there were conceptual confusions. But on the other hand, it seemed likely that within the fields of the sociology of accountancy, and in particular the in some of research approaches I employed, there was going to be some kind of conceptual confusion. I decided therefore, to review some of the literature pertaining to the sociology of accountancy and to ethnomethodology. I noted that there were indeed, many instances of conceptual confusions. So rife in fact, did they seem, that I decided that I would have to incorporate consideration of them in my thesis. For, if nothing else, it became apparent to me that my own ethnographic research was likely to misunderstood if I did not. In addition my consideration of confusions would allow me the opportunity not just to unravel ones that had already

occurred, but to make some suggestions about how to avoid some of them in the future. In particular my reading had suggested to me that one major source of confusion were attempts to mix or *integrate* sociological approaches. My use of two different approaches to examine more or less the same topic (in chapters six and seven) offered me a vehicle to illustrate the complexities entailed when attempting to mix approaches, and furthermore, would allow me to underline the benefits of keeping them separate.

This then, is the background to why the thesis takes its present form. Although it started out as an ethnography, the scope of its concerns has been extended to incorporate analysis of conceptual confusion, and thereby, what ethnographic analysis is included is hopefully, made clearer. That it takes this form however, has meant that some of the features associated with ethnography are absent, or are given less prominence. Amongst these are the discursive length and descriptive elements often incorporated in ethnography to bring the situations described 'to life'. This absence may mean that the thesis lacks the richness that often makes reading an ethnography so entertaining. But, I hope that this is an acceptable cost for the attainment of conceptual clarity. At the same time, the theoretical discussions of Wittgenstein's work and its implications are also compromised by the need to allow space for the ethnography. This has necessitated a brevity that severely constrains any attempt to do justice to the

matters at hand. The already mentioned immensity of any analysis of conceptual confusion in sociology underlines these constraints. The limitations this compromise brings then, must be borne in mind when the thesis is read. It would not be fair for example, to criticise it on the grounds that the ethnographic descriptions are not rich enough, or that the theoretical discussions do not survey enough of the pertinent literature. For it is to be judged by how well these two aspects are integrated; and how, at the same time, each serves to illuminate the general case I wish to make.

The Structure of the Thesis

The thesis is structured so that the reader is made familiar with the features of Wittgenstein's later philosophy at the outset. With that in hand, the arguments will unfold with increasing specificity until we reach the ethnographic sections. Specifically, chapter two will outline what I believe are the key elements of Wittgenstein's philosophy and their implications for social science. Although I shall make reference not only to advocates of Wittgenstein but also to his detractors, the main function of the chapter is exposition. Those negative commentaries I do refer to are used therefore, only to illustrate the kinds of errors often made when trying to understand Wittgenstein's work. One consequence of this is that the chapter will seem somewhat one-sided. But, as I have explained, the compromise that

lies at the heart of this thesis necessitates brevity. For the same reason, chapter two must not be thought of as a literature review of the Winch debate.

Chapter three introduces us to some of the conceptual confusions I detect in ethnomethodological literature. These confusions, I will suggest, derive from errors about particular language terms. One possible reason for them is, I venture, that ethnomethodologists, like many sociologists, have still not taken on board Winch's wise suggestion that they do some *conceptual analysis*. If they did, I believe they would recognise what they can and cannot say, and thus what leads to the kinds of confusions they make. The chapter will conclude with some observations about sociology as a whole, and I discuss the desire to integrate the ethnomethodological program with other sociological approaches. Such integration, I will assert, often leads to more confusion.

Chapter four will pursue the same Wittgensteinian concerns but more generally. My topics will be with theorising that is unjustified, criticisms that are beside the point, and mistaking methodological stipulations for descriptions. Specifically, and first of all, I will discuss some theories of ethnography. I commenced this introduction with the observation that ethnographers do not always know what they are looking for. Some commentators have argued that ethnographers need a theory - one that would enable them to find 'culture'. These theories are, I will argue,

unjustified. This is because the notion of culture that underlines them is misconceived since it is based on a mixing up of two ways of using the concept 'culture'. Secondly, I will look at how certain approaches are criticised for not doing what they were not intended to do in the first place. In other words they are criticised on irrelevant grounds. Next I will consider the belief that different approaches can be triangulated. I will contend that this belief is based on a failure to recognise the stipulative distinctiveness of different approaches. One reason for holding it may be the notion that sociology wants to produce a unified description. This will lead me on to my last concern : the idea that one can advocate an approach on the grounds that its methodological stipulations offer the best descriptions of society. This is a mistake I believe because these stipulations are not descriptions. This error is particularly onerous for it leads to theoretical closure. I shall look at the sociology of professions to illustrate this kind of closure. For there, one particular approach or paradigm has gained complete dominance. This has resulted in a stifling of interest in the area. I will use all these examples and discussions to further justify my suggestion that one should not be so keen to link or integrate sociological approaches. I will argue that an appropriate way of thinking about them is to treat them as *language games*.

Chapters five to seven constitute the ethnography. In my

discussions of ethnomethodology I will have pointed out that one of the primary concerns of this approach is the ways in which common-sense knowledge is deployed in skilled activity. Chapter five will describe how common-sense knowledge about the predictable nature of weekly accounts underscores the accounting procedures in which some of the weekly figures are used. Familiarity with this knowledge is a requisite of competence and as such, is a feature of professionalism.

Chapter six will take a dramaturgical view of the socialisation of accountants. I will argue that the use of this approach helps one explain this process, by defining it as one that equates to a move from back to front stage. It also involves learning to adopt front stage roles all the time, even back stage. Chapter seven will take up the same topic but will do so from an ethnomethodological position. It will look at how individuals make sense of the socialisation they go through.

As I have explained, one purpose of the thesis is to suggest that sociological pluralism is likely to lesson the frequency of conceptual confusion. By locating these two chapters side by side, and by dealing with the same topic, I hope to elucidate the differences between the two approaches. I will emphasise the considerable difficulties and confusions that would ensue if attempts are made at integrating them. I will suggest that they are best treated

as different language games. I will conclude with some remarks about what makes the selection of each approach analytically worthwhile.

Whether the thesis succeeds in everything is, of course, not up to me to assess. One obvious fault, even at this early stage, may appear to be my obsession with conceptual confusion, almost, it might seem, to the exclusion of everything else. It is certainly true, I suppose, that there is nothing like the recently converted. But I do hope that I justify, in the pages that follow, my firm belief that sociology is in need of substantial revision. Not on empirical or methodological grounds, but *conceptually*. I know that not many people will read this thesis, and that therefore it would be naive of me to think that I will actually persuade anyone to do some conceptual thinking. Moreover, the two or three people who do read it are my examiners, and since they also taught me, it is hardly likely that I will come up with anything that they have not thought of before. Be that as it may, I hope that what I do argue is not so dogmatic as to bore these 'old converts'. It is to them I have dedicated this thesis.

CHAPTER TWO

MAKING SENSE IN SOCIOLOGY

Most theses include some reference to philosophy. This will be no exception. My concern in this chapter is to explicate some of the implications for social science of the later philosophy of L. Wittgenstein. As I have already pointed out in the introduction, my purpose will not be to explain or justify how subsequent chapters can or should be immersed in Wittgenstein's philosophy but to show how his work can be used as a method to sort out certain types of difficulties or problems. In particular, Wittgenstein's philosophy can be used as a technical device or aide. Thus it will not be used, for example, as a system or paradigm that defines intentions beforehand. That philosophy will have a solely technical role will distinguish this thesis from others that prefer, or rather attempt to use, philosophy to extend the ambitions and scope of their inquiries¹. The kinds of problems that I believe can be remedied by reference to Wittgenstein's philosophy exist both at a theoretical and empirical level in sociology. I shall limit my remarks about these problems to theoretical matters in this chapter; but

¹Indeed so ambitious are some that it is not unfair to say that their work incorporates sociological and philosophical issues in such a way as to make their work deeply pretentious. They claim for example to be dealing not only with sociological matters but with the nature of existence itself. This pretentiousness has unfortunate consequences: it often means the technical difficulties of doing empirical sociology are ignored, and even the fact that sociology is intendendly empirical becomes obscured.

will take them up again and more specifically on empirical grounds in subsequent chapters.

I shall try to prove my case by looking at the arguments of three advocates and three critics of his work. These are respectively, Winch, Louch and Pitkin; Gellner, Giddens and Hekman. The selection of these six may seem somewhat arbitrary. For one thing it has to be acknowledged that there is a veritable Wittgenstein industry - no self respecting publisher will go a year without adding another volume on the philosopher. Hence to choose only six necessarily means ignoring many more, some of which are no doubt worthy of attention. For another, I do not pretend to be familiar with all of this vast corpus of literature. Nonetheless, the six are chosen because, firstly, they exemplify tendencies in the debate and secondly, because the different angles each takes will, I hope, provide me with enough materials to properly elucidate and substantiate my

case². Specifically, I have chosen to discuss Winch partly because his was the first major attempt to define what are the implications of Wittgenstein's work for social science. Indeed, his name is now associated with this question. In addition, his work also, teaches us about the kinds of difficulties that one can get into when trying to elucidate the implications of Wittgenstein's work. By looking at these difficulties I hope to further clarify the issues at hand. I consider Louch because his work was the first major attempt to reformulate the problem after Winch; he also slightly alters the importance given to *evaluation* in social science; Pitkin partly for her clarity and elegance but more importantly because she places a greater emphasis on *language games* than the previous two, an emphasis that I will argue is justified. Gellner's critical comments are examined because, though, as I will show they are incorrect, they serve to highlight important aspects of the concept of

²It is perhaps important to note at the outset that the impact of these authors has varied enormously. In particular, the work of P. Winch and E. Gellner have come to be the templates on which many subsequent studies have tended to build. This, despite Winch's work not being a clear exposition of Wittgenstein's philosophy nor of its implications for social science; and Gellner's interpretation dealing with issues that are almost totally tangential to the key ones. That Winch's work has received the status - or perhaps notoriety - it has can in part be explained by the fact that his book 'The Idea of a Social Science' (1958) was the first to deal with the subject of Wittgenstein's philosophy and its importance for social science. Why Gellner's work has attained an equally central place is far less clear. It may be that Gellner voiced views that were current in sociology at the time, and discussion of Wittgenstein became a vehicle for them - even though these views, as I will show, have little relevance.

form of life. Additionally, they are symptomatic of the kinds of errors that undercut much of philosophical inquiry, errors which Wittgenstein attempts to correct. Giddens' remarks, again critical of Wittgenstein, are discussed because they are typical of the readings social scientists make. For Giddens appears to assume that Wittgenstein is producing a *social* rather than a philosophical theory. This kind of confusion, it seems to me, is one that social scientists should be particularly wary of. I shall attempt to show why. Finally, I look at Hekman because, though rather sophisticated in some of her remarks, the central claim of her critique appears to derive from the kind of conceptual confusion Wittgenstein is at pains to point out. That Hekman fails to recognise this is, perhaps, indicative of how deeply rooted conceptual confusions are.

But beyond all this, and I want to underline this, it is not my intention in this chapter to review the so-called Winch debate; it is simply an exercise in elucidation. Discussion of the critical commentators for instance, is designed only to further this intention. Their remarks are included because they illustrate the kinds of errors made when attempting to understand the complex and subtle aspects of Wittgenstein's philosophy. One consequence of this is that this chapter can justifiably be accused of one sidedness; but, as I have explained already, the overall ambitions of the thesis necessitate this.

Notwithstanding all these limitations then, I will show

that the implications of Wittgenstein's philosophy relate largely to problems of meaning. His philosophy does not so much provide a schema, a program or model of, say, society (in the manner of Hegel), as it provides a mechanism for sorting out problems of meaning. It is in this sense that Wittgenstein's philosophy can be seen as a tool. These problems occur in sociology just as they do in philosophy. They can take many forms: conceptual confusions; puzzlements; unwarranted implications; false generalisations. Insofar as they derive from the use of language they are endemic to all forms of thinking. I shall show that the advocates of Wittgenstein's philosophy have slightly different interpretations of how such problems are to be avoided or resolved. As I have already said, the critics will be seen to have largely misunderstood Wittgenstein's purpose. They tend to ignore the emphasis on method and instead claim he is creating a model. They define this model and then criticise it. In effect, (and if I am allowed to bring in a little levity to the thesis at this early stage), they employ a straw man approach to philosophical and sociological debate but unfortunately the straw man they create has little - or perhaps only a family resemblance - to Wittgenstein himself. Even so the criticisms they make are instructive in that they show the pre-occupations of social scientists; pre-occupations that are so strong as to blind them from seeing, let alone taking into account, at least some new ideas. The criticisms are

also sometimes based on the kinds of problems of meaning that Wittgenstein had provided methods for sorting out.

Though the critics discussed will be said to have missed the mark I will not claim that Wittgenstein's work is beyond reproach or that its implications are unproblematic. But it is probably true to say that social scientists have moved away from Wittgenstein (despite the flurry of interest after Winch's book) without giving him a proper hearing. Moreover, the newly emerged sociology of accountancy does not take Wittgenstein into account at all. It is perhaps opportune therefore, to reconfront the implications of his work at this time.

The chapter will be divided into sections. I shall begin by outlining, briefly, Wittgenstein's philosophy. This will provide a basis for understanding the arguments put forward by the advocates and critics of his work. I will then discuss each of these authors individually. Finally I will review their various positions, add some remarks of my own and come to some general conclusions.

Wittgenstein's Work: Philosophy as Method

As will be well known, Wittgenstein changed his view of philosophy in mid life. His later philosophy repudiated much of what he had said and written before. It is this later

work that is of concern here ³. Wittgenstein published little or nothing on his later philosophy during his life⁴. 'The Philosophical Investigations' (1958), now regarded as the centrepiece of the later philosophy was published posthumously. Other works, most notably 'Remarks on the Foundations of Mathematics' (1956), 'On Certainty' (1969), and 'the Blue and Brown books' (1958) are based on students' notes of his lectures and seminars or were dictated to students. Even though these publications may well not have been edited or published in a way Wittgenstein would have preferred, the arguments they contain are substantial and of such a consistency that they can be used to represent a philosophy, albeit one quite different from any that had been propounded before.

Wittgenstein's essential concern in this later philosophy was to understand the nature of thought, especially in everyday life (Pears, 1971:90). Whereas in his earlier work he considered it possible to generalize about certain features of language, in his later work he did not believe it always possible, let alone desirable, to generalize in

³As I have mentioned there are a multitude of books on the philosopher. Amongst these are: Bogen, 1972; Cavell, 1966; Kenney, 1973; Kripke, 1982; Pears, 1971; Specht, 1967. Pitcher, (1966) edits an interesting collection of essays. Articles will be mentioned as the occasion arises.

⁴His earlier work, which will not be discussed here, is encapsulated in his opaque 'Tractatus Logico-Philosophicus' (1961). To properly understand Wittgenstein's thinking it is necessary to consider this earlier work. Space precludes doing so here. But for those interested they should look at M. Black, 1964.

the same way. For the results of this were typically counterproductive in philosophy. Instead he came to believe that if one wanted to understand language one had to examine how it is used in practice and in specifics. If we did this, he argued, then one would see that language did not embody a logical system, nor hierarchy of propositions enabling the truth of statements to be judged. Rather, language is best thought of as a part of the action(s) in which it is embedded. Or put another way, language can be compared to a tool, employed alongside others, in the organization of human activity. Wittgenstein went on to point out that, to the extent that the activities people engage in are diverse and the purpose of language in each correspondingly different, then the meaning of specific (language) terms may well vary in different contexts of use. If this was so, Wittgenstein suggested, then to learn about the meaning of any term one could look at the different ways it was used in ordinary practice.

In putting forward this argument, Wittgenstein was calculatedly challenging an "older and almost ubiquitous view of language which stresses reference, correspondence, representation" (Pitkin, 1972:3). He was also attacking the view that language was in need of improvement especially through the construction of a logically sound syntax. (Something he had believed possible in younger days).

Wittgenstein elaborated his ideas by suggesting that one way

of thinking about language terms was by locating them in what he calls "language games", wherein the particular language term serves a specific purpose in a certain set of circumstances. Hence the expression 'one pint please' will result in different response dependent upon whether it is delivered to a milkman or a barman. The phrase serves a different (though similar) function in two different contexts or two different language games.

Wittgenstein did not argue that there was no distinction between use and meaning. His case was that philosophical puzzles about the 'nature of meaning' could often be seen to be illusory if we look at the ways we use words of concern to philosophers - like 'meaning', 'truth', 'reality' in their ordinary, 'home' contexts. Since most language terms, phrases, etc. can be used in a variety of ways, and since language is learnt through usages, then the meaning of language terms is complex, a compound of usages. Language as a whole cannot be said to be a system in the sense of being massively consistent and unitary. It is *ad hoc* and piecemeal. It may be and indeed often is the case that usages in different games seem to contradict one another.

Wittgenstein suggested that sometimes problems occur about the meaning of language when terms get placed in the wrong language game. This is particularly the case in philosophy, where the meaning of words is often considered in isolation, removing the words in question from their ordinary context of its use. This leads to what Wittgenstein called

conceptual puzzlement. As Cavell puts it:

What is left out of an expression if it (is) used outside its ordinary language game? Not what the words mean (they mean what they always did - what a good dictionary says they mean) but what we mean in using them when and where we do. Their point, the point of saying them, is lost. What we lose is not the meaning of our words - hence definition to secure or explain their meaning will not replace our loss. What we lose is a full realization of what we are saying. We no longer know what we mean. (Cavell, 1966:261-262, in Pitkin, 1972:98).

It is at such points of conceptual puzzlement that Wittgenstein's philosophy comes into its own: because in saying we need to take a look at the use of language we can see if we are using the language in a way that *makes sense*. He offers a method for recognizing and possibly helping sort out such problems. As such Wittgenstein's philosophy hardly constitutes a system in the manner that has typified philosophers work in the western traditions since Kant. In this respect Wittgenstein's work is a radical change. But insofar as his concern is with meaning and in particular, how language mediates meaning and thus delineates the explicable world, then Wittgenstein's philosophy is consonant with that tradition.

If this then is Wittgenstein's philosophy, what implications does it have for the doing of social science? As I have explained, I will answer this question by looking at the work of commentators. Their cases, responses to them, and resulting consensus (insofar as there is one) will be surveyed and considered and hence the specific implications

of Wittgenstein's work defined. It is to this task I now turn.

Commentaries on Wittgenstein

It is not immediately obvious what lessons for social science are to be found in Wittgenstein's work. Apart from anything else, he did not ever say anything on the subject directly (with the exception of his comments on Frazer's "Golden Bough" (1969)). His is not a philosophy that resonates with sociological implications like Hegel's. But from what I have shown, it is possible to see that Wittgenstein's work might be useful in helping social scientists *clarify* the language they use. Each of the first three authors I shall now discuss employ Wittgenstein's philosophy to make suggestions as to how we should go about clarifying the purposes and techniques of social science. Their arguments turn around consideration of the ways in which we talk about social action and hence what we mean or can mean in sociological discourse. Each propounds somewhat different views. This partly reflects the distinct audiences each is aiming for, but more significantly reflects a different assessment of Wittgenstein's work. Here I shall not analyse them in detail but rather provide synopses of each for the purpose of distilling a consensus of opinion about Wittgenstein's philosophy. I begin with Peter Winch.

Winch

Basically, Winch argues in 'The Idea of a Social Science' (1958; see also 'Understanding a Primitive Society', 1964: 307-324; 1972; 1976: 322-337) that social scientists make mistakes, conceptual mistakes, about what it is they study. In particular they employ certain, somewhat naive, notions of science or, as he calls them, 'misbegotten epistemologies' (1958:43). One is that human behaviour can be predicted in the same way as concrete phenomena and-or inanimate objects, i.e., and concomitantly, in terms of cause and effect. Winch suggests that social scientists need to abandon these misbegotten epistemologies and reflect on what social science is concerned with. They need to do that before anything else. This would involve philosophical reflection. This is to be contrasted with scientific or empirical deliberations. Such philosophical reflection would lead social scientists to the recognition that the topic of social science is *rule governed behaviour*. This distinguishes it from the study of inanimate, non-human behaviour.

Winch's argument is quite elaborate and covers many topics. Given the need for brevity it is perhaps not unreasonable of me to condense it by considering how his ideas might be applied to the social scientific program known as

behaviourism⁵. According to the behaviouristic model of human conduct, individual actions are "caused" by events that occur within the brain. Since there is no access to these internal states behaviourists argue that to understand what happens in the brain one has to examine what goes *in* to the brain (i.e, external data). Subsequently, the analyst can see what response occurs in terms of behaviour.

Behaviourists claim that, eventually, they will be able to understand and even predict behaviour in so far as they are able to determine how external variables 'cause' internal states and ultimately, external behaviour (see Williams, 1985, Coulter, 1972).

Winch argues that this approach to social sciences is wrong. It derives from a trick of language, or a conceptual confusion. That is, some of the ways we talk about ourselves would seem to imply that there was something internal, some event internal to the body, say within the mind, which causes specific external results or behaviour. But if we consider the language in question thoroughly we will see that to understand someone's action is not to recognise a thing (a thought perhaps) that preceded and caused the act, it is to recognise the *meaning* the act has. And to do that one needs to recognise an act for its purpose, point, or intention. This is not the same thing as discovering an

⁵If they are still not clear however, then the reader will have to bear me with for the moment; but I shall endeavour to further elucidate Winch's case throughout the subsequent discussions.

event independent, prior to and causing the act.

Furthermore the meaning of an act is partly recognisable by the context of its performance. Thus, Winch argues, to understand particular acts and the concepts that make sense of these acts, the social scientist needs to be familiar with the context or community in which the acts take place. A community could consist of, for instance, a group of engineers of similar training or even a small scale society such as a tribe (indeed these are the two examples he uses) or more generally, within a *mode of living*. Winch suggests that the relationship between the meaning of an act or acts and the contexts of its or their performance is an *internal one*. Therefore social science is the analysis of internal relations⁶.

Winch's thesis was not without its critics - many of whom were also advocates of Wittgenstein's philosophy. Some even wondered what it was he was trying to say. The problems in Winch's work appear to stem from the examples he chooses to illustrate his case. In particular his reference to the use of magic oracles by the East African Tribe known as the Azande. This example was used in his article 'Understanding a Primitive Society' in the American Philosophical Quarterly in 1964 (307-324). He uses the example to

⁶The notion of internal relations has of course been of considerable philosophical interest long before Winch adopted the term. Its meaning is far from agreed upon, and is used even by social scientists in a variety of ways.

illustrate the conceptual analysis of internal relations. He shows, for instance, that it involves reference to the context in which meaningful actions occur. Reference to the context in the case of Azande will show, for example, that their use of oracles is not the same kind of activity as that of, say, the use of magic in the west. Not only does each practice occur in distinct contexts, but each has very different meaning. Therefore the concepts associated with magic in the west, such that magic is a kind of inferior rival to science, should not be used to explicate the significance or meaning of magic oracle use by Azande.

The example is deceptively simple and its use leaves Winch vulnerable to a number of charges. On empirical grounds he is criticised for being inaccurate; for over emphasising the distinction between Western and Azande culture; for over-simplifying Azande culture and for ignoring cultural dynamics in Azande. On philosophical grounds he is criticized for implying that Azande magic and Western science cannot be compared because there is no ultimate, or independent criteria against which the efficacy, accuracy, or what you will, of each can be compared. In other words he is criticized for being a 'relativist'. Let us consider the criticisms at greater length. Consideration of them will underline what Winch meant to say.

Most of the empirical criticisms miss the point. It does not really matter what Azande oracle use is really like. I have already noted that Winch had urged social scientists to

reflect on some conceptual matters; these are philosophical and not empirical. Thus the point that Winch wants to make is that if, for the sake of argument, Azande oracle use is like this then it is necessary to take care in deciding what aspects of our western way of life it can be compared to. This is a conceptual distinction. One upshot of it is that one should certainly not begin with a dogmatic assumption that Azande magic is like our science (except for being an incompetent version of it). For though, at first glance, these practices may seem to have fundamental similarities, and may even appear to be rivals, proper, *conceptual* analysis of the meanings of them will make clear that they are different. Basically Winch is employing an extreme case to illustrate a point that more often relates to subtle cases. It is that to understand the meaning of a practice one needs to see how that practice is located in its context. Of course it was, in retrospect at least, unwise of Winch to use a dubious empirical example but, nonetheless, it appears that those who make empirical criticisms miss the crucial issues a hand. Specifically what are the problems of comparison, of understanding? And these are, clearly, philosophical and not empirical.

Those criticisms that claim to be philosophical are also wide of the mark. His purpose was to show the wrong headedness of comparing two practices as if they were the same thing; not to show that particular practices cannot be compared to any other or judged on their own merits. By and

large, however, Winch's article on the Azande makes what is already a difficult and subtle argument particularly unclear and moreover, by encouraging the accusation of relativism, it serves to distract attention from the real issues. Moreover, Winch lacks clarity and consistency. He inter-changes terms like mode of life, mode of living, culture, society and context, for example, in ambiguous ways. The next author I shall discuss does not in contrast, make the same kinds of mistakes. It is to the work of A.R. Louch then, that I now turn.

Louch

Many of the targets of Louch's 'Explanation and Human Action' (1966) are similar to Winch's, but perhaps most attention is given to psychology. "Psychology as a science with its own explanatory laws, falls in the no-mans land between physiology and the ad-hoc deliberations of everyday life" (1966:38). Louch bases his critique on a discussion of the notion of "internal states" and contends that it makes little sense to talk of a separation between the external (behaviour) and the internal (mental), typically an assumption of psychologists, not simply because meaning is incarnate in action but because the meaning of action is to be found in the warrant for the action provided by the context in which it occurs. That is to say, it is the situation that defines the purpose, the intent and the point of meaningful actions. By this token he argues that

analysis of meaningful behaviour is essentially *moral* analysis. Moral in the sense that it is about recognising and describing (explicating) how conduct is appropriate for specific contexts. On this basis, Louch argues that social science is essentially evaluative and critical. Thus the emphasis in Louch is somewhat different from Winch who prefers the phrase 'analysis of internal relations' to *moral analysis*.

As part of his general case Louch also notes how it is that explanations of conduct in ordinary, everyday life, do not rely on scientific theories, apparatus or paradigms; they are always essentially ad hoc, common-sensical, piecemeal and specific. He argues that not only does this bring into question the status of scientific theories about social conduct - since they are never used in the ordinary world - but begs the question of why they were devised in the first place (and thus his criticisms of psychology). Moreover, to the extent that common-sensical explanations are specific and piecemeal it also means, according to Louch, that social scientific explanations need to be specific and piecemeal. This is antithetical, of course, to the bulk of social scientific theorising. Taken as a whole, Louch's thesis may be defined in the following paraphrase: like Wittgenstein and his observation that ordinary language is alright, so Louch is saying that ordinary explanations are alright.

If Winch's thesis came to be the subject of considerable debate the same could not be said of Louch's work. Though

it is still referred to as an exemplification of the implications of Wittgenstein's work, the specifics of his arguments are largely forgotten. There is no obvious reason why this is so. One reason may be that he does not get involved, like Winch, in unwieldy examples which allow for empirical disputes. Instead Louch concentrates on the use of particular terms in the explanation of human conduct (for instance, motives, needs, pleasures, desires, intentions). He shows how each can only meaningfully be used in ways already mentioned - in specific and piecemeal ones. That Louch's explanations are somewhat broken up, and turn around the use of multiple, but very specific examples, is indicative of how closely he tethers his thesis to the way Wittgenstein reasoned. For, in the *Philosophical Investigations*, Wittgenstein does not follow any definable system or build any general theory but investigates many instances of reasoning. It is therefore also seemingly piecemeal and seems to lack narrative continuity. He argues this way because language is piecemeal and ad-hoc. But that the upshot of all this is that social scientific reasoning can itself only be piecemeal and specific, is such a radical thesis that it runs counter to almost the very foundations of social scientific thought. It is this radical-ness, perhaps more than anything else, that explains the obscurity into which Louch's 'Explanation and Human Action' has fallen.

Pitkin

Pitkin's work is both the most recent of these discussed and perhaps the most specific. She asks what are the implications of Wittgenstein's work for the study of law and jurisprudence, rather than more generally, social science. Nevertheless she does take up the same themes as Winch and Louch, only altering the emphasis.

Whereas the other two authors tend to focus their discussions on the question of causality, Pitkin gives central prominence to the diversity of language games. She points out that language games can make the particular meaning of words seemingly contradictory and that almost any attempt at the definition of words (for example the various attempts to define 'Power') is almost bound to fail since it will (by necessity) preclude certain uses or meanings. Anyway, definitions of words are not what we need. Wittgenstein had tried to show that the trouble with philosophers is that they act as if they did not know what particular words mean (like subjective, meaning, understanding) when they perfectly well know this - after all not being able to define a word is not failing to understand it (one may recall the quote from Cavell earlier on in the chapter). Social scientists seem to make the same mistake as philosophers - that is 'pretending' not to know. To fully appreciate the world, Pitkin argues, we need to consider language in all the diversity of its uses. Even

seemingly misappropriated language games like the notion that all social action can be explained causally, should not just be dismissed but should be considered as an illustration of why certain types of conceptual puzzlement occur. Hence the question should be asked why have social scientists, be they psychologists or sociologists, used language in this or that way? What can be learnt about both the disciplines of sociology and psychology and the nature of language if we attempt a Wittgensteinian unravelling of the various instances of conceptual confusion? One possibility is that social scientists like to 'fiddle' (Pitkin, 1972:174) with concepts like power; another is that they like to choose between ways of explaining conduct so that some kinds of explanation, which are perfectly reasonable, are precluded. As she puts it in connection to methodological individualism and methodological holism:

Too often the philosopher or social scientists feels it imperative to choose between... perspectives, assuming that there must be a single consistent reality - either that of [for instance] the individual and his experiences or that of the society in which they take place... (Pitkin, 1972:286. Brackets added).

Pitkin suggests that this keenness to choose and thereby preclude is partly because social scientists are aware that certain forms of expressions do not rest easily amongst others. Some appear resistant to use in certain ways. For example the language of the observer does not mix easily with the language of the participant. When the observer

tries to talk as if he or she were a participant we can neither accept that he is a participant or an observer. This begs the question of what status we should give his or her description. It is to avoid such problems that social scientists try and use only a limited number of explanatory forms.

It is with discussions like these that Pitkin pursues her argument. She draws attention to the enormous subtlety and complexities of language. Whereas Winch and Louch tend to provide dictums, Pitkin provides a warning: do what you will with language but attend to what you say for it is easy to lose meaning in the process of saying.

Despite the elegance of her writing, Pitkin's work has, like Louch's, become submerged in the rancour surrounding Winch's earlier work. By the time her book had been published minds had been made up and articles claiming to address what had become the Winch debate rarely quote from 'Wittgenstein and Social Justice' (see for example Lamb, 1976:689-719; an exception to this is Hughes 1977:721-741).

These then are three interpretations of Wittgenstein's later philosophy. Each has certain unique elements but all can be seen to maintain a certain consistency of argument. Before I go on to specify what recommendations can be distilled from these authors I shall now briefly turn to some of the critics of both these interpretations and of Wittgenstein's philosophy itself.

Critics of the Later Philosophy

Gellner

Perhaps the most common criticism made about Wittgenstein's philosophy is that it implies relativism. (As a side consequence of this it is also said to imply cultural ethnocentrism). By this is meant that a person's morals or values, are not absolute but are determined by his or her position in the world; that is *relative*. This criticism turns around a notion I have only discussed obliquely in reference to Winch, that is, 'form of life'. In fact, Winch interchanges this term with a variety of others, most commonly, 'mode of living'. Wittgenstein had originally used the term in the *Philosophical Investigations*: 'To understand language is to understand a 'form of life' (1958: 40). Unfortunately the concept has become something of a shibboleth. This is illustrated by Gellner's comments.

The first wave of Wittgensteinians, including the master himself, using the perception merely to beat rival theories of language, do not worry too much about the fact that 'forms of life' (i.e. societies, cultures) are numerous, diverse, overlapping and undergo change. Which of them is to be accepted? All of them? Or each of them on the principle when in Rome do as the Romans do? (Gellner, in Giddens, 1974:137).

Gellner argues that Wittgenstein does not offer a solution to this problem or a method for choosing between forms of life; they are all, if you will, equally valid. To the extent that Wittgenstein cannot or will not choose between forms of life, Gellner asserts that he must be a relativist.

This is because Wittgenstein seems to refute the existence of any external criteria whereby forms of life can be judged. It will be recalled that Winch was criticised for much the same reason. Gellner goes on to say, however, that though Wittgenstein was a relativist in theory, in practice he was not:

For Wittgenstein (relativism) was a solution not a problem. It was a rather special kind of relativism, with as it were only one term, in the abstract, a general relativism of 'form of life' was formulated but in application only one form of life was considered - that of the academic philosopher and his disciples. (Gellner, 1974, in Giddens:154).

So, what exactly did Wittgenstein mean by form of life? Did he mean things that change just as societies (and language) change over time and space? One way of beginning to sort this out is by suggesting that Wittgenstein meant something like the following: a form of life is that cultural nexus in to which language games fit and which is part of how we make sense of and use those games. For example, the Azande use of oracles is to be understood within their form of life. But there seems to be a considerable lack of clarity, possibly confusion, even when one makes such a brief and simple definition. One aspect of this confusion seems to relate to the level of generality implied when the term is used. Winch, for instance, uses it on some occasions in reference to very particular things, like specific aspects or *forms of life* of the Azande; but at other times in terms of very general things such as the life of human beings. Thus, in relation to the latter, he makes reference to such

features of human existence as birth, sex, families, death etc, and says that these are the bedrock of our form of life. But, to make it all the more complicated, he implies, with the Azande illustration, there are considerable differences in the specificities of life which are built on this bedrock.

Pitkin, in contrast, tries to explain what is meant by form of life by dint of explaining what underscores language games. (This is, in fact, more or less what Wittgenstein attempts to do in *On Certainty* where he tries to discuss what is assumed in any language game by showing, paradoxically, how difficult it is to discuss this without losing sense). Pitkin comments:

Our playing these language games rather than others is the result neither of accident or of arbitrary free choice. It is a result of what the world is like and what we are like, what we naturally feel and do. (Pitkin 1972:123).

In other words, the basis of our language games are not conventions, i.e., things that can change, say over time and space.

It may be that what makes it difficult to explain the concept of form of life is that it refers to what *has to be* assumed; to such things that are so basic that to dispute them would bring in to doubt one's competence as a rational being. Consider the following example taken from the *Philosophical Investigations*. Wittgenstein spends some time on measurement. He says: "What we call 'measuring' is partly

determined by a certain constancy in the results of measurement" (1958:242). That is to say, though the choice of inches or centimetres is arbitrary or conventional, that we can measure anything itself *cannot* change. It is basic to our form of life. Let me put this another way, if we suddenly found ourselves in a world where we could not compare measurements, then the world would have changed very radically indeed: in fact it is difficult to imagine such a world, let alone envision its practical features. For example if "The procedure of putting a lump of cheese on a balance and fixing the price by the wieght on the scale would loose its point if it frequently happened that such lumps suddenly grew or shrunk for no obvious reason" (1958:142). In a phrase, then, Wittgenstein's argument is that the world does not change in respect to fundamental features of living, nor does it vary around the world; but he does not attempt to define what are the basic features of our form of life. *It would make no sense to*. His argument does not, therefore, equate or even imply relativism. The notion of form of life is, it has to be admitted, one that can mislead if it is used out of the context of the arguments Wittgenstein is involved with. But when it is recognised that the concept is intended to help explain *how we understand* then the accusation of relativism is seen to be false; or more exactly, an *irrelevant* one.

It must be made clear however, and despite the foregoing, that Wittgenstein is not arguing that there are no

differences in languages, or differences in what people do. But these relate to what may be thought of as the social institutions of people - ironically the very thing that is the concern of social scientists. It may be, therefore, that the concern of social scientists with these institutions leads them to misunderstand Wittgenstein.

Giddens

Giddens formulates his version of Wittgenstein somewhat differently from Gellner: he places emphasis on the "rule following" which Winch had located at the centre of his discussions about acting meaningfully. According to Giddens, Wittgenstein is claiming that such rules are never fixed or given processes, but change over time and space (1979:41). If this is so, then there are no ultimate rules whereby the rest can be judged. Thereby one comes back to the same criticism that Gellner made: relativism. However, insofar as rules are a feature of a form of life then, despite the difference in emphasis given in Giddens reading of Wittgenstein, it seems reasonable to reject the criticism on the same grounds.

But there are lessons to be learnt from considering Giddens' other criticism. Giddens argues that Wittgenstein's philosophy (and ordinary language philosophy as a whole) is fundamentally lacking in any critical quality. In particular, it ignores the question of power (1979:50 & 256). This a problem, Giddens asserts, because the ability

to define a language game, as one thing or another, constitutes an exercise of power by the individual (or a group of individuals) who have their interpretation held to be true rather than any others. Wittgenstein's failure to point to this is, Giddens pugnaciously claims, symptomatic of the smug complacency of ordinary language philosophy.

Let me consider what Giddens means. For simplicity, I will use the example of measurement again. One is tempted, when reading Giddens, to employ the same kind of mocking tone he does. Thus one might suggest that he is claiming that, for example, the fact that cheese does not vary in weight (or size) whilst being weighed is something to do with the exercise of 'power'. But leaving aside this temptation, it would appear that he is saying that weighing itself (rather than something to do with the cheese) is an exercise of power. For the need to sell things or even the possibility of selling things presupposes an ability to assess or *measure value*. There can only be a value if there is simultaneously a concept of ownership. Or rather, there is little point in weighing something unless someone owns it (otherwise people will just take what they want). Ownership itself can be thought of as based on the power to maintain that ownership.

This way of talking about 'weighing' however, bears little relationship to how Wittgenstein might have considered the topic. Wittgenstein would probably say that involving the

issue of power and value would only serve to confuse the issue of how to analyse the language game of measurement. But there is a more important point: that Wittgenstein was interested in considering weighing, as an activity, as part of his general intention to clarify the sorts of 'problems' that had beguiled philosophers. Often these problems derive from getting things mixed up, like the facts that something has a value, that its weight is constant and that it can be weighed. This mixing up manifests itself in various ways, but often in the confused use of terms in language games. That is, Wittgenstein wants to deliberately separate or untangle issues such as measuring, value and power so as to sort out what can and cannot be said and hence what we mean when we talk about something like measuring. In short then, Wittgenstein is interested in the way the misuse of ordinary meaning creates philosophical confusion. Not whether certain groups of people have the right to own objects or to define which interpretation of the rules 'count'.

Giddens in contrast, wants to mix them all up. It seems to me, that this may be due to Giddens wrongly assuming that Wittgenstein is propounding a theory about society. A theory of society would naturally (or more exactly, is most likely to) consider power as it will do many other things. But in contrast, Wittgenstein is primarily interested in philosophical problems. Hence he analyses language games to see how the misuse of terms within those games can lead to philosophical confusion. If confusing sociological and

philosophical concerns is the cause of Giddens misreading, then he is not alone in making it. For it is a very common mistake. If nothing else, it underlines Winch's observation that social scientists need to engage in some conceptual analysis. For if they did so they might realise that philosophers are not always concerned with the same things as social scientists.

Hekman

Hekman (1983) has a more thorough understanding of the implications of Wittgenstein's philosophy than Giddens. For a start she acknowledges that Wittgenstein does not provide a particular *rationale* or corpus of concepts with which to do social science but provides a method for getting out of conceptual problems. Yet paradoxically, Hekman appears to be suffering from a conceptual problem of her own, one which derives from Max Weber.

According to Hekman, early interpretations of Weber's '*verstehen*' method argued that it involved, to use a metaphor, getting 'inside a social actors head' (1983:90). Thereby analysts' could re-live the experience of the actor and thus understand his or her decisions. More recent interpretations of the *verstehen* method have understood it not as a process of intuition or re-living but a question of gaining access to 'subjective meaning'. This can be done via analysis of the meanings of an individual's actions (1983:97). She suggests that the social scientist and

philosopher, A. Schutz held this latter view. She argues that Wittgenstein, in contrast to Schutz, claimed that subjective meanings are beyond the intelligible and that therefore the *verstehen* method, at least its more recent version, is a waste of time (1983:38). This is because Wittgenstein argues, at least according to her, that there are only publicly available meanings, not subjective ones. Louch, she goes on, follows Wittgenstein's lead and treats mental events as "irrelevant". However, she continues, what Wittgenstein does is not actually say that subjective meanings do not exist, only that they are impossible to analyse. Yet if they are there, Hekman asks, surely we must inquire into them, especially for social scientific purposes. She suggests that all we need is a method to study them. This method can be found in Schutz's interpretation of Weber's *verstehen* approach. Thus her case is that Wittgenstein offers the social scientist only a limited approach to explanation. It is limited by the extent to which it does not incorporate or precludes analysis of subjectivity.

However, this is incorrect understanding of Wittgenstein's (and hence Louch's) position. To start with I have shown that Wittgenstein did not have a position, in the sense that his critics would like him to, only a method for sorting out problems. Hekman recognises this. But she fails to see that the approach is designed to sort out problems of precisely the kind she is interested in, i.e, for instance, the

problem of meaning and subjectivity. If one uses Wittgenstein's philosophy in relation to subjectivity one will see that it involves looking at the ways one talks about the self, "what goes on inside my head" and so on. These ways of talking might give the impression that there is something like an internal state, the subjective mind, being referred to. But careful deliberation will make one realise that when one talks about such matters as "That's how I see it", or "My experience is different from yours" one is not suggesting that there is some internal view of the world that is different from another's, rather, what one is doing is more like a performative utterance in the Austinian sense⁷. It is to say perhaps, that there is a disagreement about some matter, about how to interpret some event and this performative utterance provides grounds for the difference. It is to suggest that there were circumstances which resulted in this difference. Perhaps one person did not have the same information as the other or one was more subject to emotional interpretations in regard to that matter. And so on. This is not to say that internal states exist but that there are understandable reasons for people having different *opinions*. And moreover, if one thinks carefully about these ways of talking we will remind ourselves that it needs no special methodology to gain access to someone's "opinion" or "point of view", simply the

⁷See Austin's 'How to do things with Words' (1955); also his 'Philosophical Papers' (1961).

patience to understand (but of course this is no easy matter⁸).

In short, Hekman's critique flounders because her main interest is a false one - how to create a method to understand individual subjectivity. We need no method. We ordinarily understand each others 'opinion', interpretations and so on adequately; and moreover we treat them for what they are: just opinions, points of view and so on; and certainly not some thing -a state perhaps- that needs a scientific method to be understood⁹. Wittgenstein did not preclude analysis of such things, but provided an approach that would enable us to make sense when we did so.

Conclusion

As I have said my purpose has not been to provide a detailed analysis of the advocacies and critiques of Wittgenstein's philosophy but to use these as a resource to characterize the implications of his work. Let me recapitulate the arguments I have covered. I will begin with the criticisms.

I discussed three different types of criticism. The first was that Wittgenstein's philosophy was relativist. Both Gellner and Giddens argue this. I argued that Wittgenstein's system should not be so interpreted. It seems to me that two

⁸It might be added that we even have a vocabulary for those who are good at understanding: i.e., she's very sympathetic, etc.

⁹As it happens, it was Weber's overriding intention to provide a scientific basis for sociological analysis.

points are brought home by this. The first relates to the concept of 'form of life'. It will be recalled that this concept was central to Gellner's remarks. One possible reason for misunderstanding the concept was the ambiguous ways it - and similar ones - are used by commentators like Winch. But more importantly, and this is the point I wish to draw attention to, that Gellner fails to recognise what Wittgenstein is getting at with this concept seems to confirm, in part, what Wittgenstein was trying to say. For, according to Wittgenstein, forms of life, which are the basis of language games, are so fundamental to our existence, and hence to the way we understand, that it often makes no sense to define them. They have to be assumed; not for analytical purposes, but because they are the assumptions that we, unwittingly, base our lives on. Thus Wittgenstein does not, for reasons internal to his argument, define forms of life. But in not doing so, he leaves an opportunity for those who, like Gellner, have not understood the argument to produce their own definitions of the term. And this is precisely what they do. Gellner, it will be recalled, came up with a definition of one form of life: that of the academic philosopher. This leads me on to the second point.

Wittgenstein was very critical of philosophers (and by the same token social scientists) who 'pretended' to doubt things for the sake of analytical inquiry. For instance, philosophers would doubt such things as understanding. But

they can also doubt the very basics of our existence, or aspects of our form of life. For example, they might doubt something like the fact that cheese does not vary in weight. Now Wittgenstein's case, as I have just noted, is that it makes no sense to doubt such things. To do so in ordinary life would lead, for example, to the questioning of someone's sanity, not admiration for their philosophic ingenuity (a response that might occur in philosophy seminars when language can go, to use Wittgenstein's words, 'on holiday'). One concomitant of this wish to doubt things is the need to define things. Defining a form of life would be a case in point. So, Gellner's criticism is instructive in two ways: it underlines the importance of the notion of form of life and reminds us that what this concept refers to is, in important respects, not to be defined.

The criticism Giddens makes offer us another lesson. Primarily, it shows us that social scientists often mistake *social theories* with *philosophical* ones. For, his criticism turns around the claim that Wittgenstein's model of the social world does not incorporate all the important factors, most especially *power*. But, in fact, Wittgenstein is not interested in providing a model of the social world. It was not his purpose to provide a social theory. Wittgenstein was interested in resolving *philosophical* problems, not *sociological* ones. As I have noted at several places, this involved looking at confusions about meaning in language, not about how society is organised. (Although of course,

Wittgenstein makes reference to this organisation). That Giddens is mistaken about this would seem confirm Winch's claim that social scientists must engage in some form of conceptual analysis. They must recognise, in other words, what it is they are dealing with. One consequence of this would be, hopefully, that they would realise that philosophy, although of some relevance to social scientists, is often interested in other matters.

The third criticism, that Wittgenstein ignores subjectivity, shows the everpresent temptation to make conceptual mistakes. For the way that Hekman formulated subjectivity, and claimed it was a "problem", was the result of a conceptual confusion in language. It derives from some of the *implications* of the way we talk about our 'opinions' and so on. But, on close analysis, such implications will be seen to be false, because they are not what we *mean*.

These criticisms then, underline the complexity of the concept *form of life*, the almost pathological need amongst philosophers to doubt and define, and the pervasiveness of conceptual confusion. I think it necessary to do so partly because advocates of Wittgenstein's work do not always make the issues clear and partly because critics often misunderstand them. I shall return to some of these issues again; but first let me recapitulate the cases put forward by Winch, Louch and Pitkin. Then I can begin the task of coalescing the arguments.

Winch, it will be remembered, developed a thesis that social science should engage in some philosophical deliberations for it needs to define, conceptually, what are its topics. These deliberations are about epistemological matters. Winch suggests that once this has been done, social scientists would realise that their task is to examine the internal, logical relations between meaningful actions. Further, Winch argues that the terms and language of analysis must be based on actors' own terms if it is to have any claim to empirical accuracy. Subsequently, Louch, in his book, though supportive of much of Winch's thesis, criticized Winch for implying that there should be no evaluation of behaviour. Louch pointed out that it is intrinsic to the ways in which we talk about social behaviour to evaluate, or assess the competence with which it is done. Thus social science would not be the analysis of rule governed behaviour, but *moral analysis*. In addition Louch argued that social scientific explanations should reflect the nature of ordinary explanations, that is to say, they should be specific and piecemeal. Finally, Pitkin argued that there is no need to limit the scope of social scientific explanations. Instead we need to be extremely careful about how we say things and hence 'what we say'. One of the problems she saw in both the work of Winch and Louch was an over keenness to banish such things as causal explanation from social science or as in the case of Louch, explanations that are general (as against specific). She argues that there are occasions when

generalisations and the use of causality are appropriate - or at least interesting. Likewise there should be no requirement for explanation to stick to 'actors terms' since certain language games use or incorporate ways of talking apart from actors' own which are 'adequate', and sensible (even so, she notes, explanations must take account of actors' views if they are to remain plausible).

One of the first things that comes to mind here is that there is an obvious difference between Winch and Louch on the one hand, and Pitkin on the other. This difference turns around two things: the central place given by Louch and Winch to actor's or participant's terms; and second, the greater emphasis Pitkin places on the notion of language games and how there may be a variety of language games used in social scientific analysis. Both these points need some further consideration. But before I do so I think it is necessary to recognise that these authors are all considering social science at a conceptual level. Insofar as they do, then Louch and Pitkin are following in Winch's footsteps. All want to engage in conceptual analysis because they want to define what is distinctive about the study of human behaviour. Winch argues that what is distinctive is that it is analysis of the internal relations between meaningful actions; Louch that it is the analysis of moral behaviour. I do not think these differences in terminology are significant: for both authors, as well as Pitkin, are drawing attention to the same thing: the paramount^{ty} of

meaning in human behaviour. Now this is, of course, nothing new. But all three authors want to draw attention to another matter that has not been discussed, to my knowledge, outside the Winch debate. For they emphasise the relevance to social science of how people in ordinary everyday situations explain and interpret their behaviours.

Everyday Discourse

Winch, Louch and Pitkin argue that social science should model itself on, or should learn from, everyday techniques of description and explanation. Winch for example, notes how individuals draw on the general cultural context when explaining and understanding their own activities. Thus, he contends, social science should also attend to matters of context. Louch argues that most explanation is piecemeal and ad-hoc; therefore social scientific explanations should have a similar character. Pitkin argues that ordinary explanations do many things or have different purposes. These different purposes are manifested in distinct language games. Social science should consequently feel no restraint about the numbers of language games it employs, but should use all and any of those used in interesting ways in everyday discourse. But in doing so it should make sure not to get these games mixed up.

Leaving aside the different emphasis each gives to aspects of everyday discourse (and I think they are less significant than might at first appear), one reason why these three

place such importance on it is that it is grounded by its practical efficacy whereas social scientific description is not.

Let me illustrate what the issue is here. Consider, in ordinary life not everything is in need of an explanation. Some things are so basic or so obvious that if an individual were to start describing them it could lead to his competence, even sanity, being brought into doubt. For instance, it is unlikely that anyone would describe how they recognise that two people, say on the other side of the road, are walking together. *For anyone could see.* Of course it may be that someone might ask: are those two together? when it was not so clear. One can easily imagine scenarios when such a question would be sensible. For a couple may want to disguise the fact that they are together: they are illicit lovers for example. But no one would say 'I can see that those two are walking together because they are walking two feet from one another' or 'because they constantly look at each other'. There is no need to say such things. In other words, there is no need of explanation. There are many other activities that do not need to be explained since they are instantly recognisable: that two people are in love or are arguing and so on. Sometimes social scientists attempt to explain precisely these things: in other words they analyse what does not need explaining. But because their work serves no practical function, it does not matter that they describe such things.

There is no body to say, if you will, 'how odd that you explain this'.

That social science should ever want to explain such things is perhaps not only because it fails to take account *what* people describe in everyday life. It may also derive from an excess of doubt (and concomitantly the need to define). I have already mentioned this in reference to Gellner and the concept of form of life. Wittgenstein offered some partial explanations for why individuals, especially academics, suffer from an excess of doubt. One is that in academic discussion language has no practical grounding. Thus academics are led to ask questions that have no practical justification or purpose. (Once again then, one is lead back to the need to base social scientific studies in the descriptions, topics and language of everyday life).

There is another side to everyday discourse that may be worth noting. Just as *some* things are not explained because no explanation is needed, so in everyday life, explanations that are too complicated are dismissed because they are too complicated. We have a number of words for this particular problem. I won't bother mentioning them here. But sometimes complexity seems to be the very goal of social scientists. I think this is a highly pervasive problem in the social sciences. One reason for it may be that social scientists like to 'show off' with complex arguments; another may be that it is difficult to resist adding more to an explanation when there is no-one to say 'enough!' It may be that Louch

is relevant here, reminding us to abide by the piecemeal and presumably economical descriptions of everyday life. Another possibility is that diligent use of Occam's razor would sufficiently tame the verbal excesses of social science¹⁰.

These then, are some of the considerations underscoring the claim that social science should take account how people explain and describe in everyday life. There is still one other matter to clear up however. I noted that Pitkin can be distinguished from Winch and Louch by her unwillingness to place actors terms at the centre of social scientific analysis; and because she places greater emphasis on language games and the problems that emerge when they are mixed. I think it is clear, from the proceeding, that one test of empirical accuracy is the concordance of social scientific descriptions with actor's own. But it may be that social scientists do not always want to place a premium on being empirical. And here, I think, Pitkin's emphasis on language games is pertinent.

Pitkin suggested that one reason social scientist's get confused is because they do not want to stick to participant's terms. They may want to take a different approach to that employed by participants themselves. But

¹⁰Cornelius Disco (1976:265-287), basing his arguments on Wittgenstein, goes somewhat further when trying to answer the same question. He says that academics need to publish and hence explain even what does not need explaining just so as to be able to say something that no else has (the fact that no-one would want to, Disco remarks, is irrelevant to them).

the language they sometimes opt for when taking a different approach occasionally makes social scientists uneasy. Consequently, they make the mistake of, for instance, operationalising terms or banishing the use of certain others. Social scientists do this, Pitkin argues, because they recognise that they are using language in confused ways. Their efforts at operational definitions and so forth are attempts to deal with these confusions.

Pitkin suggests that these confusions can be seen to derive from *the mixing of language games*. For instance, one game might involve referring to participant's understandings, another to those of an external observer. Confusion might emerge when the two games are mixed or merged. One way of avoiding such confusions, Pitkin suggests, is to treat these games as distinct and to maintain that distinctiveness in analysis. Thus one study would stick to participant's understandings; another to those of an external observer. Such a procedure would minimise the confusions that occur when language games are mixed.

One can push this argument further. I do not think it unreasonable to suggest that one can treat different social scientific approaches as different *language games*. So, consider the two sociological approaches known, respectively, as ethnomethodology and dramaturgy. The former places members' understandings in the centre of analysis whereas the latter seeks to see how behaviour can be seen to

be like theatrical behaviour¹¹. Now it seems to me that each of these approaches is legitimate; although ethnomethodology could claim to be more empirical to the extent that it attends more closely to participant's understandings. Any attempt to integrate the two would, I believe, lead to serious ambiguities. It would become, for example, difficult to distinguish between metaphor and literal description. It would also be difficult to know what the purpose was: to account for member's understandings or to see how they behave in theatrical ways. If however, the two were kept separate these problems would be, to some degree, avoided.

These then, are some of the implications of Wittgenstein's philosophy for the social sciences. As I say, perhaps the most important of these is that social science should reflect on and take account of everyday discourse. The topics, simplicity and manner of this everyday discourse is indicative of what can and cannot be said in social science. At times social scientists may get confused not because they fail to take account of this discourse (although sometimes they do), but because they mix up its forms. Such mixing also occurs I suggest, when social scientists attempt to mix or integrate approaches to social science. To avoid this I have argued that different approaches be treated as

¹¹Another possible illustration is by looking at the so-called micro-macro distinction. Some of the more recent anthologies on the matter are: Knorr-Cetina & Cicourel, (eds), 1981; Fielding, (ed), 1988.)

different language games. Needless to say, my discussion of the Winch debate will have passed over some interesting issues. Some of these I am fully aware of - such as my failure to touch upon Louch's emphasis on the evaluative aspects of social inquiry. Others I will have unwittingly missed. These absences aside however, I hope in this chapter to have justified the notion that Wittgenstein's work is vitally important for the doing of social science.

CHAPTER THREE

APPROACHES TO SOCIOLOGY AND CONCEPTUAL CONFUSION

Louch and Winch argued that social science must focus its attention on the understandings of participants. In that the sociological approach known as *ethnomethodology* does so, it might seem reasonable to suggest that is *Wittgensteinian sociology*¹. This, I think, would be unwise. As I have noted, Wittgenstein did not have a program for sociological research, and, apart from their dicta about social science having to be the analysis of internal relations or moral behaviour, nor did his advocates. Moreover, Ethnomethodology derives its background, at least according to its founder, H.Garfinkel, from phenomenology and not ordinary language philosophy. Nonetheless, it is uncontentious to claim that, to the extent that it abides by a concern for actor's understandings, then ethnomethodology is empirical. Certainly more so than those approaches which do not place so much emphasis on actor's understandings. My elucidation of the implications of Wittgenstein's work has shown on what basis such a claim can be justified. But sociology is concerned with more than just being empirical (although that is no easy matter). There is also the question of what interest sociology might have in participant's understandings. Ethnomethodology for example, is interested

¹ See for example, Phillips:1978; and also Heritage's response in the same journal.



in how participants - or 'members' as it prefers to call them - use their common-sense knowledge of the world to make their behaviour *accountable*. This might appear quite an arcane notion. What, for instance, does accountable mean here? In the first part of this chapter I will fill out what this term, and hence what the ethnomethodological program is all about. I will review some ethnomethodological research including *studies of work*. This will provide a background to the use of the ethnomethodological approach in latter chapters. It will also prepare the ground for consideration of what I believe are some of the conceptual confusions in ethnomethodology. These confusions, I will argue, often derive from mistakes about language categories and from the mixing of language games. In particular I will look at the term *reality* and suggest that several authors get confused and misuse it; at how certain sociological terms get misused and finally at the difficulties and confusion that ensues when ethnomethodological studies are evaluated by methodologies taken from other, in this case specifically statistical, approaches.

Ethnomethodology

Ethnomethodology emerged in the middle of the late sixties. The central figure was Harold Garfinkel, whose charismatic manner had much to do with ethnomethodology's cult like status (Coser, 1975; Wood 1976; Zimmerman, 1976). The ethnomethodological program was centrally concerned with explicating the so-called 'problem of social order'. It was

not a replacement of other approaches but was designed to supplement and extend the topics of social inquiry.

Garfinkel made all this clear in his preface to *Studies in Ethnomethodology* (1967)². Since these words seem to have been forgotten by some of the authors I shall discuss later on in the chapter, it is perhaps worth quoting from the preface at length:

In doing sociology, lay and professional, every reference to the 'real world', even where the reference is to physical or biological events, is a reference to the organised activities of everyday life. Thereby, in contrast to certain versions of Durkheim that teach that the objective reality of social facts is sociology's fundamental principle, the lesson is taken instead, and used as a study policy, the the objective reality of social facts as an ongoing accomplishment of the concerted activities of daily life, with the ordinary, artful ways of that accomplishment being by members known, used and taken for granted, is, for members doing sociology, a fundamental phenomenon. Because, and in the ways it is practical sociology's fundamental phenomenon, it is the prevailing topic of ethnomethodological study. Ethnomethodological studies analyze everyday activities as members' methods for making those same activities visibly-rational-and-reportable-for-all-practical-purposes, i.e., 'accountable', as organizations of commonplace everyday activities. (1967:vii).

The term ethnomethodology was chosen because people employ methods in the ways they make their activities accountable, and because the term 'ethno' had been used to describe studies of methods in such areas as biology, chemistry and music. It made sense therefore to join the two terms to make

²There are a variety of commentaries on Garfinkel's program. See for instance Benson & Hughes, 1983; Handel, 1982; Heritage, 1984; Sharrock & Anderson 1986.

ethno-methodology³.

The bulk of *Studies in Ethnomethodology* consists of empirical examination of people's methods to make their activities accountable. Garfinkel looks at how jurors go about making their work accountably and demonstratively the work of jurors (1967:104-116); how transexed persons demonstrate their gender (1967: 116-186); how clinic record keepers display their conduct as organisational conduct (1967:186-208).

All of these studies are remarkable for the detailed manner in which they examine the ways those under study 'reflexively' accomplish their tasks. Examination of these ways or methods made available for the first time description of the skills involved in the doing of daily activities. Hitherto, these skills had been confined to a residual status in sociology, so much so that it was almost as if people did not need to use any skills in their daily conduct. They were assumed to be, methodologically, little better than 'judgemental dopes' (1967: 76-104).

Garfinkel's work was at once hailed as of enormous significance. Yet not everyone agreed. Some suspected it to be undermining the very nature of sociological inquiry (Coser, 1976) or a symptom of the decay of sociology

³As Garfinkel notes however, in Turner (1974), the term became a shibboleth in the rancorous debates that followed.

(Gouldner, 1970)⁴. But these criticisms notwithstanding, Garfinkel was not alone in his endeavours. He gathered a number of colleagues around him and these pursued his program in a variety of empirical locations⁵. Since Garfinkel's empirical work is well known, I will review some of the work of these other authors, rather than Garfinkel's own. Furthermore, these authors write concisely, clearly and in correct grammar. Garfinkel's work is not only famous for its sociological originality, but notorious for its anacoluthia.

⁴ Space precludes consideration of critiques of ethnomethodology. This is not too great a deficiency, for many of these critiques are based on misunderstandings that derive from conceptual confusions. Insofar as this chapter specifies the kinds of problems that confusions result in, then some of the grounds for these critiques will be removed.

⁵ Edited collections include: Douglas, 1967; 1971; Psathas, 1979; Sudnow, 1972; Turner, 1974.

Other Studies

One of the most cogent exponents of ethnomethodology was Egon Bittner. He discussed the concept of organization (1965;1974:44-72). Bittner argued that it was often the case that sociologists would provide their own definitions of the concept of organisation. This definition would be used to explain organisational conduct. Bittner claimed that such an analytic procedure was flawed. For it was difficult to know what status the sociological conception of organization was to have. It was not tied, for example, to laypersons' use of the concept. Rather it appeared to be a product of some kind of fusion between laypersons' conceptions and the sociologists own. Was it therefore a rarification of laypersons' concepts? Or was it intended to be a purely theoretical conception in the way Weber's ideal types purported to be? Bittner suggested that all this confusion could be avoided, and some form of empirical and methodological clarity be gained, if the way that real members of an organization used the concept of organization themselves became the topic of inquiry. He says:

The study of the methodological use of the concept of organization seeks to describe the mechanisms of sustained and sanctioned relevance of the rational construction to a variety of objects, work and occasions relative to which they are invoked. (1974:56)

He suggested three ways in which the concept of organization might be used by laypersons or 'members'. It might be used to explain or justify "compliance" to rules, codes of

conduct, etc.; or it might be required to explain some "stylistic unity" within the organization, say in regard to professional manner; and lastly it might be used as a "corroborative device" to explain or justify seemingly disparate activities. These suggestions have not been taken up for empirical examination however, and have remained interesting suggestions (Watson, 1987) with the exception of the gambit of compliance.

D.H.Zimmerman also used the ethnomethodological program. But instead of looking at the concept of organization, Zimmerman wanted to see how individuals made sense of 'rules' (1969 (a); see also 1969 (b);1971). He suggested that the ability to make sense of rules was displayed in individuals' capacity to abide by them throughout the working day despite, and indeed as a consequence of, the practical exigences they had to deal with.

The competent use of a given rule or a set of rules is founded upon members' practical grasp of what particular actions are necessary on a given occasion to provide for the regular reproduction of a normal state of affairs. A feature of members grasp of his or her everyday affairs is the knowledge gained by experience of the typical but unpredicted occurrence of situational exigences that threaten the production of desired actions - often the troubles develop over which little control is possible, save to restore the situation as well as possible. Certain exigencies may be dealt with in an ad-hoc basis and others may be provided for systematically. (1971:14)

Zimmerman's concern was not to give the impression that organizational actors can make up rules how they want, but that organizing themselves so that their conduct conformed with a rule involved adaptation, interpretation, skill. So,

to make sense of a rule, how it was or was not to be applied, required filling out the details of the rule's meaning and purpose in a variety of contexts, some typical and routine, some not.

Somewhat later than Zimmerman and Bittner, but still part of the same general program, D. L. Wieder analysed what he called the convict code in his book 'Language and Social Reality' (1974(a); 1974(b):144-175). The code was a set of maxims which indicated that convicts or parolees should not help parole officers, the staff of halfway houses, and the like. If they did help they would be seen to be betraying other convicts. Such a betrayal could result, according to the code, in retribution, some of which was violent. Wieder wanted to see how the code was used and understood, despite its apparent generality, to mean specific things in a whole variety of contexts. His analysis showed that the code was not so much a set of rules, as it was an interpretative device that enabled individuals in halfway houses - be they convicts or staff - to recognize, understand, predict, explain and excuse conduct. Specifically:

The use of the code identified the meaning of a resident's act by placing it in the context of a pattern. An equivocal act then becomes 'clear' in the way that it obtains its sense as typical, repetitive and more or less uniform, i.e. its sense as an instance of the kind of action with which staff are already familiar. Staff experience was also structured by the flexibility of 'telling the code' which could render nearly any equivocal act sensible in such a way that it was experienced as something familiar, even though the act might not be 'expected' or 'predicted' in any precise meaning of the term.(1974:78).

It was also the case that all members of the halfway house were obliged to interpret events via the code because the code itself provided grounds for sanctions against those who did not 'use' it. In effect the code was a morally enforceable device. Using it was not to be dismissed or taken lightly. The code was a more pervasive matter than say, the concept of organization or the use of rules.

Wieder noted that reference to the code in ordinary talk within the halfway house simultaneously described and organized the setting insofar as reference to it was consequential. For reference to it instructed hearer's how to understand. Failure to adopt the code's version of reality could result in retribution. Thus Wieder was able to emphasise the significance of talk in social interaction, something that, as Garfinkel had also noted, was largely ignored in sociological analysis.

Wieder also suggested that insofar as the code was only a version or interpretation of reality, then it could not claim to be the only possible version of events. What distinguished it as a version was not so much its accuracy, although there was that, as the sanctions that supported its use. Wieder suggested that if this was the case, then it might also be true to say that interpretations of activities in whatever setting were based as much on moral grounds as empirical. He did not mean that reality could be interpreted anyway; only that given certain facts, a variety of interpretations was possible. Wieder's argument is to be

contrasted with that of Mehan & Wood, Pollner and others who I will talk about later on.

In addition to these studies, Bittner also examined the police (1967:(a); 1967:(b)). David Sudnow plea bargaining and the use of 'normal' categories of crime (1965) (this area has been of continuing interest to ethnomethodologists (see Maynard 1984). Sudnow also described the social organization of hospitals and in particular 'dying' (1967). A. Cicourel, in a series of articles and books, looked at the use of statistics in sociology (1964), and the production of juvenile crime statistics (1968, reprinted 1979). Harvey Sacks, in largely unpublished work, looked at how individuals manage to make sense in conversation (this formed the basis of conversation analysis. I shall not examine this derivative of ethnomethodology in this chapter⁶).

The ethnomethodological program has continued to be productive. There are now so many articles that it would be foolish to try to list them all - in any case I do not purport to be doing a literature review⁷. But one program of ethnomethodological research which has emerged recently is,

⁶ There are now several handbooks and edited collections on conversation analysis as there are on ethnomethodology. See for instance: Atkinson & Heritage, (eds) 1984; Button & Lee (eds) 1988; Heritage, 1985; Schenkein, (ed) 1978.

⁷ One major research project that has employed ethnomethodology in recent years is Hughes *et al*'s study of the work of Air traffic Controllers (1988(a);(b)).

I think, worthy of some discussion.

Studies of Work

With the publication of Garfinkel's edited collection, 'Studies of Work' (1986), a new program of ethnomethodological inquiry has emerged. This program is consistent with the original formulations (Heritage, 1987:261). Specifically the intention is to inquire into the accomplishment of orderliness, but that concern is taken to a deeper level than before. For these new studies of work try to focus and describe essential or basic features of the ways people accomplish work, features that are so fundamental that they are, so to speak, 'invisible'. Garfinkel, in unpublished work, uses an analogy to explain the intention of 'studies of work'. He observes that in a fairground a ghost train only succeeds if the mechanisms for producing the ghosts are not visible to those on the train. Otherwise the ghosts will be seen for what they are: mechanical objects. It is the accomplishment of those who make and maintain the ghost train to ensure that its inner mechanisms remain obscured and invisible to the view of riders. Garfinkel argues that it is a task of ethnomethodological studies of work to specify or establish the methods whereby the invisibility is achieved. This analogy is intended to hold true for a whole variety of settings.

Garfinkel goes on to suggest that the vast bulk of

sociological studies of work do not specify in any detail what the work (whatever it is) actually involves. Instead the details of work are ignored, perhaps glossed over but never addressed in their own right. It is as if the sociologists were interested only in say, the impact the ghosts on a train had, rather than on the mechanisms whereby the ghosts are produced⁹.

One of those who pursued Garfinkel's program was M. Lynch. Lynch examined the work of laboratory technicians (1982;1985;1985b). Amongst other things he noted that they were required to mix substances. To do so technicians had to locate and clean instruments, position them suitably and generally make everything 'necessary' for a mixing to be 'at hand'. Doing so involved skills, largely tactile and corporeal, which were mundane features of the skills of technicians insofar as they were treated as so basic that they would not be, for example, included in specifications of working methods in journal articles. Rather, these skills remain tacit, assumed and ignored features of the competence required in doing a technician's work.

These typically ignored skills or details of work are not so

⁹ For example H. Beynon's 'Working for Ford' (1974), which is often treated as seminal sociological examination of work, is a study not of how work is done on a production line, but a description of the politicking between shop stewards and line management. At no point are the skills required to work on a production line described. This would not matter but for the fact that the book purports to be an analysis of *work*.

easy to analyse however. For as has just been noted, workers themselves would not mention them in methodological glosses of their activities. It is as if the workers were 'uninterested' in them. Garfinkel argues therefore, that to access these details of work the ethnomethodologist must not criticize those methodological glosses which ignore them, but should observe work as it is done *in situ* so as to see work in all its diversity and details. Moreover, this is doubly necessary because, Garfinkel argues, part of the details of work consists of dealing with the specificities or unique details of the local environment. Thus a laboratory technician will know certain things about particular laboratory equipment which he will draw upon in doing his or her work. This knowledge is uniquely relevant to that setting and nowhere else. Garfinkel labels these locations specific skills 'unique adequacies'.

One problem about locating the unique adequacy of workers' skills however, is that they may not be visible to the analyst, however carefully he or she observes work activities. A laboratory technician may be doing something so subtle that an analyst may not be able to notice. These subtle, almost invisible skills, may be vitally necessary for the eventual success of the work. Garfinkel proposes two ways of dealing with this. The analyst can either learn the skills in question, and can thus verify what is happening, or the analyst can observe the way in which workers deal with practical problems. For, it is in dealing with the

practical problems that many of the usually tacit or invisible skills become, at least momentarily, a cause for concern and hence, visible to the analyst. In effect this latter technique is equivalent to the notorious breaching experiments (or demonstrations) that Garfinkel used to illustrate practical reasoning in the '60's (1967:24-31).

It is this policy that Lynch employs. Specifically, and for example, he examines how laboratory technicians deal with problems in the production of photographic slides. Some slides picture the kinds of lesions (for they are photographs of lesions) that are suitable for presentation in journals etc. whilst others are in one or another suitable for inclusion in statistical analyses but not good enough for presentation. Finally, some slides are completely unusable either for presentation or as a basis for statistical proofs. The technicians have to ensure that at least a certain amount of adequate photographs are produced. They do so through endless re-tries of the photographic procedure until the desired number are produced. Lynch examines and characterized the way they select good photographs from bad and hence describes some of the 'unique adequacies' of the technician's skills.

E.Livingston pursues Garfinkel's research program in a different field altogether, and indeed in one that has not been examined by sociologists before. He inquires into the social organization of mathematical reasoning, in particular

of mathematical proofs (1986;1987). But instead of employing the breaching method, Livingston attempts to become competent in the skills he is trying to analyse. Moreover, his analysis requires of the reader that he or she also becomes, in some senses, a competent mathematician insofar as the reader is required to employ considerable mathematical elan to follow the explanation.

Livingston attempts to show how a mathematical proof involves (a) a sequential demonstration, and that this depends upon (b) conventions about mathematical reasoning. The first of these points is at once seemingly obvious; for it is hardly surprising that a mathematical proof needs to be shown and that a 'correct showing' needs to be done in a certain order. But that this showing is physical, i.e., is a corporeal demonstration, suggests that a showing or a 'proving' is a social interactive activity. It is not, therefore, a purely tacit, mental activity. One of the conventions embedded in the social interactional activity of mathematics is, for instance, the tacit agreement about which side of a geometric diagram is to be considered, which one ignored.

M. Baccus opted to analyse a less esoteric though nonetheless skilled activity: how truck tyres are replaced (1986:14-49). She showed how the skills actually required to change a truck tyre, the 'situational competencies' as she calls them, are somewhat different from what could be construed from examination of the formal rules of tyre

changing as contained in safety regulations. This was not because formal rules always 'gloss' activities, but because, in this case at least, the rules were devised to protect the bureaucratic organizations truck tyre repair men worked for rather than to specify actual work techniques.

'Studies of work' can be seen to have quite considerable significance. For they specify, or at least attempt to, the details, both local and technical, which underscore the accomplishment of work. In so doing this approach describes some of the competencies or skills that social activity involves. Consequently it is an improvement over those studies of work which, as I have noted, disattend to examination of the details of work. In that this is the case, this strand of ethnomethodology can also claim to be a serious attempt at empirical sociology. And this is no bad thing given the preference for theorising in the discipline⁹.

Conceptual Confusions and Ethnomethodology

Some authors, I believe, have attempted to use

⁹ There are some faults in studies of work. For example, all the articles in Garfinkel's edited collection (1986), with the exception of the one by Baccus just discussed, are opaque to the point of meaninglessness. They also fail to specify any features of the activities in question that are 'recognizable' as tacit skills or even as skills at all. Even Baccus's work is flawed: the description of tyre changing is so confused as to make it impossible to conceptualize the situational competencies she is talking about. I can see no reason why the articles should have been written so badly. A comment is deserved even if only in the footnotes.

ethnomethodology in ways that can best be described as conceptually confused. In particular it seems to me they make mistakes about language categories and the proper use of such terms as *constructed* and *reality*. One can go straight to the heart of such confusions with a quote from Mehan & Wood's book, 'The Reality of Ethnomethodology' (1975):

In ethnomethodology, persons are treated as reality constructors. Rules are dependent upon the ceaseless ongoing activities of persons within social situations. The Ethnomethodological model is a characterization of the way persons create situations and rules and so at once create themselves and their social realities. (1975:98).

In other words, these authors suggest that one should not treat social reality as an accomplishment simply for sociological or analytical purposes, but should treat it as a fact. M. Pollner (1973;1974:35-54; 1988) takes a similar point of view. He claims that there can be a multitude of different realities. One simply chooses between them. He attempts to show how this is so by analysis of court cases and psychiatric interviews. He argues that 'mundane reality' is only one amongst many possible *realities*; and that one feature of adopting mundane reality is the belief that it is the only possible one. That is, having faith in this reality excludes faith in any other. Pollner suggests this exclusivity is a self-sustaining and justifying feature of any reality - mundane or otherwise. Carlos Castaneda made a name (and no doubt a fortune) for himself by adopting a similar argument in his studies of the teaching of Don Juan

(1970;1971;1974). In these publications - novels rather than studies - Castaneda attempted to show that the world changes as one's way of looking at it changes. More exactly, he argues that the world is subject to one's state of consciousness. This can be altered by the use of intoxicants. More recently, R.Hines, participating in a discourse about sociological conceptions of accountancy, employs Castaneda's work to advocate a similar idea: that accounts, for example, are constitutive of the world (1988:251-261).

Now clearly, each of these authors is not putting forward precisely the same case, but it seems to me that all of them are confused by the same terms. In particular they get conceptually confused when considering 'reality'. In that this is the case, then it would appear reasonable to contend that analysis of the conceptual confusion in the work of only one of these authors would be sufficient to illuminate the kinds of confusions manifest in them all. Since accountancy is the subject matter of later chapters in this thesis, it would be equally reasonable, therefore, to choose Hines' work as a basis for such an analysis.

Hines' article consists mainly of Castaneda's words, while her own discussion and commentary is limited to the footnotes. I think that Hines' conceptual confusion has two aspects. The first relates to misunderstanding what is going on in the Castaneda article. This is better treated as a

seperate matter from the question of the confusions common to all the authors. The second, which I think is common to all, derives from her failure to recognise what one can and cannot say. To do justice to Hines, I shall discuss both these aspects.

Hines takes selections from Castaneda's 'The Teachings of Don Juan' (1970) which relate to accounts. In these sections, the teacher of Castaneda, a shamanic priest, describes a landscape in a valley below them. A farm covers part of this valley, and fences and buildings sprawl out from the farm. The priest asks Castaneda if he believes the world can change if a different account of it is given. Castaneda replies that he does not believe so. But if, the priest then asks, the farm's accounts are audited and if these accounts show the farm to be bankrupt, what changes would occur? Castaneda replies with the suggestion that the farm might be forced, by creditors, to stop working. If that is the case, the priest asks, what would be the result? Castaneda replies that perhaps the fences and buildings would rot or be dismantled. If that is so the priest retorts, the landscape would have changed. Thus, the priest concludes, an account can change the world.

Hines uses this to illustrate the argument that accounts are about opinions or versions of *reality*, and that if they change or are different, then the world itself, i.e.,

reality, changes¹⁰. This claim is unjustified on two grounds. Firstly, the section from Don Juan does not support this thesis, and secondly the argument that reality can change does not *make sense*. To deal with the first of these I will have to look even more closely at what the shamanic priest purportedly said to Castaneda.

It will be recalled that the priest gave a lesson. He began with a question: do accounts change the world? A negative answer was given. Subsequently, in an illustration to this answer, the priest stopped talking about accounts, and talked instead about audits. He showed that audits can make a difference to the world in that, for example, a farm may be declared bankrupt, and this can sometimes have results in terms of, say, the condition of the former property of the farm. He then claims to have refuted the original answer that accounts cannot change the world - for he has shown that certain types of account can change the world. However, the Priest's argument consists of a trick: for he changed what he was talking about halfway through. At the start he talked and asked a question about accounts *in general*, but he commented on the answer given in terms of audits *in particular*. An audit is, of course, a form of account. But this does not mean that audits are representative of accounts or that audits and accounts are interchangeable terms. The words represent different

¹⁰In some respects this is a 'hard version' of Kuhn's paradigm thesis.

categories; or rather an audit is a category within the category of accounts¹¹.

But this begs the question of why the priest changed what he was talking about halfway through. It seems not unreasonable to suggest that he was trying to emphasise certain things: that for example, individuals have considerable importance in terms of how the social world is organised. An auditor, say, can have enormous impact on the running of a farm. In emphasising these matters the priest was encouraging Castaneda to be more conscious of the things that his social actions can change; to reflect more on the relationship between seemingly insignificant activities like auditing and the conspicuous importance of a farm on a landscape. He was, in other words, engaging in didactic talk. That he was doing so may mean, and this can be readily understood, that he exaggerated his case, or even *cheated* with language. And this seems where Hines goes wrong. For she ignores the fact that the priest is teaching, and thinks that he is making some claims about reality itself.

Thus I think that the sections Hines takes from Castaneda do not support the notion that reality can be constructed. For

¹¹Being able to cheat in language like this was not discovered by Castaneda's priest. It has been known of and used for centuries. The philosopher, G. Ryle, in his book *Dilemmas*, deals with very much the same kind of cheating. His analysis is basically an inspection of meaning, of what is properly meant and what is not and in this manner he solves problems from antiquity like Zeno's Paradox.

all they illustrate are the conceits of a teacher¹².

The other aspect of her confusion is, I would argue, her belief that one can talk about reality changing and yet still make sense. It may be that this belief led her to misunderstand what the priest was about; although it is impossible to tell which came first, the belief or the misreading (but I do not think we need concern ourselves with that). This confusion, I think, derives from conceptual confusions about the term *reality*. For Hines seems to use the word as if it represented, say, a theory about 'life', 'existence' or what you will. Thus, for example, one can have 'reality' or 'x' reality or 'y' reality. And these examples seem to underline the kind of confusion I am talking about. For that I have to say 'x' *reality*, or 'y' *reality* reminds one that there are no other words that could be used to represent or signify another reality. For one cannot talk about reality as if it were something that could change, be constructed, be a matter of versions, or even, more simply, just a matter of words. Now, one of the things I have argued in the previous chapter was that confusions often occur because language terms are used in the wrong

¹²In part, Hines' understanding of the priest's intentions may be coloured by her keenness to counter the narrowness and 'empiricism' in the vast bulk of academic literature on accountancy. In this literature the possibility that accounting may be a 'version' of events is completely obscured and one would get the impression that accountancy is simply a matter of mathematics. There may be, therefore, some polemical excess in Hines' argument. But if her intentions are basically polemical, if, like the priest, she is trying to make people think more carefully, then she should say so.

contexts or in the wrong language games. Here, it seems to me, we have the mixing up of several language games. In this case, this includes those relating to construction and versions as well as reality. I shall keep my discussions to reality however since this will suffice for my purposes.

One way I said that one could sort out these mistakes is by looking at how a term is ordinarily used. In this case how the term 'reality' is used. Before I do so however, I think it may be appropriate to recall some of the things I said about *forms of life*. I suggested that a form of life is a term for those things that are so basic that it hardly makes sense to describe them. The term reality is similar. For we cannot describe reality; we do not need to, it is simply there. Now let us think about how we ordinarily use the term. If one were to say about someone that they were no longer in reality, we are not saying they are somewhere else or have opted for another reality. For they are still in the same place, perhaps even, right there beside us. What we are saying is that there is something wrong with them which makes it difficult, even impossible, to understand them. We have many words to help us here such as 'mad', 'mentally ill', 'deranged', 'crazy' 'psychotic' and so on. It is not simply that we do not believe that for example, other persons do not have the right to 'choose' another reality. *There is no other reality*. It would make *no sense* to say so. Another way one can attack this issue is by suggesting that

Hines confuses the language games about such matters as say, the *colour* of something with the language games involving the term reality. For it makes sense to say that one can dispute about what colour is appropriate in certain situations. Eventually, of course, there has to be some agreement or else one might say that someone is colour blind (exceptions to this are at those points where the colour in question is close to the boundaries between two others. It would lead me a way from the main issues here if I dwelt on this at any length however). Likewise, Hines uses reality as if it were a term that could be changed with others. Another possibility is that games about the number or amount of things are being mixed up with the reality language game. So, Hines may be talking about reality as if, if you will, there are several of them from one say to half a dozen. Clearly this is wrong. Reality is not something that can be counted, or even measured; it cannot even be described.

One other possibility that needs to be mentioned is that Hines may be suffering from an excess of doubt. It will be remembered that I suggested that Gellner's difficulty with 'forms of life' was partly because he could not accept that something need not be defined. Underscoring and related to this, I suggested, was a preference for doubting all things. This, it seems to me, is the same kind of problem Hines has. For in suggesting that reality is, say, a construction or a product or accounts, or whatever, she is implying that one can doubt reality or that one needs to

define which reality one is taking about. I hope it is clear, now, that it makes no sense to doubt reality. For, if one did then one would have to begin doubting one's sanity. And this is, of course, not a happy path to go down.

Ethnomethodology and other Sociological Approaches

Another kind of conceptual confusion that struck me as worthy of some deliberation relates to efforts to integrate ethnomethodology with other approaches. Many authors have attempted this¹³. One is Maynard. Maynard was part of a new generation of ethnomethodologists who appeared in the late 70's (see 'the Social Organisation of Plea Bargaining' (1984) also Maynard & Wilson, (1980)). Maynard made an attempt to relocate ethnomethodological inquiries within the body of what ethnomethodologists had called 'orthodox' sociology (Maynard:1984: 207-23). Specifically he tried to show how it was that in the reflexive articulation of their activities, children - for they were his subjects - came to produce not just the local orderliness that was traditionally the topic of ethnomethodological inquiry, but also features of social organisation that could be thought

¹³One of these is A.Cicourel. In particular, he wants to integrate it with macro-sociology. For Cicourel believes ethnomethodology is a micro-sociology (1981:51-80). However Cicourel has only advocated such an integration theoretically. It seems to me therefore, that it would be somewhat difficult to prize open any conceptual confusions that might occur if such an attempt was made. I will therefore concentrate my discussion on empirical attempts such as Maynard's.

of as 'functional'. By functional he meant those aspects of social organisation that had concerned theorists like Talcott Parsons and R.K.Merton. In displaying these functionalities, Maynard hoped to justify the notion that ethnomethodological inquiries could be integrated with functional ones.

Maynard suggested that virtually the only difficulty was that functional analysis needed some revision, especially in light of Giddens' (1979) critique. Maynard tried to build his claims around the use of contemporary sociology. In particular Merton's distinction between *latent* and *manifest* functions.

Maynard tried to show that the interaction of children could be seen to display *manifest* and *latent* functions. He argued that this could be seen to be the case by using an ethnomethodological approach. That is, he tried to show that ethnomethodology could be seen to provide data for functional (and hence orthodox sociological) theorising.

The way that Maynard formulated the proposal would seem to have a degree of plausibility. But if one looks carefully one will see that Maynard's version of latent and manifest functions is not the same as Merton's original formulation. What Maynard attempts to do is treat as the same two different uses of the terms latent and functional.

Merton had defined latent and manifest functions on the basis of a discussion of Hopi Indian Rain Dances (Merton:

1957). The manifest function of these rain dances was, Merton suggested, to bring rain. But there is no scientific evidence to support the idea that such rain dances do indeed cause rain. Merton argued that the rain dances can be explained scientifically in terms of their latent function: that they bring the tribe together and so lead to social cohesion and group solidarity (in much the same way Durkheim suggests religious rituals have a function). It is important to note however, that participants in the rain dance would probably not agree with the latent function explanation since for them the dance could only be a rain dance. But above all, the difference between latent and manifest relates to a difference between a scientific explanation and a layman's *unscientific* explanation. Latent equated to scientific, manifest to unscientific. This may be said to equate to an epistemological distinction.

In his analysis, Maynard suggests that conflict between children serves the immediate or *manifest* function of joining groups of children into alliances or groups. These alliances relate to specific matters like who is allowed to finish first, who writes properly and so on. But, Maynard goes on, in doing so a latent function is also served: in learning about alliances children are also learning about the nature of politics in adult life, or rather they are practising for adult political life. This is because they are learning, for example, how to make or dismantle alliances. And learning to do so is essentially a political

matter.

These two versions of manifest and latent functions are not the same. According to Merton, manifest function is contrasted with latent in that the latter is discovered by scientific inquiry, whereas the former is simply or 'merely' the layman's point of view. There is a distinction between the epistemological status of each level of function: the one scientific, the other common-sensical. In contrast, according to Maynard, there is no similar distinction between the two types. Rather there is a difference in the level of generality. Manifest functions relate to the particulars of the local situation and are 'recognised' by participants. Thus the children would recognise that they are forming alliances; that is the manifest function of their activities. Latent functions relate to the general order of society as a whole. So alliance making in the local situation, that is to say in the *micro* context can be said to reflect or are instances of the general fact that alliances form at a political level in society as a whole, i.e., at the *macro* level. (They are also a feature of adult life). This difference of generality is not the same as a difference in epistemological status.

The use of the two terms by Merton and Maynard, is then quite distinct: one might say that they are part of different language games. In that this is so, each has its meaning determined by that game and each is to be made

sense of and evaluated in terms of how it fits in to that language game. Consequently it would be incorrect to say that the two different ways of using the terms latent and manifest are the same: they are not. So, Maynard's claim that he is integrating ethnomethodological inquiries with functional ones does not hold true to the extent that the terms around which this conjunction is supposed to occur are not identical.

This then is one empirical attempt at integration: it seems to me that it is flawed to the extent that it involves a mistake about terms. I think it necessary to add however, that the lesson to be drawn from this is not that terms like manifest and latent cannot be used in ethnomethodological inquiries. Rather, if they are, the analyst must be careful not to *confuse* them with the same words in functional theorising. For I have shown that although they may be the same words, their meanings are distinct¹⁴.

Heritage & Greatbach

One other kind of confusion that I want to discuss in this chapter relates to the use of methodologies taken from other

¹⁴Merton's work seems to be peculiarly susceptible to these kinds of adulteration of meaning. For example his notion of 'self-fulfilling prophecy' is often taken to mean that if someone believes something will come true then it will. In fact, Merton meant by this term specifically the idea that some people fail because they expect to fail. There is a self fulfilling prophecy in expectations. But he points out that this is only meant to explain or be an account of failure, not success. For success is not guaranteed if people think they are going to succeed.

approaches to evaluate ethnomethodological inquiries. An example of this is Heritage & Greatbatch (1986: 110-157). They try to statistically assess an ethnomethodological examination of public speeches by Atkinson (1984). Now my concern here is not to argue that statistical analyses are conceptually confused. I have no doubt that some are, just as I have no doubt some ethnomethodological inquiries are. Rather, I want to suggest that, in this case, the use of statistical evaluation techniques confuses the issues Atkinson wants to draw attention to.

The Atkinson study attempted to show what were the visible, methodic practices whereby speech makers and speech hearers were able to generate applause that occurred simultaneously and at 'appropriate' times. This was a potentially interesting inquiry, for, like turn taking in conversation which does not always involve verbal specification of who speaks next (Sacks *et al*, 1974:696-735), so too is there no obvious direction given by the speech maker as to when applause should occur. But in practice it does occur and occurs simultaneously. Atkinson argued that invitations to applaud succeed dependent upon the use of a variety of rhetorical devices, the content of the talk, and timing. That any applause occurred at any particular moment could be explained by reference to the artful, self-cognisant ways speech makers were able to utilise these resources and the artful ways these actions were interpreted by speech hearers. Atkinson attempted to characterise this

artfulness. Leaving aside how successful this characterisation was let me consider what Heritage & Greatbatch attempted to do in their analysis of Atkinson's work.

Heritage & Greatbatch's intention was to see which particular rhetorical devices could be seen to be ones that, on the basis of statistical inference, led to applause and those which did not. Thus their concern focuses on how behaviour can be explained by reference to the determination of events or behaviours. One determinant relevant here is the rhetorical construction of a speech. They are not interested, however, in such things as the skills involved in knowing and recognising when there is a moment for applause. These skills are disregarded. That is, they place minimal attention on the *skills* of audience members, and a premium on those events that can be treated as causal of the action of audience members. The result of this is that Heritage & Greatbatch's study treats actors in the world, specifically speech hearers at British political party conferences, as *judgemental dopes*.

As it happens, Garfinkel had been critical of such a procedure when he first developed ethnomethodology twenty years ago. For one thing, Garfinkel suggested that the ignored skills of actors *should* be a topic of sociological inquiry. This also brings to mind Winch and Lounch's injunction to study actors understandings. But I do not want

to criticise the Heritage & Greatbach study on these grounds. I think this is a separate matter. Instead, I want to point out that the emphasis in their study is different from that in Atkinson, if not specifically contrasting. For the skills they ignore are the very skills that are the topic of the Atkinson study. Now this begs the question of what Heritage & Greatbach were intending. Because clearly, what they were about was very different from Atkinson. It thus seems odd that they claim to be testing Atkinson's observations. It may be, and I think is all I can say here, that they failed to take account of the significant distinctions between the kinds of inquiry they were involved with and what Atkinson is involved with. This lead them to the rather glib assumption that there were adequate - or enough - commonalities to justify their claims.

Conclusion

One of the observations I made in chapter two was that conceptual confusions are endemic. Wittgenstein's philosophy offers remedy's for many of these confusions, but also serves as a reminder: there will always be more. Not only that, but confusions most often arise in specific occasions and as a result of particular exercises. This may well mean that the kinds of confusions I have just discussed are one-offs. It might be unwise therefore, to extrapolate from these examples to the idea that ethnomethodologists endemically confuse terms like reality or latent and manifest functions. Nor is it the case that confusions will

necessarilly arise when attempts are made to consider ethnomethodological studies from different perspectives. These observations aside however, it seems reasonable to contend that one implication that can be drawn from these discussions of Maynard, and Heritage & Greatbach, is that there may be less confusion, or rather less confusion of a certian type, if sociologists did not spend so much time endeavouring to integrate or link ethnomethodology with other approaches. Moreover it may well be that this will hold true in relation to the linking of other approaches to one another. A side benefit of this may be that sociologists will be more able to recognise the specific meanings of terms within the varieties of sociological approaches.

There is not time however, to justify these arguments at this point. But, it will be recalled that one of my intentions in this chapter was to explain what 'accountability' meant. This term is central to ethnomethodological inquiries. Failure to understand its specific meaning can result in someone not understanding ethnomethodology. There are, of course, many other arcane terms, some of which I have covered, which are also important. But something else I have emphasised, especially in relation to *studies of work*, is that the ethnomethodological approach builds much of the justification for its program by pin-pointing those areas not covered by other sociological approaches. At times this

has meant that ethnomethodological inquiries are critical of some of the claims made by those other approaches. But more often it has meant that it has extended the scope of sociological inquiry. At the beginning of this chapter I suggested that it would be wrong to think of ethnomethodology as *Wittgensteinian* sociology. I argued instead that it is to be understood in terms of specific interests. These are distinctly sociological, not philosophical. It has been my purpose in this chapter to outline why this is so and what it involves.

CHAPTER FOUR

THEORY, CRITICISMS, THEORETICAL CLOSURE: CONCEPTUAL CONFUSIONS AND A SOCIOLOGY OF ACCOUNTANCY

Making a compromise between theoretical and empirical analysis necessarily involves making choices: how much attention should be given to this, how much to that. One consequence of this may be that only the bare minimum is given to any one topic and many others are left untouched. The decision about what criteria to use is therefore crucial, and should not be taken lightly. In the previous chapter I discussed ethnomethodology for several reasons. One was that I will be using the approach in subsequent chapters; another was that some of the problems I noted in the ethnomethodological literature seemed to me illustrative of those I wanted to draw attention to in sociology as a whole. One of these problems was that there is an over keenness to try integrating or linking different sociological approaches, a keenness which sometimes results in the specific meanings of terms within approaches becoming confused. I put forward the possibility - although I did not discuss it at any length - that many of these confusions could be avoided if sociologists did not spend so much time attempting to link or integrate approaches.

It may be that one reason why sociologists are so keen to integrate approaches is that they are confused by some of their own theoretical discussions. Some of these discussions

are based on confusions about meaning. This leads sociologists to make assertions or theoretical claims that are unjustified. In other cases, these theoretical discussions under-emphasise the stipulative aspects integral to any approach, aspects which are central to what is distinct about each. A failure to recognise these stipulations may lead to a commentator misunderstanding what the purpose of an approach is. It may also lead to gratuitous criticisms or attempts to integrate approaches on false grounds.

Before saying any more about this I think it necessary to specify what I mean by *stipulations*. It is not contentious to claim that each and every approach incorporates certain assumptions about what is and is not relevant. These assumptions provide the framework within which an analyst works. They are, if you like, *methodological*. Another word that would be suitable here is *hueristic*. Stipulations are not made in a totally arbitrary manner: a whole gamut of criteria may be used to select them. One might be chosen because it defines what is the topic of an inquiry, another because it defines what is excluded. I have discussed how one stipulation of ethnomethodology is that inquiries should focus on how individuals make their activities accountable. Another I mentioned, albeit tangentially, was the stipulated distinction between scientific and common-sense theorising in Mertonian functionalism. In other words, Merton stipulated that layperson's accounts are to be contrasted

with accounts generated scientifically.

In this chapter I will concern myself not with one approach in particular, but with the kinds of problems that sociologists get themselves into and which result, amongst other things, with their failure to properly understand what different approaches to sociology actually involve. It seems to me that making unjustified theoretical claims on the one hand, and under-emphasising, confusing, or even ignoring the stipulative aspects on the other, often results in the selection of approaches on spurious grounds. It can also result, and I think this is a particularly serious error, with one approach coming to monopolise thinking.

I think there are so many instances of conceptual confusion and misleading theoretical debate that it would be foolish to attempt to deal with them all. Instead I shall limit myself to three cases, each of which relate in one way or another to my concerns. I shall begin with discussion of what I will show are unjustified, even gratuitous, theories about ethnography. I will argue that they are unjustified because they derive from a confusion about two uses of the concept 'culture'. The theories will be seen to be unnecessary when this confusion, which lies at their root, is unravelled. It is necessary to discuss these theories because it will help clear up any confusion about what my own ethnographic enquiries entailed. I would also like to discuss failures to recognise the import of the stipulative

character of some approaches. One consequence of this failure is that assessments and ultimately criticisms, are sometimes made on irrelevant grounds. One such criticism is that an approach does not take into account matters that it had stipulated were irrelevant. Another problem I will look at is when the distinctiveness of an approach and the problematics of integrating them is under-emphasised. This often occurs when triangulation is advocated. Lastly, I will examine some instances where the stipulative aspects of approaches are confused with, and treated as, descriptions. This last type of mistake, apart from being wrong, has the unfortunate consequence of leading many to believe that it is right to advocate that approach which they think most accurately describes society. This results, as I say, with one approach coming to monopolise sociological thinking. An instance of this can be found in the sociology of the professions. There, the so called 'power paradigm' is used almost to the total exclusion of other approaches. This monopoly, I think, not only derives its justification from conceptual confusion, but is antithetical to a truly pluralistic sociology.

The purpose of these discussions, however, is not to list or catalogue these types of confusions, but to provide materials for me to discuss how one should choose sociological approaches. My arguments will not be about which approach should be chosen, but about which criteria are relevant in such decision making. In particular I will

argue that one needs to emphasise the distinctiveness of approaches, a distinctiveness that often, although not always, resides in the stipulations. I will reassert my claim that one way of ensuring that this emphasis is correctly placed is by conceiving of approaches as different language games.

Ethnography

I have said that I embarked on my research without any particular idea what I would find. This seemingly casual attitude, is, I think, more common than many would care to believe. The rhetoric of publications based on ethnographic, and indeed many other types of research methodology, often gives the impression that the researcher knew what he was looking for from the very beginning. In practice, it is likely that many researchers only discovered what they were interested in once they started. One reason why publications seem to disguise this is perhaps, that ethnographers, (and sociologists more generally) would like to emulate a certain type of scientific practice. Whether that type actually exists or not is debatable, but in any case it is of no concern to me here. What I am interested in are some of the recent discussions about the so-called problematic of ethnography. For some have argued that it is need of a theory. The problem, as they perceive it, is basically this. If the ethnographer and the natives studied live in and come from different cultures, then how can the ethnographer be sure that he or she has understood the culture, the

attitudes and world view of those studied, i.e., the natives? To solve this apparent difficulty, several authors have advocated one theory or another about how to do ethnography.

Now of course there can be many different types of theory and many of these will be beneficial to the ethnographer. But the particular theories I want to discuss here are, I think, based upon misconceptions about the use, and hence the meaning of the the concept of 'culture'. To help us get a handle on the issues it may be appropriate here to bring to mind the topics of some of the most famous ethnographies. One can recall for example, E.E.Evans-Pritchard's classic study of the Dinka. I do not think it would not be unreasonable to suggest that in most ethnographies, including Evans-Pritchard's, the concept 'culture' will be used as part of the language game of description. So, for example, the concept would be used as part of describing what (say) Dinka do and believe, their political and religious institutions and so on. One may loosely describe this use of the term culture as part of the way an ethnographer might talk *about* Dinka. (It will be clear of course, that I am not interested in whether the description put forward by Pritchard or anyone else is accurate). In contrast, one will not find many examples of another way of using the concept culture in Pritchard's work. This is because this other way is used by those who *are* Dinka. For example a Dinka might say, 'Only a Dinka can understand this'. Comments to this

effect may be made in reference to such things as the Gods of the Dinka, or the relationship between the Dinka and their cattle (these are only hypothetical examples). In short, then, one can think of at least two ways that the concept culture is used. However, I think one has to be very careful about how we understand these uses.

For example, one possible reading of the phrase 'only Dinka can understand this' is that it is about epistemology. Thus if only a Dinka can understand, then an ethnographer who is not a Dinka, will not be able to understand the (cultural) phenomena in question. This, it appears to me, is the reading of several commentators. For, since one of the avowed purposes of much ethnography is to describe (a) culture, these commentators propose various theories to get around the *problem of understanding*. One is that the ethnographer needs to immerse himself in the culture, and *become* a native, e.g., become a Dinka. Another is that the ethnographer needs to research in *reflexive* ways (Jules-Rosette, 1978:81-98; For a Wittgensteinian critique see Sharrock & Anderson, 1982:119-125). But this, I would have thought, is a wrong understanding of what is meant here. Consider, would it make sense to say that to understand some particular aspect of a culture one would have to have been born in a certain place or be a certain colour, even a certain sex? Or would it, perhaps, be more appropriate to think the term 'only a Dinka can understand' to mean only a Dinka is *entitled* to understand. This latter reading is, I

believe, the correct one. There are several reasons for this.

For one thing reading or hearing the phrase this way might help explain why it is that the subjects of ethnographic inquiry so often say 'only we can understand'. For they may resent the arrogance with which an ethnographer makes judgements about what they do. But in addition, and perhaps this is a more significant justification for this reading, it draws attention to the possibility that some aspects of culture turn around notions of membership or entitlement. One may recall my definitions of stipulations here. So for example, it may be a stipulation of Dinka culture that only a Dinka can understand Dinka Gods. It is, if you like, a *rule* of Dinka culture (and of course is therefore a very different thing from the methodological stipulations I have been talking about elsewhere). But it is not a rule that makes a difference to what can be described, only what an individual can claim. If this reading is accepted then, one upshot will be that one does not need a theory about how to get inside a culture.

Another upshot is that the ethnographer need not limit himself to describing a culture. He may also analyse *how* Dinka for instance, go about instructing each other and outsiders about their culture and their entitlements within it. It may be that if he does so, the ethnographer will discover that reference to the distinctiveness of a tribe or

a culture is *ad hoc* and piecemeal. Consequently it may be that a culture is not to be grasped 'all at once' (Sharrock & Anderson, 1982:131). It has to be pieced together. The ability to piece it together then, the *how* of this process can become a topic for ethnographic research.

So those theories of ethnography which purport to solve a problem are unjustified. This is because they are founded on the belief that there might be an epistemological difficulty in describing and understanding culture. Once it is seen that this difficulty is really 'only a misunderstanding of certain ways of talking about culture, then it will be realised that no theory is necessary, because there is no problem. Unjustified theorising seems to me to be part of a set of conceptual problems afflicting sociology. Another aspect of this set relates to confusions about stipulations. This leads to criticisms, and more generally to theoretical discussions, that are equally unjustified and gratuitous. It is to these I shall now turn.

Misunderstanding Stipulations

I would like to argue that there at least three types of conceptual confusions about stipulations. There are many instances of each type. Here I do not so much want to get involved in any depth with particular instance as outline the three types of confusion. One of these types relates to criticisms of an approach which, I suggest, miss the point. There are many cases of this. Perhaps one of the most

obvious examples to my mind, is A.Giddens' critique of Goffman's notion of 'Interaction Order' (1988: 250-80). Goffman coins the term 'The Interaction Order' to define the parameters of his inquiries. In particular Goffman wants to demarcate face to face interaction as a legitimate locus of social scientific analysis (Goffman,1983:1-17). In short, he makes a methodological stipulation. It is not necessary to consider why he does so here (I shall have more to say about Goffman in chapter ix). Giddens criticises the concept on the grounds that such a realm (i.e., the interaction order) does not correlate to reality. Specifically, Giddens claims that the separation of face to face interaction is ill conceived because all face to face behaviour is premised upon, and is related to, other behaviours that occur elsewhere in time and space. As he puts it, "the interaction order is never separate from either the ordering of behaviour across contexts of co-presence, or the ordering of such contexts themselves in relation to one another" (1988:276).

Now these criticisms, it seems to me, are beside the point. It does not matter whether face to face interaction is affected by and related to behaviour elsewhere in time and space. For it is a methodological stipulation that face to face interaction be treated *as if it were* separate from these matters. One may draw this out by considering what in contrast criticisms that were relevant would look like. I suggest that they would take the following form: that the

concept demarcates an area that is not sufficient unto itself for the purposes of analysis. If this were the case, an analyst may have to refer to matters that the concept had defined as being external to its concerns. This kind of criticism would be *methodological*. It might appear that this is what Giddens is hinting at. That is, that methodologically, the interaction order is an unsustainable concept. But this is not, I think, what he does. For he makes no reference to methods, stipulations or hueristics; provides no illustrations of where Goffman has made reference to matters that the concept of interaction order had ruled out of bounds in analysis. All he offers is the observation that the real world is not like this. In other words, that there is no such thing as an interaction order. But clearly, this is an irrelevant remark, because Goffman never claims that that this is how the world is; only that the concept defines an area that can be analysed fruitfully¹.

The lesson I want to draw out here is that Giddens illustrates a tendency in theoretical debates to under-emphasise, and even to completely fail to recognise, that different approaches are often based on methodological

¹As it happens, I think it is difficult to criticise Goffman for not bringing enough into his analyses. For he mentions matters of such diversity that it becomes extremely difficult to know what are his methodological parameters. It may be, therefore, that if Giddens is guilty of anything, it is for being so naive as to take Goffman at his *methodological* word.

stipulations. If these stipulations were given the correct amount of emphasis, then I think the kinds of remarks that are made in theoretical debates would be more relevant. For instance, an approach would be criticised or judged by how effective it was as a *tool* of analysis; its limits would be discussed and abided by; improper use of it criticised. Instead, and at the moment, we have remarks that are as pertinent and which make as much sense as saying that a tea pot should be criticised for not boiling the water.

Another type of mistake I would like to mention underscores the notion that one can triangulate different approaches. This notion reflects a failure to recognise the distinctiveness and ignores the problematic of integration. Sometimes, however, and superficially it might appear that advocates of triangulation are fully aware of the stipulative differences between approaches. Consider for example Tomkins & Groves paper 'The Everyday Accountant and Researching His Reality' (1983:361-374). In this article, these authors argue that the bulk of studies of accountancy take (what they call) a 'scientific' approach. They claim that these need to be complemented by more 'naturalistic' studies (1983: 362). These latter would place a greater emphasis on the 'understandings' of accountants in real world contexts². Underscoring the choice

²Articles that took up the ideas put forward by Tomkins & Groves include: Abdel-Khalik & Ajinkya, 1983: 375-384; Morgan, 1983:385-388; Willmott,1983:389-405; see also further comments by Tomkins & Groves, 1983: 407-415.

of naturalistic or scientific approaches, are different *ontologies*. (These ontologies seem to be equivalent to what I have labelled stipulations). They suggest that there are six possible ontologies: reality is a concrete structure; a concrete process; a contextual field of information; a symbolic discourse; a social construction; or a projection of human imagination (1983: 367)³. But despite their discussion of these different ontologies, they seem not to realise that the use of any one approach may be inimical to the use of another, and that the results of using one may be of an entireley different character to those generated by using another. Consequently they argue that analysts should use all the approaches and should triangulate the results. The end product would be a more accurate understanding of accountancy.

To be frank, I am not sure why Tomkins & Groves ignore the differences that I think are so important. One possibility that would explain it is that they are influenced by the apparent unity of descriptions in the hard sciences. This may lead them to believe that sociology should have an equally unified model of the social world, accountants and

³ Aother example can be found in Burrell & Morgan's 'Organisational Analysis and Sociological Paradigms' (1979). I mention this work because Tomkins & Groves take much of their inspiration from it.

all⁴. Whether this is true or not need not concern me, but that a single description might be the ultimate goal of sociology leads me on to the last type of confusion I would like to discuss. It is the mistake of thinking that methodological stipulations are *descriptions*. This mistake has a particularly unfortunate consequence: it leads not just to unjustified or unnecessary theorising, but to the belief that one can choose an approach on the grounds that it offers the best description of society. This in turn, leads to one approach gaining ascendancy over all others.

A case in point can be found in the Sociology of the professions. Hall (1983:5-23) notes: 'The nature of the professional model together with the more basic question of the nature of the professions appears to a dead issue in the sociology of work and occupations' (1983:1). This is because of theoretical closure. A 'paradigm' has emerged which is believed to sum up all the issues (Ritzer, 1987;1977). This paradigm centres on one notion: 'The key to understanding the nature of the professions is... possession of power' (Hall,1983:12). Moreover, 'In the minds of experts in the field, the power approach to professions is now in power' (1983:12). Alternative models or paradigms

⁴A paper on the question of the methodology of the sociology of accountancy which might be of relevance here is by the philosopher, C. Lyas (1984:99-110). For a background to this see: Sterling, 1979; Stamp, 1981.

do not get any opportunity for publication⁵.

To be fair, and before I make much of this, this closure is not entirely complete. Macdonald & Ritzer (1988:251-272) note that there has not so much been a decline in interest in the sociology of professions as there has been a difference between British and American studies. In Britain for instance, the sociology of the professions is a growth area (1988:252). Moreover, there are differences in the topic of US and UK studies. In the US there has been a tendency to focus on particular professions, and to attempt to specify the common characteristics of professions; whereas in Britain the focus has been on the relation between the polity and professions⁶. But this difference notwithstanding, Macdonald and Ritzer agree that power is central to understanding the professions (1988:254). They add however that the power paradigm,

⁵This power paradigm is also popular in the sociology of accountancy. Some authors have employed this approach to show how accountancy is the language of capitalism (Roberts & Scapens, 1983); how it is biased towards the needs of managers as against workers (Bougen & Ogden, 1985); it is ideological and reflects assumptions about the class order (Laughlin, 1985; Loft, 1985). Many of these studies justify their use of the power paradigm by claiming that they will bring about a better society (Hopper & Powell, 1982; Tomlinson, 1985). One of the most famous examples of the approach is Hopper *et al's* study of accounting procedures in the National Coal Board (1985).

⁶Macdonald's work on accountants (1984:175-190; see also Briston & Kedslie, 1985:123-130) is an example of this. He examines the relationship between the Institute of Chartered Accountancy and the State; and shows how accountants in Britain have not been united in choosing strategies for their conflict with the state.

certainly as it is used in the US, has failed to take account of the continuing importance of the professions in society.

I would like to argue that the ascendancy of this paradigm or approach reflects a failure to realise that other approaches offer distinct and worthy benefits. Not insofar as they improve the description or account of professions; but insofar as they extend the ways in which one might conceive of the professions sociologically. I am not saying that there is anything wrong with the particular approach that has ascendancy at the moment, only with the fact that any one approach gets into that position. There are, it seems to me, many other approaches that offer us fertile possibilities. The ascendancy of one makes commentators forget this.

For example none of these authors I have just mentioned make any reference to or countenance the possibility that professions can be studied in ways other than those stipulated by the power paradigm. In effect, and through a form of theoretical negligence, they leave no room for divergent sociological approaches. One might for example, want to study professional behaviour in terms of Goffman's Interaction Order. This would involve limiting the analysis to face to face behaviour. It will not therefore incorporate reference to such things as the state. Another approach might be to see how common sense knowledge is used as the basis for professional work. As I have made clear in

chapter three, such an approach would be part of the ethnomethodological 'studies of work' program.

Conclusion

What seems to be crucial here is that commentators will not realise that other approaches are justified and indeed productive ways of looking at, say, the professions, until they clear up some of the conceptual confusions I have discussed. For one thing, and this relates to my consideration of some theories of ethnography, commentators must not allow themselves to be distracted by promiscuous theorising. If nothing else, and apart from the fact that the theories in question are often wrong, such promiscuity sets a bad example. For it serves to commend ambivalence about the precise use and understanding of language. This ambivalence is reflected throughout sociological literature. It becomes a particular cause for concern when, for example, criticisms of an approach are based on misunderstandings. Giddens' conflagurations about the Interaction Order are a case in point. It is also a concern when it leads to the confusing of methodological stipulations with descriptions.

One of Wittgenstein's key observations was that many of philosophy's problems where, on close inspection, little more than the product of the confused use of language. Close reflection of these usages would make these confusions

unravel. Wittgenstein suggested that one way of locating the meaning of language terms and thus discovering the source of confusion was by looking at the language games in which particular terms are used. He noted that on occasion the same terms are used in different games to mean slightly different things. It would be a mistake therefore to confuse the meaning of a term in one game with its meaning in another. If what I have said about ambivalence and imprecision is true, then sociology would also benefit from close reflection. For then it would see that much of what it does and attempts to do derives from the confused use of language.

This returns me once again to the notion of language game. I have already suggested that the language game analogy would be useful in sociology. What I have argued here further substantiates this claim. For if I am correct about ambivalence to precise meaning, then if different approaches are treated as different games commentators may become more wary about assuming that the meaning of the same terms used in more than one approach is the same in each. There is not only the matter of the precise use of language; another thing that Wittgenstein drew attention to with his language game idea was that language is always part of an activity. And this I think, is also important. For if commentators treated different approaches as language games then they might also begin to recognise that employing different approaches might involve doing different things. For

example, it can mean focusing on certain topics in specific ways and in reference to particular matters. This recognition has interrelated consequences. It will mean that evaluation would not be on the basis of say, comparison with other approaches, since other approaches will involve doing different things. They are therefore, largely irrelevant. Instead, it would be on the basis of how other analyst's have used the approach and what they have done with it. In other words, evaluation would be on the basis of matters *internal* to any particular game.

If this last suggestion is accepted, then a considerable amount of sociology would be made redundant. For much of it involves evaluating one or other approach from the standpoint of one approach in particular. This I have argued, almost inevitably leads to, and derives from, conceptual confusion. Sociologists are too often unconcerned with and do not recognise these confusions because they are ambivalent to precision.

It has been my intention in this chapter to discuss the kinds of confusions that distract sociologist from being able to fully comprehend what is involved when one selects one approach over another. I have not provided grounds for why one approach should be chosen over another, only specified how sociologists are led astray from the important issues. One of these issues is the need for the precise use and understanding of language. Without it one may be led into debates that are entirely unjustified. In

showing the importance of precision, I hope also to have prepared the ground for the kinds of justifications I shall make for the selection of different approaches in my ethnographic chapters. These justifications will relate, for instance, to how other analyst's have used the approach in question and will not indulge in claims about how they result in more accurate descriptions of the social world.

CHAPTER FIVE

PROFESSIONAL COMPETENCE¹

My ethnographic research had three main components. Firstly a participant observation study of the work of the central accounts office of a large, multi division electronics company. Second, a two month study of the work of a large partnership. Again a participant observation methodology was employed, although in this case it was supplemented by extensive interviewing and documentation. Third, interviews with and observation of the work of an audit client of the partnership. Observations made in the first of these is the resource for this chapter.

The central accounts office in question comprised of seven personnel of varying degrees of accounting skill. The bulk of my research time was spent on the activities of the most junior members. Their work was the initial stage of all the accounting in the office. It was therefore a convenient beginning for a sociological characterisation. The relative simplicity of their tasks also made comprehension easier. A variety of aspects of the activities of these personnel struck me as interesting. I have discussed some of these in my MA thesis²; here I want to focus on what I shall call the

¹A version of this chapter was printed in The Journal of Interdisciplinary Economics, 1988, Vol 2, pp 297-306. And as Occasional Paper Nos 19, Sociology Department, Manchester University, under the Title: Not Any Old Numbers.

²Manchester. Faculty of Economics and Social Studies, 1983.

common sense knowledge these personnel had to employ to do their work. This focus, it seems to me, is one that coincides with the ethnomethodological studies of work program. In the previous chapter I have argued that the use of an approach is to be evaluated by reference to how that approach has been used by others, and not so much by regard to other programs. This is a policy I will abide by here. Consequently my references are to the ethnomethodological literature alone.

In chapter three I explained that one of the primary concerns of ethnomethodology is to examine the relationship between programmatic statements about behaviour and practical action (Bittner, 1965; Cicourel, 1967; Garfinkel, 1967). Such examinations are particularly useful for the purpose of generating a sociological conception of work since they provide detailed evidence about what work consists of. I argued that the recently developed program known as 'studies of work' has added a new emphasis and has shown, amongst other things, that some important aspects of work activities are not described or necessarily implicated in programmatic statements of organizational definitions (Bittner, 1967 a:278; 1967 b:699; Sharrock & Anderson, 1985; Garfinkel, 1986; Wieder, 1974; Zimmerman, 1969 a:319; b:1969; 1971:221). This is despite the fact that these aspects are sometimes crucial for practical real-worldly action.

An example of this is M. Lynch's study of the work of bio-chemistry laboratory technicians (Lynch,1979;1982). I touched on his work in chapter three. Although Lynch is interested primarily in characterising the *work of science*, his approach and observations are equally applicable to all those activities defined as work. He observed that one important aspect of the laboratory technicians' activities was separating and distinguishing photographs suitable for display from those only suitable for including in statistical calculations. This would not have been an observable fact of laboratory work if Lynch had simply examined the methodological descriptions found in journal reports and articles. In these latter statements there was no specification of the fact that choices had to be made between photographs, the problematic of that choice being obscured by a description of methods that focused only on what was done with the photographs once they had been selected.

Nonetheless Lynch did not suggest that the methods reports distorted the nature of laboratory work. Rather they were designed to reflect only certain parts of it. Some activities, like the selecting of photographs, were treated by technicians as so mundane - irrespective of their actual importance - that they were considered irrelevant when it

came to characterisations of their activities³. This was because some activities were a prerequisite of the work and underscored efforts to follow methodological schemas. They were, so to speak, things that were "taken for granted", "obvious" and "what any technician would do". To include descriptions of them in a methods account would not only be unnecessary but would also bring into doubt the technicians competence at their job since they should know that that was not the kind of stuff that went into these descriptions.

Thus, with his observations, Lynch brought to light some *minimal adequacies* or routine decision making procedures that were so basic that they were not considered relevant for methods' commentaries or work definitions by participants themselves.

The lesson that can be taken from this is that a sociological conception of work should include (A) what is contained in programmatic texts; (B) the other unspecified but nonetheless necessary aspects of action and (C) members' (i.e. workers') own ability to apply what may be thought of as the conventions about what is and is not included in work definitions. Failure to incorporate these elements would mean that potentially important and certainly interesting features of social action might be missed.

³To return to the discussions in chapter three, another way of expressing this is to recall Garfinkel's 'ghost train' analogy.

As I say, this kind of research program struck me as relevant to the task of explaining some of the activities I found interesting in the central accounts office. For I noticed that there were several aspects to the accounting work which were, as far as I could see, not included in organizational programs, but which were, nonetheless, minimal requirements or *adequacies* that made the completion of work possible. Primary amongst these were devices to check and correct numbers *before* they were included in the accounting procedures. I noted that these devices were premised upon workers' abilities to recognise patterns in the numbers. Furthermore, these recognition procedures were treated as a tacit aspect of work. They were not 'visible' in the products of those activities (i.e., one could not see they had been done by looking at the figures in question. What I mean by this will become clear by the end of the chapter). Nor were they included in descriptions of the methods by which those products were produced. Insofar as the accounting work was premised upon these activities it seems reasonable to suggest that one might call them the *common sense* aspects of the work. Moreover, insofar as using this common sense was treated by the workers as a sign of professional competence, then one might suggest that what I am looking at here are the common sense skills of the professional.

The Weekly Figures

One of the main tasks of the accounting personnel in the

central accounts office was the compilation of the *weekly figures*. This task took up nearly three full days of any working week. The work of doing the weeklies, as they were known, was glossed to me in the following way. It was explained that each week every division of the company submitted schedules of numbers representing various items of company activities. These figures were formed into columnular arrays on the schedules which were standardised and regular over the weeks. The task was to transcribe these figures onto the central office's own schedules, thence onto computer. After that a total of the figures for all the divisions were calculated so as to produce an account of the company's activities as a whole. These final schedules were identical in construction and pattern to those submitted by the divisions. In principle the work only involved arithmetic and transcription. All of this had to be done within pre-set time limits. The remaining time in each week was necessary for other tasks. Although the weekly schedules were little more than records and were not used in the management or financial accounts, they were helpful as an index of the company's activity.

Judging by this description, the work of making the weeklies might appear to be straight forward : there were no difficult accounting procedures or elaborate calculations. But the work was not entirely unproblematic, nor was it inconsequential. In fact, personnel devoted considerable effort and time checking that their work was done properly.

One reason for this, and perhaps *the* overriding one, was that they were *accountable* for this work. This was because the production of weekly schedules and the presence of the figures on the computer readouts were the *products* of the activities of personnel. It was through these that their competence was judged. These products had to conform with certain standards - standards which lay at the very core of proper accounting. Minimally this meant that accounts had to be accurate and complete. This was necessary if they were to be a *reasonable* and *objective* representation of the company. Fulfilling these two criteria is virtually the *sine quo non* of all accounting (Johnson, 1981). It was therefore the goal of the accounting personnel in this context. Failure to achieve this goal would bring into doubt the competence of the staff. This was one reason why staff put so much effort in to the weeklies. But another was that doing the weeklies involved more than simple arithmetic or the appliance of the rules of accounting. For, if they were to simply add up and transcribe the figures submitted from the divisions, the personnel would be vulnerable to the possibility that their work was inadequate in the most important way : the figures they used might be wrong in the first place. If that was the case, however properly they were added up or transcribed, (i .e, whatever the accounting procedures they went through) they would never be right.

Therefore ways had been devised of checking the figures so as to prevent the inclusion of mistakes in the work. These

prevention procedures were like preparations for doing the weeklies but were not included in definitions of the work. They were only mentioned as a relevant matter when the work was actually done. This was because, in being preparatory, they made the accounting possible *without* being strictly part of the accounting itself. My concern here, then, is with these activities and their inter^frelationship with *professional competence*.

Preparation of the Figures

Broadly speaking, being able to check the numbers were correct before they were used in the calculations involved recognition of patterns and regularities. With knowledge of these patterns, personnel were able to predict, or at least have some idea, what the figures ought to be each week. If the figures did not coincide with these expectations then investigations would be begun into possible causes of these discrepancy between the numbers received and those expected. The numbers would be corrected as necessary in light of these investigations. Patterns in the numbers resulted from the correspondence between changes or regularities in the thing represented and the number itself. There were four characteristic patterns.

First, some items represented on the weeklies altered with the season. The primary difference was between summer and winter. For example, the heating bill was, unsurprisingly, larger in the winter. This figure would be included in the

category "electric and gas" (although gas was not used, its inclusion in the title of the heating bill column was legacy from some previous time when it was). The figures in the electric and gas column would reflect these seasonal alterations but would remain fairly consistent from week to week. That is to say, during the summer they would be consistently low, during the winter consistently higher.

Second, some items reflected trends. For example one of the expanding divisions was steadily increasing its output. Also demand for the goods produced was very predictable and steady. This meant that the figures for income displayed a steady increase too. Staff would expect an increase in each week's figures by a regular amount. If a figure did not correspond with the expected then inquiries were begun to ascertain why.

Third, some items would not alter much from week to week. For example it was company policy that routine maintenance cost be calculated to be even throughout the year irrespective of actual expenditure in any one week. Thus any difference in the latest week's figures from previous would be treated as good grounds for doubting the correctness of the latest figure.

Fourth, some items fluctuated without any discernable pattern. For instance, one of the divisions sold high technology items at a low rate of turnover. Sometimes income from this division would be very high, the next week very

low. In these cases it was very difficult for staff to judge whether the latest figure was correct because there was no expected figure against which it could be compared. However, there were only two or three figures like this out of the two dozen or so on the schedules.

Once a number had been identified as incongruent with a pattern (whatever form that pattern took), staff would start to look for possible sources of that incongruity. The philosophy behind the inquiry was : there is a typical answer to this calculation, why has it not come out?

To begin with, on the basis of what they knew about the individuals who had produced the schedules, personnel from the central accounts office would locate possible sources of information in the division concerned. They either phoned or went along to ask why there had been a change. Most often the oddity was seen to be the result of a change in the circumstances of the division that personnel in the central office did not know about. For instance the division may have recruited some new employees or may have bought some expensive new machinery and had, in either case, not informed the central office. Consequently, although the figure *correctly* accounted for the new state of the company, the personnel in the central office did not know this when they first recognised the number as odd. In discovering that the number was in fact a good account, staff revised their knowledge of the company to accommodate the new facts. In

subsequent weeks they would not make the same mistake since they expected a number that reflected the change.

However, such inquiries could not always take place. There were occasions when a figure's oddness could not be explained at that particular moment. For example the personnel who had originally calculated the figure and who therefore may have been able to provide information about why it had changed or was odd in some way, may have been on holiday or perhaps ill. Sometimes documents were temporarily unavailable. This was especially the case when auditors were on the site and were inspecting documents.

The response in these situations was to leave the number as it was and not to return to it until more information was available. Often this meant in practice waiting until there was some spare time or when senior executives or auditors drew attention to it. The number was left until its true significance became apparent in the future. It would not be disregarded, but set aside for another time. Needless to say, sometimes a number such as this could be forgotten, but the general rule for dealing with them was that if resolution of the oddity could be done more expediently at some time in the future (and that might only be the following afternoon or the end of the week), and if it did not hinder the completion of the rest of the figures at that time, then they could be put aside.

In some cases it was not deemed necessary to make inquiries

into why a number did not conform with what was expected. Instances of this involved numbers that were very regular or did not change from one week to the next. The example of maintenance costs has already been provided. It was similar with depreciation costs, that is, the amount that an item was judged to have lost in value over a specific period. This was calculated to be even each week. It was only when an item was "written-off" which means that it had reached a point where it was considered to be valueless - that these costs would change. But that was very rare and if it did occur the division informed staff in the central office. Thus if the figure for depreciation was different from expected, staff had a good reason to consider the number as incorrect. And indeed this was most often how they treated it. All that happened was that the discrepant number was changed (literally whitewashed out of the accounts) and replaced by the expected number. This replacement number was assumed to be correct. No effort was made to contact the division because it was assumed that just as the staff in the central accounts office had easily recognised these mistakes so too would somebody in the divisional accounts offices - even if they did not do so immediately. In effect, deviation in these figures was automatically seen by staff as a result of human error at a previous stage in calculation. Human error was of course an ever present possibility in accounting activities but was assumed to be only a factor on these occasions.

But staff could not always treat changes in these types of numbers in this way. There were very rare occasions when it came to be asked why personnel in the divisions had not in the first place noticed what - to people in the central office- was so obviously an error. If there ever was any doubt of this kind it was only ever taken account of if central accounts staff had an inkling that some kind of change had occurred - perhaps a machine had been sold or written off which affected the depreciation costs just mentioned - which had affected the otherwise regular numbers. Although these suspicions were evanescent things, based on what can best be described as rumours or overheard comments in the canteens, they would still be enough reason for staff to be concerned. As a result the responsible person would be contacted and the number checked.

Not only were personnel aware of the four general characteristic patterns of the numbers, but they also had considerable insight into particular facts about each figure. For instance, the wage bill was very regular over the weeks. Therefore the figures that accounted for this bill were also regular. But there was an occasion when a Muslim worker in one of the smaller division took six months leave to go on a religious pilgrimage to Mecca and because the man claimed that it was a religious holiday, which was hotly disputed by the company - the division was legally obliged to keep his place open for him and a replacement could not be hired. Consequently the figure for wages



dropped. This meant that there was an inconsistency between the previous weeks' figure and the first week that this change came through in the figures. But the central office staff did not recognise this figure as odd. Instead they *expected* this change since the divisional accountant (or one of his deputies) had mentioned it to them. Therefore, they did not waste time checking to see if there was an error since they knew that the change in numbers reflected a real alteration in the company's state of affairs.

Calculations of the totals were also subject to the same procedures. Just as with the divisional figures, those for the entire company had detectable patterns in changes to them which could be used as a guide for assessing their correctness. Indeed nearly all of the work done in the office went through similar kinds of informal assessment processes.

The main advantage of working this way was that there was not the time to check all the mathematical stages of calculation of every number each week. But it was not necessary to do so anyway. The bulk of numbers were correct. Using predictions to assess the latest week's figures enabled staff to concentrate their enquiries in only those areas where the wrong figures were most likely to be present. It did not thereby guarantee that all errors were removed but served to ensure that the bulk were dealt with in the minimum of time.

It was very important that there were as few errors as possible in the accounts. If too many were left unattended their appearance in the finished accounts would draw the attention of other personnel in the future. Specifically these might be external auditors or senior management in the company. But, irrespective of who generated these queries, personnel would have to provide explanations to the following sorts of criticisms: why were the procedures of calculation not checked? why are these numbers in the accounts if they are an unsound representation of the company's state of affairs; and most tellingly, why didn't the accounting staff see these mistakes in the first place?

Thus preparing the numbers so as to pre-empt such questions was an important aspect of a worker's competence. Failure to work this way was only excusable for the novice, that is to say one who had not been there long enough to be familiar with the patterns in the numbers. This kind of novice could include someone with extensive accounting qualifications but without the specific experience gained in that setting.

It can be seen then that it was both a concern and the responsibility of staff to minimise the occurrence of errors in the finished results. Procedures for this were an inextricable feature of those tasks. But these procedures were not manifest in the accounts themselves. For correct accounts showed only that transcription and addition had occurred. The fact that considerable effort had been expended trying to guarantee that only correct numbers were

allowed into the accounts in the first place was not itself an observable feature.

This was why the definition of the work of doing the weekly figures did not include these activities. For, strictly speaking, the work only involved transcription and addition. Certainly, checking that numbers were correct beforehand was minimally required to be able to do the job but it was not itself part of the job. Such a definition was provided.

Implications

It might be thought that using what the numbers typically were as a basis to assess what a latest number should be is a dubious practice. This would be wrong. These practices did not constitute ritual (Gambling, 1977); nor were they subjective accounting techniques (Stamp, 1981, Sterling, 1979). Staff knew the figures well enough to be sure that what the numbers ordinarily came to was indeed what they ought to come to. They were confident that even if they did investigate the methods of calculation of every number the figures then obtained would be the same. The crucial point to be learnt is that staff would not accept *any old numbers*. Experience had taught them what the correct numbers were; their daily practices ensured that only these were used.

Taken as a whole, the existence of these kinds of *ad hoc* practices (Garfinkel, 1967 pp1-13) might bring into doubt the notion that the verisimilitude of accounting is based on

the independence of accounting practices and methods from the practices of the individuals engaged in the tasks. As has been pointed out, belief in this independence is virtually the central principle of the accounting profession. The evidence provided here indicates, however, that this independence is itself dependent upon the context specific practices of the personnel involved. By this I mean that staff had to use their own locally generated knowledge of the typical and predictable features of figures as a method whereby they could decide whether a number was suitable for entry into the accounting. This knowledge was derived from practical experience of the company and of the accounts. Through endless inquiry, revision and confirmation it was kept up to date. This proved the bedrock for competent accounting. Thus, the independence or objectivity of the numbers can be thought of as an *ongoing social accomplishment* of accounting personnel. This is thoroughly embedded in practical daily activities and was not achieved by applying the rules of accounting alone.

Conclusion

I have argued that important aspects of accounting work are not visible in the products of that work. The reasonableness and objectivity of accounts was partly premissed and maintained by the methodic application of common sense knowledge about the accounts and the company. This knowledge was subject to endless revisions and adaptation. Use of it was treated as a mundane matter and

therefore not suitable for methods' descriptions. Use of it was, nonetheless, a measure of competence.

Finally, it seems to me reasonable to suggest that these observations resonate with some of the implications of Lynch's study of laboratory science. It will be remembered that Lynch had observed that there were some aspects of laboratory work that were essential but which were not included in definitions of work methods. This was because these aspects were seen as so basic that any competent technician would know about them and apply them as appropriate. This was because they were not the sort of thing a technical would *need* to be told about. Lynch defined these elements as *minimal adequacies*. Likewise, in an accounting setting, some features of work were not considered relevant for *definitions of methods* but were nonetheless necessary. This was the case with the preparation of figures before they were used in the compilation of the weeklies. This preparation may be thought of as a minimal adequacy of accounting work. Inability to know about and achieve this minimal adequacy would render the products of accounting activity unsatisfactory. Ultimately this would result in the questioning of a worker's competence.

CHAPTER SIX

ACTING PROFESSIONAL IN AUDITING ENVIRONMENTS

In this chapter I will discuss some of my observations of a chartered accountancy practice and in particular of the audit division of that practice. As I have explained, I employed a participant observation approach, but this was extensively supported by interviews and analysis of documentation. My research involved spending considerable time with the auditing accountants, traveling with them and spending periods away from home on audits. From the beginning I made it clear to them that I was an ethnographer but this did not seem to handicap me in any way. I came across no restrictions to access or materials, indeed the reverse seemed to be the case: the title ethnographer seemed to confer on me a right to every aspect of an auditing accountant's activities. Needless to say I did take up every opportunity offered me; for one thing there was simply too much on offer. But in any case one naturally began to focus on certain things at the exclusion of others. From virtually my first day in fact, I was struck by one phenomenon. How it was that so many were prepared to commence an accounting career in the audit division and how few ended up making it to the top of that career, to partner level. In practice, only one in seventeen made it. Apparently this was not unique to the firm, but is common throughout the profession. I decided therefore that this would be one of the topics that I would consider in greater depth. My reflections on

this matter constitute this and the subsequent chapter.

It seemed to me that there were several ways one could consider this issue. The first of these is *dramaturgically*. One reason, and perhaps one of the most powerful, is that participants themselves made references to the 'acting of a role' and 'being on stage'. In other words, they had their own lay version of events which was very similar to what one might imagine would be a dramaturgical version. Given what I said in Chapter Two, this would be reason enough to pursue a dramaturgical approach. For if nothing else it means that the sociological analysis is very close to actors' own understandings. But this notwithstanding, this is not the main reason why I have chosen to adopt this approach here. Instead I have done so because I think dramaturgy offers me a vehicle to explain a variety of conduct in different settings in a unitary manner. One reason for this is because dramaturgical analysis focuses on the presentation of self and two features of this presentation in particular: how behaviour can be seen to be front or backstage, and how it might involve doing role distance. Use of the dramaturgic approach is then to be justified not on the grounds that it reflects the way auditors and accountants talked about themselves - although there is that in part - but because it offers some methodological advantages. This may mean that the chapter is to be assessed not on solely empirical grounds. As I have said, disputes about empirical matters are often spurious; here I make no pretense that being

empirical is my primary concern. But it does mean that what follows is to be judged by how well I marshall the concepts in question and thereby illuminate the phenomena. As in the last chapter such assessments then will need to be based on comparison with studies that employ a similar approach. In respect of the common sense aspects of professional competence, this was ethnomethodological literature; here, in respect of the presentation of self, dramaturgical studies.

Let me explain what I propose to do in more detail. I would like to argue that much of the work of auditors consists of *frontstage activity*, and a lesser part of it *backstage*. It seems to me that movement up the auditing hierarchy increases the time an auditor spends frontstage. If this is so then promotion equates to movement toward frontstage. I will also suggest that role distancing has strategic importance for the maintenance of the structure of front and backstage environments. One reason for this is because it allows for the preservation of harmony, and for the display of charm. The central thesis then, that I put forward in this chapter is that the ability and willingness to adopt front and backstage roles and to utilise role distancing techniques effectively, is one way of explaining why some individuals make it to partner and some do not.

Frontstage, Backstage

The *dramaturgical* approach derives from K.Burke, (1962;1965) although perhaps its most famous exponent is Goffman (1959; 1961;1963(A); 1964; 1969)(for a background see: Messinger *et al*,1962:98-110). But it is an approach that has been employed by a whole variety of authors (Brisset & Edgeley, 1979). It is based on a very simple analogy: that some behaviour is in accord with a role that is performed in front of certain audiences, this is the *frontstage* role; whilst other behaviour is carried out away from that audience where the more 'formal' role is dropped and a *backstage* role adopted. Typically, backstage roles contrast with frontstage ones insofar as the former are supposed to be more representative of someone's true self. Furthermore, front and backstage refer not just to behaviour but to the organisation of physical objects in a setting. So for example, objects are selected and arranged so that a particular impression is given. As in a theatre, objects provide a context or *set* in which *roles* can be played. In effect this approach treats social activities and the materials of social activities - anything from clothes to buildings - as analogous to theatrical behaviour and sets;

and hence the name: *dramaturgical analysis*¹.

In this chapter I will suggest that frontstage activities for auditors are all those activities that are engaged in before clients and superiors, backstage are those adopted in the absence of either of these. Most often backstage environments involve individuals of equal rank. As individuals progress up the career ladder they spend more and more time in front of clients and subordinates and so more and more time frontstage.

¹The dramaturgical approach has considerable similarities with *symbolic interactionism*. And indeed dramaturgical and symbolic interactionist studies are often indistinguishable. For example, Ball's symbolic interactionist '*Ethnography of an Abortion Clinic*' (Ball:1967:293-301) describes the impressions given to clients by the decor and atmosphere of a clinic and the personnel manner of its personnel. This impression results in client's believing the clinic to be hygienic and professionally run; and that the staff are genuinely concerned with their predicament as women with unwanted pregnancies. That they show concern helps bolster the confidence of the clients at a time of personal grief for them. However, Ball goes on to describe the activities of staff behind the 'closed doors' of the clinic. He observes that behind the scenes staff are concerned with such things as the money made out of each abortion, ways of cutting costs, increasing the speed, etc. The aborted fetuses are treated as 'merely' garbage and thrown into trash cans along with the waste paper. Staff have no real interest in the fate of the clients. What Ball describes are the two sides of a symbolic environment; the symbols present in each communicate certain ideas or impressions. It is therefore symbolic interactionist analysis. But in addition, and by the same token, these two symbolic environments are analogous to frontstage and backstage. In one the clients - who are the audience - are presented with a certain kind of image, in the other they have no access and if they did the image they had of the place would be very different. For, in each, the roles performed by staff, and the 'sets' used, are distinct.

Role Distance

To explicate the details of individual conduct within these locales I shall refer to the idea of *role distance*. However, whereas the front-backstage analogy has been used by a variety of analysts, and my use of it reflects the general principles they employ, my use of the concept role distance stems directly from the work of Erving Goffman (1963:85-151; but see also Levitin, 1964:251-60; Ford *et al*, 1967:370-81; Stebbins, 1969:406-415). I have already made some remarks at an earlier juncture about this author. That I limit myself to Goffman's use of the term means that I also have to deal with the idiosyncratic features of his argumentation: it is at once logical and paradoxical, loaded with insight and acute observation but tied together by a seemingly untamed use of categories, labels and definitions - most of which do not appear, at first glance at least, to be adhered to by Goffman himself². In fact however, I think that it is generally accepted that close analysis will show that he uses the term in a way that Williams (1988: 64-89), using Baldamus's original coinage (Baldamus 1972), calls a process of 'reciprocal double fitting'. That is, he uses it (amongst many others) to define the object of his inquiry, and having done this, produces a provisional analysis, which

²One reason why theoretical issues are sublimated in this chapter is because of the sterility of much theoretical debate in relation to Goffman. See for example the debate between Coser (1966: 173-187) and Stebbins (1967:247-250) over the concept role distance.

is itself used to further refine the concept. This refined version is then used to relocate the object of the inquiry. The end result of this is that Goffman's concepts, role distance included, seem endlessly revisable³. It is therefore somewhat problematical to use any of his concepts. I do not think I need to worry about this too much, for I will opt for only one version of the concept. One may begin by using Goffman's definition of a role:

'A role consists of the activity an incumbent would engage in were he to act upon the normative demands upon someone in his position' (1963:85). 'Incumbency tends to be symbolised through status uses of dress and manner, permitting those who engage in a situation to know who they are dealing with'. (1963:87).

Moreover, a role, or rather the manner of its performance, implies something about a self. Finally, all individuals have a multitude of roles. They may be a mother, an academic, a daughter. These roles are adopted or enacted dependent upon the audience present and the activity engaged in. To the extent that the adoption of any role or roles involves imputation of a self, individuals may display some distance from a role or *role distance* on those occasions when they want to indicate that their true self is somehow different. Role distance may involve such things

³For further discussion of this see Williams, *op cit*; but also the two edited collections on Goffman by J. Dutton (1980) and Drew & Wootton, (1988). More interesting papers in these collections are by Rogers (1980:100-133) who discusses the importance of social structure in his work; and Schegloff, (1988:89-136) who provides the most effective critique of Goffman's empirical pretensions.

as deliberately not adopting or the ridiculing of a role whilst performing the role.

I shall use these ideas about role distance to explicate behaviour in front and backstage settings. I shall show that doing role distance is an important way individuals express their identity, and a method whereby the social structure of frontstage environments is maintained.

The chapter will be divided into two broad sections: the first dealing with the three periods of an auditors career within a partnership: trainee, senior and partner⁴. In this I shall examine how individuals came to recognise front and backstage environments and what were the features of the roles they adopted in each. In the second I shall look in more detail at the relations between different ranking auditors. In particular I will consider the preservation of harmony in audit teams and the social functions of 'charm'.

Section One

Trainee

When they began work in the auditing firm individuals were

⁴Categorization of the accounting career into three is for simplification of analysis. In fact, individuals would go through as many as a dozen levels on their way to the 'top'. Even so this categorization is adequate for the purpose of delineating the main characteristics of learning the professional role and shifting from backstage to frontstage behaviour.

already incumbent of a variety of roles⁵. They were graduates, single, married, middle class, working class, scousers or geordies etc etc; most believed however, or at least they said they believed, that their new role, as trainee auditor, was the one which would lead to success, affluence and status⁶. For they were quick to say that the firm was the finest in the profession (all the other firms were either stuffy, public schoolish, pretentious); that they were the best paid, and that they were the most interesting of all trainees taken on by any firm⁷.

The particularities of their new role was largely foreign to them however. They were self conscious at first. They laughed at each other's suits and the apparent lack of ease with which they were worn; and smirked at shortened, tidy haircuts. But nonetheless they claimed that life was good. They seemed to be committed to their role.

Unfortunately, the work trainees had to do over the first Months - and indeed throughout the first year - was uninteresting, often repetitive and occasionally without any

⁵ Conflict between roles has been subject to considerable analysis. See R.Merton, 1957:369-379; also Biddle & Thomas, 1966.

⁶ A.Belkaoui's 'The Accounting Students' Need for Achievement..' (1986: 197-206) addresses this particular issue but employs an entirely different methodology and approach to this chapter.

⁷ The accounting world is divided in to eight multinational firms and then hundreds if not thousands of small partnerships. The bulk of those who train for accountancy do so in one of the big eight firms. For a background to this see:Stevens,M. (1981), 'The Big Eight'.

purpose (to the trainees, that is). Hours would be spent photostating, days consumed trailing pieces of paper around 'accounting systems' - a euphemism for filing cabinets -and each week there would be more of the same 'bank and creditor' circularization letters⁸ to write. Moreover, not only was the audit work uninteresting, trainees also had to study for exams. And there was a lot of studying to be done. The firm made considerable time available to trainees for this purpose especially in the slack periods, but nonetheless many trainees found themselves going home from work only to sit down to more. It was almost as if they had two full time jobs: auditor and student. This was too much for some: they left.

Those who remained however, and after only a few weeks of starting, expressed their own dismay at their trainee auditor role. They did so by *distancing* themselves from it. For example individuals would play practical jokes: they would hide things; insert false numbers in documents; inform someone that they had an emergency call from home when there hadn't been; direct people to the wrong office; insert photostats of their own faces in people's files or inform someone who had been playing some practical joke that the 'Boss' had found out and that they were now 'going to get

⁸These are letters to creditors and debtors of the company being audited. The letters ask what amounts are owed or borrowed and the responses given are compared to the audited company's own records.

it'; change the insert of a ball point pen when a colleague momentarily left his or her desk so that when they returned they would write on the all important working papers in the wrong colour. And of course people would talk about anything as long as it was not work. They would gossip about each other; tease in reference to respective spouses; contemplate unlikely assignments or verbally "letch" at some absent office mate. In short, they larked around just as school kids do when the teacher is absent⁹.

I would argue that this was distancing behaviour because these activities involved under-emphasising, mocking or parodying the trainee auditor role and emphasising another¹⁰. For instance in emphasising their lust for some trainee of the opposite sex an individual was at once declaring his or her predatory intentions on sexual matters, and emphasising his or her gender role. In so doing the person was reducing the immediate importance of the auditor role. That is they were indicating that whatever they were they were not simply or solely auditors. Or more specifically, they were not certain things that might be

⁹This kind of behaviour, the humour, even bitterness, is similar to that shown on industrial shop floors characterized (albeit not in a dramaturgical vein) under the title 'Banana Time', by Donald Roy (Roy, 1973: 205-223). In both environments adopting such behavioural patterns or 'roles' enabled individuals to express their frustration at certain experiences and requirements thereby making their work tolerable. (In the case of the workers Roy examined, it also fought off the "beast of boredom").

¹⁰The term trainee auditor role will be used inter-changeably with trainee role, trainee or auditor.

implied by the auditors role. For if auditing was boring they were not going to be, or be seen to be, boring too. Through humour, disdain, obvious displays of disinterest and frustration, trainees distinguished themselves from the task at hand and thus the role of auditor so that no one would make the mistake of thinking that the auditor role implied anything about themselves.

Trainees could not distance themselves from the role all the time however. For if they were caught by more senior personnel looking simply empty-handed, let alone obviously 'messaging around', they would be severely rebuked. They would be asked: what do you think you're doing here? Do you think you're still at university? Are you at work or in the playground? And so on.

In being told off for not adopting the appropriate role, trainees learnt that there were two environs within the general locale of work. One where they could express another side to their identity or their real feelings about the audit role, the other where they had to appear totally committed to the role. What distinguished the two locales and necessitated the adoption of these various attitudes were the persons or audiences present in each. In one there was typically only trainees, in the other senior personnel and clients. In environments with the latter categories trainees had to adopt the trainee auditor role completely. That is, they had to act out the role of trainee. Thus those

occasions when senior personnel and/or clients were present was frontstage, when only their fellow trainees present, backstage¹¹.

Formal Methods

If direct and immediate rebuke by senior staff or clients constituted an *informal* method for teaching trainees the importance of behaving appropriately in frontstage locales then there was also a more formal method to inculcate this. Those who did not 'get the message' in informal ways would be forced to consider their attitude in the light of their 'report cards' and in 'personal interviews' they had with managers.

Report cards were compiled by the person directly responsible for any trainee on an audit and included remarks about attitude. The trainee would be shown these cards. Managers used them as a basis for discussing with the trainee where improvements in ability and conduct were necessary. These discussions would occur in 'interviews'. Managers would interview trainees whenever promotions were due (every 9-12 months). Interviews of this type continued

¹¹ Senior staff were well aware of the fact that trainees engaged in backstage activities and confided that they could understand why trainees felt as they did about the work - especially in their first year when the work was dull and the *ennui* of student life still lingered in the minds of trainees. But what they wanted trainee staff to learn was not that they should be interested in the work itself but rather that they should look busy and occupied when necessary. Most importantly, this needed to be the case in front of clients.

throughout an individual's stay at the firm. During them the problems and successes of the individual in the past months would be considered. This would include discussing any behaviour that was not considered 'appropriate'. The individual would be urged to curtail such conduct. If they did not, they were told their career at the firm would not be 'successful'. In effect, they were given a choice in the form of an ultimatum -learn to behave or leave.

So, even in the earliest months of their career, individuals had to learn not only how to account (or audit) but how to conduct themselves. That is to say, they went through a *socialization process* which was designed to produce a certain type of individual or, more precisely, one who was capable of adopting different roles depending upon the audience present¹². At this level, and in very simple terms, learning to adopt these roles consisted of an ability to discriminate between the audiences before whom it was important to look busy and those in front of whom it did not matter. Adopting such a competence was achieved partly through the voluntary, self-cognizant actions of the individual, partly through constraint.

¹²Learning to discriminate in this way is, of course, not unique to accountancy. After all, even children learn that it is not such a good idea to do certain forbidden things in front of parents and adults generally.

Seniors

After three or four years, trainees would have completed their examinations. They were now *chartered accountants*. They would become 'seniors' within the firm. Hitherto, the work had been frustrating and boring, but now, with senior status, individuals were responsible for the audit teams on site, organized the nature of the audit and were effectively responsible for the efficient running and completion of the work. This made work of seniors enjoyable. It 'made more sense' in that part of the frustration of the first months in the firm was not knowing the rationale behind the drudgery - now as seniors they were the individuals who decided what the rationale would be. Being involved in decision making at this level also required more abstract reasoning which in itself was more satisfying. The 'boring' parts of the work still had to be done, of course, but new arrivals, i.e. trainees, would do the boring stuff.

Thus seniors would not have the same reasons for dissatisfaction that trainees had. But seniors still had much to learn: in particular more detail about what the frontstage role consisted of. So far I have only described what frontstage roles consisted of in loose terms: 'looking busy', 'seeming occupied' and so on. In fact, it was in precisely these kinds of vague terms that frontstage behaviour was defined to trainees. At their level in the career abiding by such maxims of conduct was, more-or-less, all that they needed to do. The frontstage 'role' for a

senior, however, was quite specific in terms of what it required. They had to adopt certain rather constraining ways, or strategies of self-presentation (Goffman, 1959; Brisset & Edgley, 1974:68-77).

These strategies were largely to do with image. So for example seniors had to concern themselves with what suits they wore. Suits were, unsurprisingly, a necessary requisite for auditing and had been since individuals first arrived at the firm; but at senior level they could no longer wear *any* suit. They had to be discreet without being bland, well-cut without being extravagant; for seniors would be told by managers and partners that if they wore extravagantly expensive suits clients would think them overpaid. Seniors had to be well groomed. If, in their first year as trainees, untidy mops of hair were accepted as a final echo of undergraduate sloppiness, by the time they became seniors, neat (and needless to say, more expensive) haircuts were treated as a token of burgeoning professionalism. Seniors would be told that clients would perhaps associate sloppy dress with a sloppy auditing. The firm helped to provide the appropriate personal image too. Seniors were issued with distinctive (and oversize) briefcases with the firm's logo embossed on the leather. They were given thick year-planner diaries which were to be placed in a prominent position on whatever desk they were beside. They were given a company

tie (a somewhat ubiquitous aspect of professional life)¹³. Concern for image went even so far as to include modes of transport: motorcycles were as unsuitable as decrepit old cars. It would be pointed out that motorcycles might give the impression of 'laddishness' and therefore possible inability to make calm and objective assessments, whilst clapped-out cars might suggest auditors were incapable of running their own affairs and therefore were unlikely to be able to assess those of another.

In personal manner, individuals had to train so called abrasive edges. Courtesy and patience had to replace abruptness¹⁴. Simplicity of expression had to supplant opacity and an ease with clients that cultivated confidence had to supercede apprehension and self consciousness. Above all, they had to learn to give the impression of mental acuity and intellectual finesse.

If seniors did not abide by and live up to any of these

¹³Very few seemed to wear them. Some said they thought company ties were old fashioned. They were not therefore suitable for their vision of the auditors' role.

¹⁴Courtesy may seem a fairly obvious requisite. But at times auditing can be a very frustrating activity. It can sometimes appear to auditors that clients are deliberately obfuscating matters or making it difficult to locate documents. One auditor was nearly sacked when it was discovered that he had pinned up against the wall a particularly stubborn clerk who had failed to produce some requested documents. The auditor did not get dismissed however because the boss of the clerk, admitted that it was probably likely that the clerk had been winding up the auditor. Thus the clerk got the more severe rebuke. Needless to say a reputation for unusual methods followed this auditor throughout his career.

strategies of self presentation, they would be informed of it by more senior personnel, either in the interviews they would have before promotions, or informally.

However, if as trainees learning to 'look busy' when necessary was fairly easy to do, all these additional details were not so easy for seniors to adopt. To start with, this side of individual conduct, this aspect of 'professionalism' was not amenable to control in the way success in the professional examinations was subject, crudely speaking, to the amount of time put into revision. And moreover, gaining the requisite attributes of the manner was not straightforward. An arrogant or insouciant air could not be changed overnight (even if they were pointed out by others), just as it was not easy to be a smart dresser. Some people have greater sartorial sense than others¹⁵.

Trying to adopt these elements of personal manner was too much for some. They opted for the ultimate role distance behaviour: they left. Others departed simply for the security of a job in industry (and so were not changing the role of accountant and auditor, only the context of its

¹⁵This proved to be problem when I, as the researcher, tried to adopt the role of senior in the research. Amongst other things I did not have the money to buy a suitable suit. I was often rebuked for looking too like a 'sociologist'. Conversely, when I returned to my sociology department I was mocked for looking too like an accountant or more scathingly as a 'displaced Yuppie'.

performance¹⁶). Those who remained, however, and who wanted to go up the ladder to partner had to adopt these elements of personal presentation if they were to succeed.

It was all the more important that they do so at this stage in their career because competition for promotion was extremely intense. It was normal for most people to make senior level but very few, even of those who wanted to stay, got promotion beyond that level. As I have mentioned, only one in seventeen of those who joined the firm made it to partner. Consequently, such things as personal manner were another way an individual could display keenness and worthiness for promotion (alongside such things as accounting skill, not addressed here). And indeed this is what ambitious seniors were told, either informally or formally. In effect promotions pressure increased individuals' willingness to adopt the appropriate methods of presentation.

In short then the role of senior was a more detailed, demanding one than the role of trainee. As with the latter, however, it had to be adopted in the presence of superiors and clients. Backstage locales were defined by the absence of these audiences and typically involved individuals of the same rank.

¹⁶It needs to be added that there are likely to be considerable differences in the role of accountant in industry as against that in a partnership. These differences will have to be investigated in the future.

One of the most common behaviours in backstage settings at senior level was story telling. These stories could not be told to frontstage audiences for they were about promotions. Specifically they were about actual and theoretical or 'supposed' criteria for promotions and about the relative merits of the story-tellers themselves¹⁷. So, for instance, seniors told stories about how it was that someone was promoted beyond senior (or to a higher grade of senior) 'merely because he was a smart dresser'. 'just a smooth talker', or a 'crawler'.

In telling such stories, seniors were defining not just what they perceived were the 'real', i.e. the corrupt, favouritist criteria for promotions, but by implication and contrast, also what they saw as what 'ought to be' those criteria. What they defined as the latter was indicative of the extent to which seniors had come to adopt some of those elements of personal presentation I have just described. For, in the stories, it would be suggested that just as an individual should not get promoted for being someone's favourite, so too should a person not get promotion if they were scruffy, lacked seriousness or were arrogant and supercilious - i.e., because they lacked the ability to

¹⁷Of course talking about promotions may well be part of a work role. In this case however, the type of talk about promotions was clearly counterproductive, since it meant that auditors did not look busy: they were just chitchatting.

adopt the frontstage role¹⁸.

Though these stories were told backstage, they were not necessarily role distancing. For often in the stories individuals would express how it was that they would be happy to adopt the 'proper' senior auditor role. It was only that they were unwilling to adopt the 'corrupt' version of this role. So they were distancing themselves from the corrupt role but showing their potential commitment to the what they saw as the correct or legitimate role.

The question of role distancing aside, telling stories could cause problems. For one thing it could lead to strains on other roles of an individual. For instance the role of friend could be undermined if too much jealousy was expressed about a friend's success. More significantly in terms of the promotions race, if these stories were to leak out of backstage environments and be heard by frontstage audiences they may be taken by those audiences as indications of an individual's unsuitability to take on the role of manager or partner. For the individual would be seen to be - or more precisely, heard to be - indiscrete, a big mouth, backstabbing, uncomradely and so on.

¹⁸ Although it is not always legitimate to suggest that stories have a 'purpose' or a function, in this context it did appear that seniors used the stories as a resource to protect themselves, or rather their dignity, in the event of failing to get promoted. For if that happened, they could always say "I told you so. People who get promoted don't do so because of merit!".

Consequently, seniors had to be constantly alert to whom they were with and what they were talking about. In some respects, they had to learn to be somewhat premeditative, even about behaviour that was backstage. This contrasts with the relative lack of guile that underscored trainees' backstage behaviour. This kind of conversational circumspection was indicative of the fact that seniors were beginning to adopt frontstage strategies of conduct even in environments that were backstage. As I shall show, switching behavioural roles between the two locales of performance was of even greater importance when individuals became partners. It is to that stage in an individual's career that I shall now turn.

Partners

Partners' work was of two kinds: internal organization work and *profile* work. The latter was the greatest part of their activities and it is upon that which I shall concentrate. Profile work involved giving the firm a 'good name' and one familiar to as many potential clients as possible. This was done by partners endeavouring to secure and expand business by developing a network of friends, acquaintances and business colleagues from whom new work, it was hoped, would come. This network would be based on membership of diverse clubs and organizations (anything from Rotary, Masons, Round Table, to the local golf club).

Profile work was co-ordinated so that partners would come

into informal contact with representatives of 'targeted' companies whose business the firm wanted. Lists of targets would be drawn up annually. When they met these targets, partners were supposed to adopt the kind of role that would impress clients and which would thereby commend the firm. For members of the auditing firm believed that potential clients would partly judge an accountancy practice by its employees. If an employee came across as, to use the word auditors themselves used, professional, then it was hoped that the potential client, the target, would assume that the firm was professional as well, and hence the kind of firm the client would want to hire. Moreover, this was all the more likely to be achieved because of the locales in which the profile work role was to be performed. For if trainees and seniors had to adopt the frontstage role in locales where the audience expected them to adopt that role, profile work involved the adoption of the frontstage, profile work role in locales where the audience did not expect it to be adopted: for it was adopted in *backstage settings*. It was away from work, it was in leisure and social activities, where the frontstage, work roles would have - apparently - no relevance, nor would anyone be expected to adopt them.

Partners deliberately chose to adopt frontstage roles in these backstage settings because it had considerable advantages for them to do so. For it is commonly believed that the true self is more visible backstage. If, in a

backstage setting, someone comes across as if they have many of the characteristics of an auditor then it is more likely that some other persons in the backstage environment would think the individual really is an auditor. In contrast, if someone met an auditor in a frontstage environment, say in a meeting, then they would expect that person to come across as an auditor, and totally into that role (irrespective of whether they 'really' were or not), because that environment required it of an auditor. To the extent, therefore, that the true self is visible in backstage environments then it is more likely that individuals will judge each other on the basis of those backstage roles rather than frontstage ones. Thus if partners want to be seen to be truly auditors they could more effectively claim this to be the case if they could show it in a backstage setting. And this is precisely what they did.

In effect partners adopted what Merton called *pseudo-gemeinschaft* (1957:109). That is they pretended to abide by a shared set of meanings: that a setting was for example backstage, when in practice they treated it as frontstage. Put another way, it is as if they adopted a *pseudo backstage* role¹⁹. Merton suggests that this is the kind of

¹⁹Stebbins (1967:406-415) argues that the notion of role distance needs to be altered to include the concept of *false* role distance. This is almost equivalent to the notion of pseudo-gemeinschaft, the main difference being that here it is related to the locales of performance.

method a salesman adopts when trying to ingratiate him or herself on a potential client.

Given that this is the function of partners, it is not unreasonable to suggest that the presentation of the self, in particular the ability to adopt a frontstage role in backstage environments was a crucial factor both in terms of the training requirement of the firm and in terms of promotions criteria. Being able to adopt *psuedo backstage* roles was of course not an easy matter. Clients were not fools. They were quite capable of 'seeing through' someone who was pretending to be something they were not. Moreover, it would not look good for the firm if its partners were seen somehow to be dishonest - as if they were for example no better than door to door salesmen²⁰. In the next section I will consider some of the techniques whereby partners were able to 'pull it off'.

Section Two

Thus far I have considered front and backstage environments in terms of the sets of obligations typically applying to participants in relation to matters of personal presentation. In this section I will look in more detail at the relations between individuals within these two locales. I want to argue that superiors used *role distancing* as a

²⁰For an examination of role distance and salesmen see Merton (*op cit*) also Levitin, 1964: 251-260.

ploy to help maintain stability and structure within the environments and in particular within audit teams. These role distancing techniques also allowed for - and indeed were a method whereby - considerable charm or social grace could be displayed. It was this charm, in part at least, that enabled partners to carry off their seemingly duplicitous intentions.

It will be recalled that trainees were told off if they were caught larking around. These telling offs were instructive insofar as they taught trainees to recognise front and backstage audiences. However I did not consider the manner or style of with which these tellings offs were done.

It was often the case that when a junior was rebuked the admonishment would be prefaced with comments like: 'I don't like to say this but', or 'you know I have to say this to you'. In these prefaces, a more senior member of personnel was distancing him or herself from the role of superior. That is a superior was indicating that though the role of superior involved being authoritative, the superior's 'true self' was not necessarily also authoritative. Rather the superior was only being authoritative because his or her role required it of them. Therefore junior personnel should not judge a superior on the basis of what he ^s or she had to do, for example tell people off, because it was not the superior's 'real' self manifest in those actions.

Moreover, when they made an error, misunderstood an instruction or had completely miscomprehended the intention of an audit exercise, the superior would, quite often, not just preface his rebukes with such distancing devices. The rebuke would be mocking, ironic, and performed in an almost cajoling way; i.e., as if to say 'anyone could do this, and there is no reason why you can't either'. Though the seriousness of the rebuke was still made, doing so in such a manner had the effect of avoiding insult to the trainee's self. For failure to perform the role was treated not as a reflection of someone's true ability, but as a consequence of some other reason: they were bored, distracted, not concentrating, and so on. It was as if the superior was mocking the role, and acknowledging thereby that failure to perform it was not indicative of any incompetence on the part of the trainee, but the result of understandable lack of commitment²¹.

Superiors would also show a lot of interest in subordinate's other roles. For instance those performed in the private lives. Thus they would inquire into someone's

²¹Another way of putting this is to suggest it preserved someone's face or their dignity. For dignity is partly shown through the poise and finesse with which a role is performed (whatever the role). Conspicuous failure to perform a role threatens or undermines that dignity. This is even more apparently the case with roles that are not too demanding to perform like that of a trainee.

sporting successes or hobbies, even their role conflicts²².

These role distancing activities and informalities were, paradoxically, and in contrast to the role distancing activities of trainees, aspects or requisites of the roles of superiors. For these informalities and distancing devices allowed for the maintenance of a certain degree of harmony between superiors and their subordinates and more generally, within the audit teams as a whole. Harmony would be fractured when someone took a telling off too personally, or developed a grudge against a superior because of the rebukes.

Harmony was especially essential in audit work because of the nature of the activity. Often auditors spent long hours together in confined spaces and inevitably got on top of one another; in addition, when away from home - not a rare occurrence - they shared the same hotel: and thus they would spend more or less every waking hour together. Consequently, enmities, personal or professional, would have long periods in which to incubate and develop.

Furthermore, in their role distancing activities, their apparent interest in junior's other roles and so on, seniors showed considerable charm. Charm of course is not a tangible

²²It is often forgotten that lay-persons often use terms similar if not identical with sociologists. In this case auditors were well aware of the sorts of role conflicts people might have to deal with. This is an issue that Goffman never really addressed. For discussion of this see Watson, R. 1987.

thing and it would be foolish to define it except in the most general terms, but that aside, seniors did come across as if they had this trait: they seemed to know when to break the tedium and initiate a chitchat; when to be humorous and shatter the tension of frontstage activities. That they could do all these things was in part due to their superior status: they were the only ones entitled to engage in the activities that came across as charming. Junior personnel, in contrast, could not ask out of the blue questions about such matters as a superior's role conflicts. Rather they had to wait for the senior to introduce or begin such conversations. This was because it was the superior who decided when work was to be done, and when it could be halted to allow for chitchat. Not the trainee. To the extent that this was so, it might be suggested that charm was a function of social hierarchy. This however, is matter that I do not have the space to deliberate upon here.

That charm was important - whatever it might or might not be - is further attested to by the fact that partners evaluated the charm of junior personnel, specifically seniors and above, when making decisions about promotions. And they did this not only in respect of how well an individual managed the audit teams, but on the basis of how well junior personnel were able to get on with clients. Partners would be able to make such assessments because they were often, if not always, present in meetings between seniors and clients. Also, they would sometimes ask client's

directly what they thought of the senior in question.

If seniors did appear to have the requisite charm, (in addition a to a variety of other considerations such as auditing competence) then partners tended to develop a paternalistic relationship with them: they would become their advocates and mentors; they would support their candidacy in promotions; defend them when difficulties emerged, and get them perks like the more interesting audits.

Seniors knew that entering into a paternalistic relationship would be of benefit to them and so, if they did want promotion, would endeavour on their own part to cultivate one. They would do so by presenting themselves as affable, reasonable and professional; in a word they would try and charm their way into a favourable position.

Consequently and as a result of the influence of partners and the actions of seniors, those who did get to the top in the promotions race did appear to be those who had considerable social grace. Unsurprisingly, that was some rancour between those who wanted to get promoted about precisely this matter. I have already discussed how this rancour was vented in the storytelling of seniors. Charm however, was not only important for the preservation of harmony in audit teams and for promotion, but was crucially important in the work of partners. For, as I have mentioned, the main function of partners, and hence the purpose of

their role, was to solicit new clients. These solicitations occurred in backstage environments which were most suitable for the exercise of charm: for partners could avail themselves of the informality of these settings to come across as unpretentious, unstuffy, and genuinely concerned in the clients as people. Moreover, they could explain why they were there, and could thus forewarn targets to take the partner 'with a pinch of salt'. In making their intentions clear, partners were not so much blowing their disguise as showing their honesty. And this perhaps was the most affective aspect of their charm: they admitted openly that they were, in effect, salesmen, and yet hoped that in making this known, they would be more likely to achieve a 'sale'.

Conclusion

Quite clearly, what has been discussed are impressions; for it is impossible to measure social graces. Moreover, each and every individual had his or her own way of being charming; some seemed relatively grubby, others positively elegant; but none completely out of place. However, my purpose of this chapter has been to emphasis the presentation of the self so as to explicate how it is that so few of those who enter a chartered accountancy firm make it to partner. The fact that partners engaged in profile work attests to and justifies the suggestion that the ability and willingness to adopt certain behavioural strategies was of crucial importance. The upshot of this is

not that being a successful accountant depends upon the ability to act or 'look the part'. But I think it is sensible to argue that the presentation of self is integral to the business of auditing. Moreover, in that Partners engaged in selling their firm through informal contacts I am not suggesting that they were being cynical and exploitative; far from it. For their commitment to the role of accountant was such that it showed proper respect for that role and hence proper respect to those they dealt with in the business world. In short, being an auditing accountant was a serious business. So serious, in fact, that it had an impact on how the individuals concerned behaved. My analysis of the behavioural requirements placed on trainees and seniors has outlined what the specificities of this behaviour was and has explicated in part, how it was learnt. Reference to the dramaturgical notions of front and backstage and role distance has helped order and give a structure to my analysis of these behaviours. Whether the chapter has succeeded or not in all of this is of course, up to the reader, but whatever the case, I hope that I have managed to underline the importance of the presentation of self in the linkages of financial capitalism.

CHAPTER SEVEN

RECOGNISING PROFESSIONAL BEHAVIOUR AND CONCLUDING REMARKS

One does not need to be well versed in sociology to know that one can consider the issue of professional behaviour in a variety of ways. It has been one of my contentions that the use of different approaches is to be evaluated on grounds internal to that approach and that contrasts and comparison between approaches, although not wholly wasteful, can lead to a multitude of conceptual problems. One reason for this is that different approaches involve doing different things: they implicate specific explanatory relevances and can result, even when the same topic is invoked, in analyses that are fundamentally distinct. One feature of using a dramaturgical approach, for example, is that it requires a focus on face to face interaction. Doing so has several methodological advantages, one being that one may be able to marshall together descriptions from a variety of contexts in ways that has some kind of unity. Other approaches may necessitate a broader panorama of relevances, and these other approaches may appear, at first glance, quite similar to the dramaturgic one. Consider for example what C.Wright Mills wrote nearly forty years ago:

In a society of employees, dominated by the marketing mentality, it is inevitable that a personality market should arise. For, in the great shift from manual skills to the art of 'handling', selling and supervising people, personal or even intimate traits of the employee are drawn in to the sphere of exchange and because of commercial relevance, become commodities in the labour market. Whenever there is a transfer of control over individual's personal traits to another for a price, a sale of those traits which affect

one's impression on others, a personality market arises.
(White Collar:1951:182)

Mills' observations encompassed many different occupations. No doubt *Accountancy* was one of them, for it, like many other professions, clearly places some kind of premium on personality. In exercising his 'sociological imagination', Mills wanted to argue that individuals were increasingly instrumental in the modern epoch and hence, alienated (see also, Rodgers, 1980). Now I do not wish to dispute with his contention. Nor do I think that what I have proposed in the previous chapter contradicts or necessarily confirms what Mills said. But it seems to me that it would be very difficult to utilise the dramaturgic approach and cover the kind of ground Mills does; it would not be sensible to do so. For one thing, it would begin to strain the credibility of the dramaturgic metaphor if one became too general, for another one would begin to ask why one needed it. For Mills was interested in a multitude of social institutions and trends in the forms of social organisation. Dramaturgical analysis in contrast, is interested primarily with how people behave in front of one another. What I am getting at here is that there are more differences to varieties of approaches than at first meets the eye. In this chapter I want to underline this by showing how distinct an ethnomethodological analysis of the professional behaviour I have discussed in Chapter Six would be. I want to show that even with more or less the same materials the remarks generated are strikingly different from those generated

through the use of dramaturgy. I will not argue, however, that either one of these approaches is better than the other or that one is more productive. I think there may well be grounds for claiming that in certain respects and in certain ways one or other of them offers more profitable avenues for research, but that question will have to remain outside the scope of my discussions. In any case, if what I have to say is true then such deliberations about relative merits would be on extremely shaky grounds. For there may be considerable distance between the approaches in question.

Previous Studies of Personality and Accountancy

Given that this is my last chapter and given also my chosen topic, it is perhaps opportune to reflect on the kinds of approaches taken when the relationship between personality and accountancy has been addressed. That there is some kind of relationship, leaving aside how one might address it sociologically, would seem to be almost obvious. For it is not unreasonable to suggest that auditing is one of those services which necessarily involves, in part at least, a dependence on personality. For example, insofar as an auditor has to judge whether a firm's accounts are 'fair and representative', then the auditor needs to be professional¹. In layman's terms professional presumably

¹As it happens, in recent years auditors have become increasingly concerned to ensure that clients can depend upon their professionalism, and one author at least has written a program of 'professional conduct' for auditors (Flint, 1988).

means trustworthiness, integrity, thoroughness. Being professional is especially important in auditing because those who take account of an auditor's report, shareholders, predatory companies, etc., have little other means of gathering information about a firm².

Surprisingly there has, in fact, been very little on this question in the academic literature. One reason for this may be the current fashion for the power paradigm in the sociology of professions. This does not concern itself with personality. But even prior to the ascendancy of this approach, professionalism in accountancy environments has been analysed largely in terms of a *role* (Biddle & Thomas, 1966, Moore 1970) and not in terms of personality. By role is meant the social structural position individuals find themselves in, and the conflicting obligations placed upon them. For example some studies of auditing professionalism have looked at conflict (Merton 1957) as a reflection of gender (Kaufman & Fetter 1980), bureaucracy (Hasting & Hinnings, 1970; Sorenson & Sorenson 1974) & conflicts within an organizational hierarchy (Rosenberg et al 1982).

Another possible reason may be that accountancy is not thought to be an interesting enough area for research. Now this might seem rather a trite observation. But consider,

²These parties have to depend on trust because it is often difficult for any of these parties to assess whether an auditor has done his or her task properly (Moizer, 1986).

the fate of an idealist in a medical school has been studied (Becker & Geer, 1958) but who in their right mind would bother to examine the same in relation to accountancy? Accountancy does not appear to be one of those professions that involves much moral deliberation; it is about numbers and files, not life and death. It may be that this lack of moral significance is one reason why accountants are so often mocked for being uninteresting. At least the medical doctor has ethical choices, but what faustian struggles disturb the sleep of a chartered accountant? It is therefore perhaps not altogether surprising how little has been done on the personality of accountants.

This lack of interest may be beginning to change. If nothing else the increasing numbers of people entering the profession may make it necessary that sociologists start to look at it more comprehensively. Moreover, practitioners themselves may find sociology of interest to them. M. Power is a case in point (Power:1988). Having completed his training, he has written about what he believes were the effects it had on his own personality and those around him. Building on the work of Habermas, he suggests that accountancy, and in particular audit training, makes individuals unimaginative and pedantic. Power comments that his study is limited however, because it does not consider other aspects of the socialisation process. An additional handicap are the difficulties involved with assessing whether someone is pedantic or unimaginative.

In short then, there is little on the relationship between accountancy and personality. My dramaturgical study and this ethnomethodological one may be said therefore to be covering new ground. It will be recalled that one of the claims of ethnomethodology was that it drew attention to areas that had not been analysed before. This claim has considerable justification in relation to the sociology of work. For, in that area of sociology many have argued that work has been described and examined. This has been shown to be spurious by ethnomethodologists who have noted, amongst other things, that work involves considerable tacit skill, skills typically ignored by sociologists. I have looked at some of those skills in chapter five. Here, I will be looking at another set of skills, in this case related to how individuals recognise professional behaviour and learn to adopt it themselves.

My materials for this will be what remained largely unexplicated resources in chapter six. For it seems to me that one can analyse how individuals made sense of the telling offs they got, of the advice they were given and the braggings they listened to. Analysis of how individuals were able to make sense of these will allow one to discover how it was individuals were able to recognise and ultimately adopt professional modes of behaviour. However, these three phenomena should not be thought to incorporate all the processes or devices whereby individuals made sense of professionalism in accountancy environments. But I think it

is reasonable to argue that they are at least central to this process, albeit that they are part of a larger composite of devices. In any case, whatever their relationship with other elements in this composite, analysis of them will provide enough detail for my purpose: to illustrate how distinct an ethnomethodological study is, the considerable difficulties of comparing it with other approaches and thus, through implication and analogy, the problematics of integration and comparison in sociology as a whole.

Telling Offs

I mentioned in chapter six that the work trainees had to do was mundane and boring. Indeed, so much so that it led to disconsolation. It was often repetitive and occasionally without purpose. Trainees would spend many an unhappy hour photostating, trailing pieces of paper around 'accounting systems' - what I said were euphemisms for filing cabinets and worse. Some trainees left after just a few months; but the majority put up with the boredom and continued. Those who stayed engaged in what I called *role distancing activities*. Trainees were told off when caught doing role distance. They would be asked: What kind of attitude is this? what do you think you are doing? Are you going to be serious here or what?

One may think of these telling offs as instructions. Not about the work itself, but about conduct at work. They

related to such things as attitude, demeanour; in a word, self presentation. Furthermore, the general question of presentation was glossed in these instructions as a question of professionalism. Thus trainees were told off for looking 'unprofessional', or 'lacking in professionalism'.

That these telling offs were instructions was taken for granted in chapter six. But here I want to look at how they were interpreted as such by the individuals concerned, (and by the same token by myself as the ethnographer)³. Hearing tellings offs as instructions is in one sense a platitude; of course they were instructions. But hearing telling offs this way requires some kind of sense making skill. For instance, telling offs did not specify every detail of what being professional would entail. Quite often telling offs were simply negative, i.e., they involved saying things like 'that is not what a professional would do'. And these negative specifications were themselves very much context bound. It was only specific events or occasions that prompted them⁴. And yet the individual was expected to use these particular events to identify what professionalism meant in general. To make sense of them, then, as

³Dingwall (1976) also looks at professionalism from an ethnomethodological view. His setting is a Medical establishment.

⁴One might venture to suggest these instructions were "occasioned corpuses" as defined by Zimmerman and Pollner(1971), by which is meant that the parametres of relevant information is delineated by the context in which these corpuses (of knowledge or information) are used.

instructions, the individual had to use certain skills, what I will call common sense skills. In doing so, the individual could use telling offs as a resource, one amongst many, in identifying what was appropriate, professional conduct.

Specifically what I mean by common sense skills is that individuals had to employ their own understandings of 'what was meant', 'what was being got at' or 'what the point was'. An important feature of these understandings were assumptions. For example, individuals assumed that the tellings off were consistent; that they were related and could, hypothetically, (although this was never tested), be justified; that they were not 'merely' capricious. Further, they assumed that there was a 'point', a 'purpose', that there was something 'being got at'. That these assumptions were made meant that a particular telling off could be made sense of by all prior and subsequent telling offs, because each and all were, so to speak, about the same thing. Thus an individual might revise his or her understanding of the real meaning or purpose of a telling off in the light of a future telling off, and the revised meaning could itself be used to make sense of other telling offs. That all this was so, i.e, that telling offs were consistent and that there was a point to them was assumed by individuals to be something that they needed to recognize or else telling offs would not have what was assumed to be their ultimate purpose - to be instructive.

Already, I think it is becoming clear that an ethnomethodological approach emphasises very different considerations than, say, the dramaturgic one. Perhaps one of the most striking characteristics of ethnomethodological inquiries is how they focus on *skills* rather than description. Consider, thus far I have not given much attention to the nature of the work itself, the context so to speak of the telling offs, but to the skills employed in making sense of them. One of my purposes is to make clear that an analysis of these skills cannot be placed side by side a description of, say, the telling offs. As if they were, if you will, pieces of a jigsaw puzzle that only need to be put together to make a whole. My argument is that the focus of ethnomethodological inquiries is such that it would make no sense to place it beside other studies in this way. This is because they are doing different things and what they define as of interest to them is not based on where the parameters of other approaches lay. Instead it is a product of such things as the methodological stipulations that underscore them. These might have been selected for a variety of reasons. In some cases, these stipulations may result in an approach whose boundaries fit snugly with those of another, but this will not always be intended, achieved and should certainly not always be expected. What I think sociologists should concern themselves with is not with how to make approaches fit together, because this often leads to confusion. Instead they should emphasise and recognise the distinctive features of each and every

approach. Here my concern is to underline the distinctiveness of ethnomethodology.

Let me now turn to consideration of reports about trainees and the interviews they went through. In chapter six, my concern was to describe the topics of these interviews and what was included in the reports. One purpose behind doing so was to further explicate what was meant by professionalism; another was to show that learning to become a professional was something the organisation made allowances for in its formal apparatus. My purpose, then, was descriptive.

But let me now consider some events and phenomena ethnomethodologically. To begin with it is clear that one would look at the interviews to see how it was that the two parties, the manager and the trainee, made sense of what happened. One could justifiably suggest that the topic of these interviews was interpretation itself. The manager would explain how he saw an action, the trainee would put forward his or her own account. The trainee was learning about what the manager thought was professional behaviour, the manager about what the trainee thought of learning to adopt a professional manner. Perhaps it is of interest here to note this was not an interaction wherein participant's points of view held equal sway. When it came down to it, what mattered was the manager's opinion. For he or she was the one who decided whether a trainee was going

to get promotion or not. As will be recalled, managers often made this imbalance apparent. For example, if the managers interpreted a trainee's behaviour as, say, 'willfully unprofessional', then the manager would threaten the trainee with failure in promotions or even, as it was innocuously called, 'termination of services' (the sack). Sometimes then, interviews involved an ultimatum - change or leave.

That this was so meant that using telling offs and the interviews as resources or instructions in the quest to become 'professional' was enforced by potentially social and economic sanctions. Trainees could not continue behaving like bored school kids and continue to be 'told off'. They had to take heed 'or else'. To this extent they are similarities between the 'convict code', as described by D L Wieder, (1974 (a);1974 (b)) and the way in which professionalism was talked about in the accounting environment. I discussed the convict code in chapter three. I noted that the code was an interpretive device, one that was enforced by a variety of moral sanctions. It enabled individuals in a half way house to recognise behaviour and to know what kind of behaviour would be appropriate for them. The primary difference between the two locales was the degree and type of sanction that backed the need to be professional and the need to abide by the convict code: failure to be professional could mean dismissal, failure to abide by the code could lead to a beating or even death.

Advice

The interviews trainees had were quite often the only time trainees spent with managers. Seniors, in contrast, were better known by their superiors and spent much more time with them. One consequence of this was that seniors were given more advice of a positive kind, and not so many of the negative rebukes so characteristic of the relationship between trainees and their superiors.

This advice related to a whole variety of topics. But in relation to conduct it was often quite specific. The reader will remember, for example, the importance of the clothes seniors wore. They could not wear any suit. A suit had to be discreet without being bland, well-cut without being extravagant. There were also matters of grooming; even the vehicles used to get to and from work were a matter of concern. Seniors would be given the advice that motorcycles were as unsuitable as decrepit old cars. It would be pointed out that motorcycles might give the impression of 'laddishness' and therefore possible inability to make calm and objective assessments; whilst a clapped-out car might suggest an auditor was incapable of running his own affairs and therefore was unlikely to be able to assess those of another.

Although the advice then was often quite specific, interpreting it, making sense of it, required a certain amount of skill. For though a particular suit may have been

criticised, what kind of suits were being alluded to as 'appropriate' in contrast was not always made clear. The senior would have to judge what was correct. In other words, those who were given the advice would have to 'fill in the details' about what was meant. It was as if there was an *etcetera* clause included in comments about such things as personal presentation.

Up to now, I have not discussed motives. Of course, when trainees were told off, or during interviews, motives were in part an element of what was implicated. But, by the time individuals had become seniors, their motives were very much a matter for concern and deliberation. Advice was one resource used to interpret and explain behaviour in terms of *motivation*. Apart from anything else, reference to motivation helped individuals interpret and explain success or failure in the promotions race. Although failure to get promotion, even dismissal, was in principle a possibility from the moment individuals arrived at the firm, in practice most knew that they would get through the system to at least senior level. But, only a handful out of every two or three dozen were promoted beyond that stage; the rest were encouraged to leave or chose to do so willingly. Promotion beyond senior level was extremely competitive and therefore a topic of considerable concern.

It is uncontroversial to argue therefore that seniors assumed that advice was about criteria for promotion. One obvious

reason for this was that this was how most of the advice was presented. But given the fact that only a small percentage of seniors would actually achieve promotion, then the degree to which advice was abided by, perhaps even exemplified and exaggerated, could also be used as an indication of a senior's keenness and suitability for promotion. For someone who was, say, really keen, would 'make a point' of conspicuously abiding by some advice, others who were less keen, would be less conspicuous in their obedience. Moreover, reference to motives could be used by individuals to assess a whole variety of behaviour. Wearing a smart suit or driving a suitable car may not necessarily be indicative of a keenness to be promoted. It may after all be coincidental. But by interpreting them as if they were an indication of a desire to gain promotion such behaviours came to be defineable insofar as they were linked by a common motivation.

Advice was not the only medium whereby individuals made motivations an accountable matter in the promotions race. I have drawn attention to the stories seniors told one another. These stories, often anecdotal, sometimes apocryhal, were told, developed and shared by seniors in their efforts to make sense not just of people's motives but of the promotions race in general. Thus stories were told confirming that 'conspicuous' motivation resulted in promotion, yet others would say that someone was promoted 'simply because he was a smart dresser', 'a smart talker',

or lowest of the low 'a crawler'. To the extent that the stories seniors told were informative devices whereby the culture of the auditing environment was defined, recognized and oriented to, seniors were not so much converts to the 'system' (in the sense that they were subject to say, ideological indoctrination) as they were exponents of it, exponents in that they were partly engaged in making the environment come to be the way it was.

In short then, the degree of someone's motivation was treated as a matter of consequence. The greater the motivation the greater the concern to be promoted and hence, possibly, the more likelihood of success in promotion. One consequence of telling stories about motivations, and in the ways these motivations were recognized, albeit in a circular, mutually self elaborating ways - the behaviour informing the motive, the motive categorizing the behaviour - was that the idea that a certain type of personality, specifically a professional personality, was of importance in the accountancy environment come to be an important and accountable possibility.

Tellings

In addition to these sense making activities in regard to themselves, seniors also learnt about how to interpret and assess the behaviour of the audit teams. In particular, seniors used a schema of interpretation with which to indicate how clients would view an audit team. Thereby

seniors were able to discern how an audit team should present itself so that it was interpreted or seen by the clients in ways that seniors believed desirable. These ways were another element whereby the linkage between work and personality were made a manifest, accountable, feature of the auditing environment.

In the trainee years, individuals had little consequential contact with senior personnel of the client, and dealt mainly with lower ranking individuals like clerks. Once they had become seniors (and in all levels thereafter) individuals could liaise with the high ranking members of a client firm. During these liaisons or meetings, which occurred prior, during and after an audit, seniors were instructed by the clients about what kind of an audit they considered appropriate. Appropriateness often related to matters of conduct. These instructions were not telling offs (although they were often related to some specific occurrence) and most typically were 'off the cuff' descriptions, sometimes quite long winded. They often began with the phrase 'I'll tell you what I want'. (It is this form that has led me to call them tellings).

Tellings would have the following character. The client pointed out that he would not accept an audit team that caused 'too much interference' with the client's own staff. Interference meant many things and was often elaborated in the following ways. Clients would say that they did not want auditors who were under-supervised, did not know what

they were doing, lackadaisical, or who larked about. Clients suggested in these tellings that if the auditors did not work in appropriate ways then the contract would be lost and other auditors engaged, or that the fees would have to be renegotiated and so on. The tellings were sometimes stated in the most forthright terms: 'I don't want your team to fuck my staff around', 'I don't want any crap, I want the work done and then I want you out'⁵.

These tellings were, in some respects, like the telling offs that juniors received in that threats were sometimes incorporated. This aside, tellings were prescriptions of an extremely general nature; and like telling offs, could only be instructive if the listener used his own sense making skills to interpret them. To do this involved the senior assuming that the tellings were referring to what was typical, what was routinely expected. That is to say the senior did not assume that these tellings were, for instance, 'wind ups', or that the client had a unique, perhaps bizarre view; the senior assumed that what the client expected was what any client would expect; that the audit should be just like any other audit, and that there was no reason to expect it to be otherwise. (If something out of the ordinary did happen of course, the tellings would

⁵It has to be noted that the client who used these particular terms was renowned for his abrasive language. However he did not just use it in the presence of the auditors, but quite generally. It was apparently part of his 'hard hitting' image.

be seen in a different light: that they were no longer relevant etc). Nor would seniors reject or contradict these tellings by suggesting, perhaps, that it was understandable if trainees seemed not to take their work seriously, after all their work could be done perfectly adequately even if there was bit of larking around. Nor would they say that larking around might make the work more enjoyable.

Tellings provided an interpretive schema whereby seniors could define how audits should be done and provided a 'why' or a rationale for this. One feature made visible through this interpretive schema was that the the conduct of auditors was important for clients. I have just discussed telling offs and report cards, it was seniors who did much of the tellings off and who also wrote the report cards. The interpretive scheme that derived from the tellings then, provided seniors with a rationale for telling offs and the comments on the report cards.

Braggings

I mentioned earlier that new arrivals had accepted a kind of contract or bargain with the firm. This contract meant that they would do boring work for low pay in return for support and training for the chartered accountancy qualification. Once those qualifications had been attained and the two years as senior (implicitly) required by the terms of the bargain completed however, one might think that a further bargain needed to be struck to encourage an individual, now

a chartered accountant, to go further up the career ladder of the firm. As it happens no further bargain was struck; rather individuals were enticed by the benefits that would accrue with promotion. They found out what these were through the 'braggings' of those who had progressed all the way through the hierarchy. In other words, what tempted them was not first hand experience of what was available with promotion but rather what they were told and assumed these benefits to be. Although they arrived at the firm with vague notions of what these benefits were, during their training they were regularly presented with descriptions and accounts of the benefits of being promoted all the way to the top. These were given by partners themselves, the recipients of the benefits.

These braggings were not simply casual, they were incorporated in the formal apparatus of the firm's training system. So, for example, partners would talk to newly commenced trainees in allocated lecture times about all the benefits that went with being partner. Such formal presentations continued to occur at regular intervals throughout the trainees period at the firm. During these presentation, partners spoke about such things as their salary - often ten times that of the incoming trainee, the cars they drove, the size of their houses; all this they would say, was available for those willing and capable.

In addition to pointing out their material wealth, partners also discussed the type of work they did in comparison to

the work of, say, seniors. This was extremely important, for in defining the kind of work they did they also explained how it was that only certain types of person were suited for it. Partners would explain that their work involved little actual accounting or auditing. In fact, they would remark wryly, they 'cost the firm money'. In contrast, seniors generated income and more than pay for themselves since almost their entire time was given to auditing work. The function, or task of partners was to do 'profile work'.

I discussed the nature and function of profile work at some length in the previous chapter. It will be remembered that profile work meant developing and maintaining contacts with clients and potential clients. These contacts were intended to result in present clients' continuing their business with the firm and for new work to be generated via new contacts. In effect the partners' work involved public relations, or as one put it, 'salemanship for the firm'.

To succeed in being a salesperson for the firm Partners would explain that they had to personify the things that attracted a target. Partners admitted that in some cases, simply being known to a potential client made the difference between being selected for audit work and not. But simply being known, being friendly and affable was not all. In addition partners said they had to show what was distinct and professional about their firm. They had to show to clients how it was that their auditing firm was the best,

the most suitable for the task. To do so depended upon the way the partners presented themselves. Thus how trainees and seniors presented themselves was a topic of concern for the firm because only those who could fulfil the function of partner would be promoted. And those who could fulfill the function of partner would have to present themselves in a certain way. If the correct people were not selected the firm would fail to prosper.

At least this is what partners themselves *said*. Those who listened to these braggings had no way of assessing whether clients or targets got these desired impressions. After all clients were unlikely to be easily duped. They were as capable of recognizing a salesman as anyone else. So, those who listened to the braggings, the trainees, were not entirely credulous but then, on the other hand, they had no reason to doubt the general emphasis of what was being said. There was no reason for partners to lie. Moreover, there was no reason for them to believe that behaving a certain way was not important. Auditing is a serious business. It is perfectly understandable therefore that auditors take themselves seriously and that clients also do so.

In listening however, trainees were not considering whether it was true or not, they interpreted what was said as an indication of what the auditing firm itself believed was important. Thus they listened to hear about how they could be promoted, how they could succeed. Central amongst the things that was being shown to be important in the braggings

was the relationship between persona, albeit work persona, and status in the firm. For with higher status, persona become of increasing, almost paramount importance. In other words, the braggings signalled that the linkage between personality and work role was a topic, an accountable matter in the way auditors saw and evaluated themselves.

Taken as a whole, braggings were treated as supplements, further indications and reasons, in the overall process of making sense of events and actions within the audit environment. Telling offs and advice were also resources in these sense making activities. In that each of these processes was used to make sense of the others, these devices were mutually elaborative.

Conclusion

Often in sociological discussion reference to assumptions, specifically assumptions about meanings, are referred to in terms like 'ideological views' or 'paradigms'. Ethnomethodologists, drawing on the work of A. Schutz, take a somewhat different view of assumptions about meaning. They argue that the beliefs or understandings individuals draw upon to 'make sense' of each others actions may be called 'common sense knowledge of social structures'. This is because these assumption are held in common and because they relate to the predictable, consistent, typical features of social behaviour, that is to say, to its structure. Common sense knowledge, according to this view, does not

consist of a corpus of, say, ideological beliefs, rather it consists of ways of doing things, in particular, *methods* of interpretation. Over twenty years ago Garfinkel suggested, drawing on Mannheim, that the process of using 'common sense knowledge' be called a method, specifically, the 'documentary method of interpretation' (1968:76-104). As he puts it:

The method consists of treating an actual appearance as a 'document of', as 'pointing toward', 'standing on behalf of', a presumed underlying pattern. Not only is this underlying pattern derived from its individual documentary evidences, but the individual documentary evidences in their turn, are interpreted on the basis of what is known about the underlying pattern. Each is used to elaborate the other. (Garfinkel, 1967:78)

One implication of this is that from the ethnomethodological point of view, 'culture' is a set of *skills*, in particular, a set of sense making skills. This has consequences for the topics of ethnomethodological inquiry. For ethnomethodologists would not attempt to describe a 'culture' since from their perspective it is not a thing or a catalogue, but a skill.

In this chapter I have been looking at three different activities wherein sense making skills were employed. Trainees had to make sense of being told off, seniors the advice they were given by their superiors and what they were told by clients. The braggings of partners were interpreted so that they provided reasons to stay on in the firm and mechanisms to succeed in this. In effect, all these sense

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making activities were about how to recognise what behaving professionally consisted of. In that each and all were indications of an underlying notion of professional behaviour, then my analysis can be said to be a description of the documentary method of interpretation.

This being the case then this chapter is fundamentally concerned with matters implicated by the ethnomethodological program of inquiries. These considerations are quite distinct from dramaturgical ones. As I have said, my purpose has not been to claim that one or other of these approaches is better but to underline and draw out this distinctiveness.

Concluding Remarks

In the introduction to this thesis I noted that the word 'professional' would be used in chapters five and six in distinct ways. In one, in relation to the common sense competence that is a requisite of professionalism, in the other, as a word that labels certain types of behaviour. Here I have pursued the same approach as in chapter five. There my concerns were with how individuals made sense of the numbers they dealt with; in this chapter with how they made sense of each other's conduct. It seems to me reasonable to suggest that the word professional was used in the same kind of analytic game in each. This game focuses on certain ways of considering social action. The dramaturgical approach, on the other hand, is altogether

another game, one that involves using the word professional as a part of a different program of inquiries. This being so, it is my contention that the ethnomethodological and dramaturgic approaches be treated as different language games. Though the same words may be used, their function, role and meaning maybe and often is, specific to each. Therefore it would make little sense to suggest that my ethnographic observations are all about the same professionalism; observations which may for example, be integrated together to come up with a definitive description of professional accountancy. Such an endeavour, I have argued, would be conceptually confused.

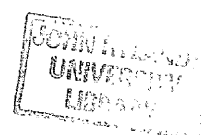
My discussion of Wittgenstein's later philosophy has allowed me to define what conceptual confusions are. I have noted how these confusions are endemic to thinking and that there is therefore a constant need to remedy them. The idea that different approaches be treated as language games has been put forward to minimise the occurrence of more confusions. Amongst other things, doing so may help avoid the kind of unjustified theoretical deliberations I discussed in chapter four.

If however, Wittgenstein's philosophy can be used as a basis for these suggestions, it does not also specify which sociological approach is most appropriate in any situation. Though his philosophy has made us realise that one consideration will be whether an approach takes more account of actor's understandings than another, there are other

considerations. These I have argued are sociological. It has been my purpose in Chapters Three, Five and Seven, to specify the distinctly sociological concerns of ethnomethodology. I have contrasted these with the equally sociological concerns of dramaturgy. One feature of choosing an approach on sociological grounds is that reference has to be made to how that approach has been used in the past. It may also be relevant to review other approaches to see what they have not analysed. The end result of choosing an approach will not be a more thorough description of social action, but an increase in the diversity of ways in which we comprehend it.

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